This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

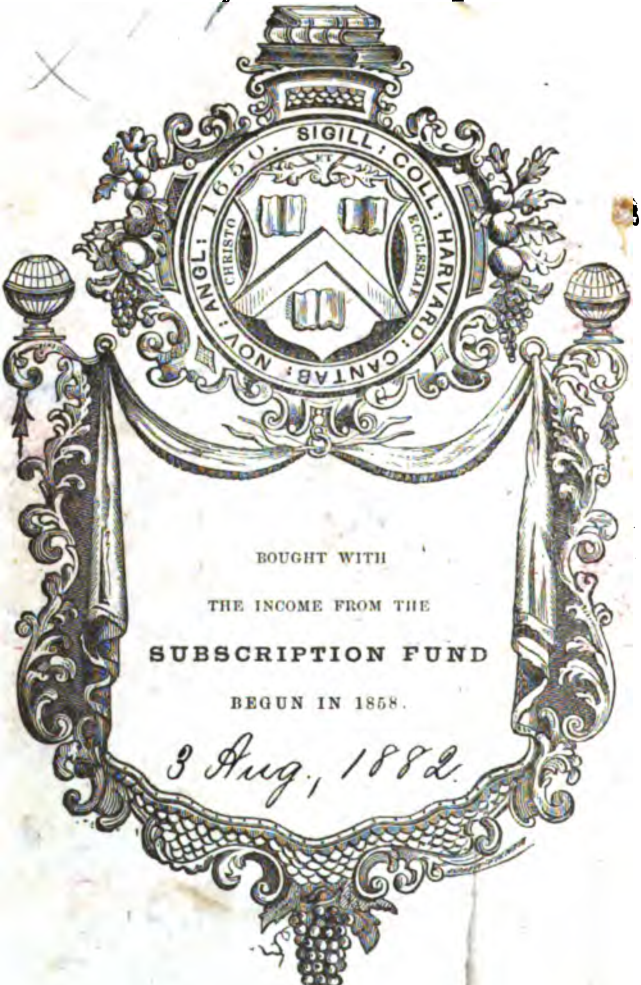
+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

Med 1058.59


SIGILL: COLL: HARVARD: CANTAB: NOV: ANGL: 1650.
CHRISTO ET ECCLESIAE

BOUGHT WITH
THE INCOME FROM THE
SUBSCRIPTION FUND
BEGUN IN 1858.
3 Aug, 1882

THE

PHYSIOLOGY OF COMMON LIFE

THE

PHYSIOLOGY OF COMMON LIFE

BY

GEORGE HENRY LEWES

Author of "Seaside Studies," "Life of Goethe," &c.

IN TWO VOLUMES

VOL. I.

WILLIAM BLACKWOOD AND SONS
EDINBURGH AND LONDON
MDCCCLIX

Med 1058.59
~~V.1239~~

1882, Aug. 3.
Subscription fund.

PRINTED BY WILLIAM BLACKWOOD AND SONS, EDINBURGH.

CONTENTS OF THE FIRST VOLUME.

CHAPTER I.

HUNGER AND THIRST.

Page

Incentives to action—Cause of hunger: waste and repair of the body—Periodicity of hunger—Comparison of the organism with a steam-engine inaccurate—The blood of starving men—Starvation—Cases of prolonged fasting in animals—Fabulous stories of long fasting in men and women—Aspects of starvation—The parts of the body which first disappear in starvation—Sleeplessness of starving men—Agonies of starvation—Pathetic stories—The sensation of hunger, and its cause—Hunger as a general, and as a local, sensation—Thirst as a sensation—Cause of thirst—Necessity of water in the organism, and consequences of deficiency—Thirst as a mode of torture—Story of the Black Hole of Calcutta—How to quench thirst—The drink of animals—Effects of thirst, ... 1

CHAPTER II.

FOOD AND DRINK.

SECTION I.—ON THE NATURE OF FOOD.

Varieties of food—The eaters of clay—The chemical and physiological methods of investigation—"One man's meat another man's poison," illustrated—A caution to parents—Wholesome and unwholesome food—Relation of food to the organism—Vital process incapable of chemical explanation—Difference between chemistry and physiology—A canon of philosophy—Is the family a type of the State?—Chemical problems are quantitative; physiological problems qualitative—Liebig's classification

Page

of food into plastic and respiratory not acceptable — Liebig's error arises from his taking the chemical point of view—Facts against him—Experiments on animals — The rice-eating Hindoos — the food in cold countries—Results of inquiry—Inorganic food—Nutrition in plants and animals misunderstood—Phosphate of lime in the body; amount of water in the body; amount of salt—Uses of salt—The law of endosmosis. 51

CHAPTER II.

(*Continued.*)

FOOD AND DRINK.

SECTION II.—ALIMENTARY PRINCIPLES.

Effect of different conditions in substances — Life interferes to disturb ordinary chemical actions—Effect of Form—Food considered chemically and physiologically—Blood as a standard of food—Milk as a standard—Albumen—Fibrine — Casein—The protein bodies—Gluten—Gelatine, is it nutritious ?—History of the inquiry—Fats and oils ; their digestibility in various conditions—Starch as food—Sugar—Does sugar injure the teeth ?—Alcohol as food—Teetotalism—Iron as food—Acids, do they prevent scurvy ?—Does vinegar keep people thin ? 109

CHAPTER II.

(*Continued.*)

FOOD AND DRINK.

SECTION III.—ARTICLES OF FOOD.

Meats—The flavouring substance of meat, *osmazome*—The flesh of young animals—The age of animals in relation to the digestibility of their flesh—Effects of roasting, boiling, and baking—German and French meat—Digestibility of different meats—Sweetbread and tripe—Horse-flesh as food : experiments to establish the excellence of horse-beef—Quantities of horse-flesh consumed in various countries—The flesh of donkeys—Fish : digestibility of different kinds—Nutritive quality of fish—Are fish-eaters unusually prolific ?—Eggs—Pastry : is it injurious ?—Vegetables—Vegetarianism—Tea, coffee, and the narcotics : curious physiological paradox of their action—The quantity of food requisite for man—Individual varieties in the quantity needed—Error of applying arithmetic to vital problems—The gluttony of some races—Does a cold climate produce inordinate eating ?—The rations of paupers and soldiers 151

CHAPTER III.

DIGESTION AND INDIGESTION.

Page

Happiness and misery connected with the Digestion—In what is science distinguished from ordinary knowledge?—The mechanical and chemical processes of Digestion: the former only exists in the simplest animals—What takes place in the mouth—Chemical action of the saliva—Its mechanical action—"Bolting" the food—Mechanical action of the stomach—The gastric juice: its composition and origin—Pepsin and acid—Action of gastric juice on various substances—It arrests putrefaction—Lesson against over-eating—Influence of fats in Digestion—Influence of saliva in increasing the gastric juice—Quantity of gastric juice—Chymification equivalent to boiling—Why the gastric juice does not act upon the stomach itself—Intestinal Digestion—The bile: quantity and composition of; how affected by fats—What is meant by biliousness?—The part played by bile—It neutralises the action of gastric juice, and assists in the absorption of fat—The pancreatic juice acts on all substances—The intestinal juice completes Digestion—Nature of chyle—The various Causes of Indigestion, ... 190

CHAPTER IV.

THE STRUCTURE AND USES OF OUR BLOOD.

Blood the river of life—Erroneous notions about medicines "purifying" the blood—How it circulates—Capillary vessels—The substances it carries: iron and soap—The blood-discs: their discovery by Malpighi and Leeuwenhoek—Hewson's researches—Size of the discs in various animals—The nucleus—The colourless corpuscles: analogy between them and the Amœbæ—What is an Amœba?—The parasite which lives in our blood—Development of blood-cells—Is the blood alive?—Coagulation and its cause—Chemical composition of the blood—Variations of, in different people—Differences between arterial and venous blood—The gases they contain—Cause of the change of colour—The marvel of the circulation—Quantity of our blood—Blood-letting—Transfusion: history of the attempts; when it may be successfully employed—Blood does *not* form the organs—Vivifying elements of blood: arterial nourishes, and venous excites the tissues—Oxygen as a condition of nutrition—How does the blood nourish?—Each organ determining the nutrition of the rest—Relation between circulation and assimilation, . . 239

CHAPTER V.

CIRCULATION OF THE BLOOD: ITS HISTORY, COURSE, AND CAUSES.

Page

Was Harvey the discoverer of the circulation?—Course taken by the blood—History of seventeen centuries—Three errors removed by Galen, Vesalius, and Colombo—What then remained for Harvey?—Discovery of the valves—Exaggeration of their importance: veins without valves—Discovery of the fact of the circulation—Reception of the discovery by contemporaries—Deficiencies in Harvey's doctrine—Discovery of the capillary vessels: their structure and uses—Discovery of the lymphatics—Harvey's opposition to it—Statement of Harvey's real claims—The cause of the circulation—Influence of the heart; not the sole cause—Rapidity of circulation—Circulation in absence of the heart—Motions of the heart; cause thereof; nerves and ganglia of the heart—The heart continuing to beat after death—Action of the Arteries—Circulation in the capillaries—Draper's hypothesis—Spallanzani's observations—Circulation and Respiration, ... 291

CHAPTER VI.

RESPIRATION AND SUFFOCATION.

Two suicides—Suffocation of seventy-two persons on board the "Londonderry"—History of our knowledge of respiration—The air we breathe—Distinction between respiration as an Animal Function and as a Property of Tissue—Oxygen the life-giver—Varnished eggs will not develop; tail of a tadpole developing after separation from the body—The breathing mechanism in various animals—The process of breathing—Tight-lacing—Alterations of the air in respiration—Necessity of ventilation—German taverns—How the organism accustoms itself to bad air—Effect of bad air in depressing the vital functions—Respiration a vital, not a physical, problem—Suffocation: various forms of—Carbonic acid not a poison—Oxide of carbon a poison: its effects on the blood—Deaths by suffocation: escape of gas: drowning—Respiration not a process of oxidation—The balance of nature—Trees not beneficial in streets—Varieties of animal respiration—Respiration during sleep—Effect of temperature on respiration—Why do we breathe? 347

CHAPTER VII.

WHY WE ARE WARM, AND HOW WE KEEP SO.

Page

Animals have a temperature of their own—This not owing to a "Vital Principle"—Description of the various instruments employed in measuring animal heat—Mistake of supposing that there are cold-blooded and warm-blooded animals—Man preserves his temperature in all climates and all seasons—His power of enduring great heat—The cooling apparatus of his organism—Perspiration carries off superfluous heat—Difference between feeling hot and being hot—Influence of age, sex, and food on our temperature—Young animals cannot well resist cold—The notion of "hardening" infants erroneous—Is food warmth?—Influence of the seasons—Effect of a cold wind—Why is the east wind felt to be cold?—Where is cold dangerous?—Can cold water be drunk with safety when we are hot?—The theory of animal heat—Respiration is not the cause of animal heat—Heat in dead bodies—Relation between respiration and animal heat in various classes of animals—Hybernating animals—Frogs and tritons in winter and summer, ... 405

S 2-1

PREFACE

The object of the following Work differs from that of all other works on popular science in its attempt to meet the wants of the Student, while meeting those of the general reader, who is supposed to be wholly unacquainted with anatomy and physiology. The many excellent Treatises which exist are only suited to the advanced student; they assume a knowledge, and a facility of apprehension, which can only issue from a practical familiarity with the subjects.

The remembrance of my own wants and difficulties led me, many years ago, to the belief that the work which was really the most suitable for general culture, would also be of material assistance to the Student. Accordingly, although there are many topics, interesting and important to the physiologist, which find no place in this work, because they are without the range of general culture, I have done my best to render the exposition, of all the topics selected, on a level with the science of our day.

In pursuance of this object I have been forced to depart very widely from the practice of other popular writers, who consider themselves bound to act as "middle-men" between scientific authorities and the public, and to expound facts and doctrines as they find them. I could not adopt this easy and convenient plan. I could not bring myself to publish, on the authority of respected names, statements which I knew to be false, and opinions which I believed to be erroneous. After having laboured earnestly to get at the truth, it would have been disloyal to contribute in any way to the spread of what I believed to be error. All that I felt bound to do, was to state impartially the facts and opinions current among physiologists; and, when those opinions seemed inadmissible, to state the reasons for their rejection. There is therefore a great deal of criticism, and much original matter in this work.

It is in the chapters on the Nervous System that the greatest amount of dissent from current opinions will be found; and it is there that the reader will most probably feel the greatest difficulty in agreeing with me, especially if he be versed in the doctrines of the schools, and not very familiar with the subjects through direct observation and experiment. I can only say that the views there put forth are the result of very considerable research, embracing molluscs, bees, beetles, spiders, locusts, crabs, fishes, frogs, tritons, lizards, chickens, moles, mice, rats, cats, dogs, sheep, pigs, calves, oxen, and men.

The literature of each subject has been given with as much fulness as seemed useful; and never at secondhand, unless specially acknowledged. The deficiencies in this respect will be found greatest in the English department; the reason of which is that my purse is not long enough to command the Transactions, Journals, and Reviews in which my countrymen have recorded their labours; and I have not the advantage of commanding a public library. It has been my desire to render every one his due; but of course a great many claims have been passed over in ignorance.

March 1860.

THE
PHYSIOLOGY OF COMMON LIFE.

CHAPTER I.

HUNGER AND THIRST.

Incentives to action—Cause of hunger: waste and repair of the body—Periodicity of hunger—Comparison of the organism with a steam-engine inaccurate—The blood of starving men—Starvation—Cases of prolonged fasting in animals—Fabulous stories of long fasting in men and women—Aspects of starvation—The parts of the body which first disappear in starvation—Sleeplessness of starving men—Agonies of starvation—Pathetic stories—The sensation of hunger, and its cause—Hunger as a general, and as a local, sensation—Thirst as a sensation—Cause of thirst—Necessity of water in the organism, and consequences of deficiency—Thirst as a mode of torture—Story of the Black Hole of Calcutta—How to quench thirst—The drink of animals—Effects of thirst.

HUNGER is one of the beneficent and terrible instincts. It is, indeed, the very fire of life, underlying all impulses to labour, and moving man to noble activities by its imperious demands. Look where we may, we see it as the motive power which sets the vast array of human machinery in action.

It is Hunger which brings these stalwart navvies together in orderly gangs to cut paths through mountains, to throw bridges across rivers, to intersect the land with the great iron ways which bring city into daily communication with city. Hunger is the invisible overseer of the men who are erecting palaces, prison-houses, barracks, and villas. Hunger sits at the loom, which with stealthy power is weaving the wondrous fabrics of cotton and silk. Hunger labours at the furnace and the plough, coercing the native indolence of man into strenuous and incessant activity. Let food be abundant and easy of access, and civilisation becomes impossible: so indissolubly dependent are our higher efforts on our lower impulses. Nothing but the necessities of food will force man to that labour which he hates, and will always avoid when he can. And although this seems obvious only when applied to the labouring classes, it is equally though less obviously true when applied to all other classes, for the money we all labour to gain is nothing but food, and the surplus of food, which will buy other men's labour.

Hunger, although beneficent, is no less terrible. When its progress is unchecked, it becomes a devouring flame, destroying all that is most noble in man. Hunger is a stimulus to crime, no less than to honest labour. It wanders through dark alleys, whispering desperate thoughts into eager ears; and it maddens the shipwrecked crew till they cast away all shame, all pity, all desire of respect, and perpetrate deeds which cannot be mentioned without horror. Hunger subjugates the humanity in man, and makes the brute predominate. Impelled by this ferocious instinct, men have eaten their companions, and women have even eaten their own children. Hunger has thus a twofold character: beside the picture of the activities it inspires, we must also contemplate the picture of the ferocities it evokes.

What is this Hunger—what its causes and effects?

In one sense we may all be said to know what Hunger is; in another sense no man can enlighten us; we have all felt it, but Science as yet has been unable to furnish any sufficient explanation of it. Between the gentle and agreeable stimulus known as Appetite, and the agony of Starvation, there are infinite gradations. The early stages are familiar even to the wealthy; but only the very poor, or those who have undergone exceptional calamities, such as shipwreck and the like, know anything of the later stages. We all know what it is to be hungry, even *very* hungry; but the terrible approaches of protracted hunger are exceptional experiences. From materials furnished by sad experiences, both familiar and exceptional, I will endeavour to state the capital phenomena and their causes.

And first we must explain what is meant by a *tissue*, as the word will be of constant recurrence in these pages. Previous to the time of Bichât, who may be called the founder of philosophical anatomy, the body was considered as made up of various parts or organs; when these parts had been enumerated, the task of description was over. Bichât flashed light upon the science when he showed that the organs themselves were made up of various tissues, or elementary structures, each of which preserved its characteristic properties in whatever part of the body it might be found. Thus the heart, for instance, is an organ constructed out of muscular tissue, connective tissue, nervous tissue, and adipose tissue—each of these tissues manifesting the same properties in the heart which it manifests in every other organ; just as the various substances out of which a ship is constructed—wood, hemp, copper, iron, tar, &c.—preserve their characteristic properties, though the wood may be rudder, deck, or mast, and the iron anchor, nail, or cable.

The tissues are the elementary portions of the animal fabric; and a distinct branch of the science is devoted to their study under the name of *Histology*, also called *General* Anatomy. The organism is composed of organs, and the organs of tissues.

I. THE CAUSE OF HUNGER.—In every living organism there is an incessant and reciprocal activity of *waste* and *repair*. The living fabric, in the very actions which constitute its life, is momently yielding up its particles to destruction, like the coal which is burned in the furnace: so much coal to so much heat, so much waste of tissue to so much vital activity. You cannot wink your eye, move your finger, or think a thought, but some minute particle of your substance must be sacrificed in doing so. Unless the coal which is burning be from time to time replaced, the fire soon smoulders, and finally goes out; unless the substance of your body, which is wasting, be from time to time furnished with fresh food, Life flickers, and at length becomes extinct.

Hunger is the instinct which teaches us to replenish the empty furnace.

But although the want of food, necessary to repair the waste of life, is the primary cause of Hunger, it does not, as is often erroneously stated, in itself constitute Hunger. The absence of necessary food causes the sensation, but it is not itself the sensation. Food may be absent without any sensation, such as we express by the word Hunger, being felt: insane people frequently subject themselves to prolonged abstinence from food, without any hungry cravings; and, in a lesser degree, it is familiar to us all how any violent emotion of grief or joy will completely destroy, not only the sense of Hunger, but our possibility of even swallowing the food which an hour before was cravingly desired. Further, it is known that the feeling of Hunger may be allayed by opium, tobacco, or even by inorganic substances introduced into the stomach, although none of these can supply the deficiency of food. Want of food is therefore the *primary*, but not the *proximate*, cause of Hunger. I am using the word Hunger in its popular sense here, as indicating that specific sensation which impels us to eat; when the subject has been more fully unfolded, the reader will see how far this popular sense of the word is applicable to all the phenomena.

We can now understand why Hunger should recur periodically, and with a frequency in proportion to the demands of nutrition. Young animals demand food more frequently than the adult; birds and mammalia more frequently than reptiles and fishes. A lethargic boa-constrictor will only feed about once a-month, a lively rabbit twenty times a-day. Temperature has also its influence on the frequency of the recurrence: cold excites the appetite of warm-blooded animals, but diminishes that of the cold-blooded, the majority of which cease to take any food at the temperature of freezing. Those warm-blooded animals which present the curious phenomenon of "winter sleep," resemble the cold-blooded animals in this respect; during hybernation they need no food, because almost all the vital actions are suspended. It is found that, at the temperature of freezing, even digestion is suspended. Hunter fed lizards at the commencement of winter, and from time to time opened them, without perceiving any indications of digestion having gone on; and when spring returned, those lizards which were still living, vomited the food which they had retained undigested in their stomachs during the whole winter.*

Besides the usual conditions of recurring appetite, there are some unusual conditions, depending on peculiarities in the individual, or on certain states of the organism. Thus during convalescence after some maladies, especially fevers, the appetite is almost incessant; and Admiral Byron relates that, after suffering from a month's starvation during a shipwreck, he and his companions, when on shore, were not content with gorging themselves while at table, but filled their pockets that they might eat during the intervals of meals. In certain diseases there is a craving for food which no supplies allay; but of this we need not speak here.

---

* HUNTER: *Observations on Certain Parts of the Animal Economy.*

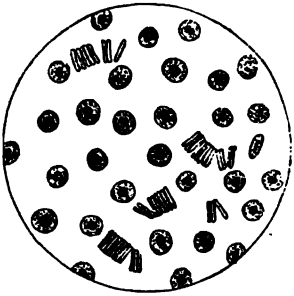
The animal body is often compared with a steam-engine, of which the *food* is the *fuel* in the furnace, furnishing the motor-power. As an illustration, this may be acceptable enough, but, like many other illustrations, it is often accepted for a real analogy, a true expression of the facts. As an analogy, its failure is conspicuous. No engine burns its *own substance* as fuel: its motor-power is all derived from the coke which is burning in the furnace, and is in direct proportion to the amount of coke consumed; when the coke is exhausted, the engine stops. But every organism consumes its own body: it does not burn food, but tissue. The fervid wheels of Life were made out of food, and in their action motor-power is evolved.

The difference between the organism and the mechanism is this: the production of heat in the organism is not the *cause* of its activity, but the *result* of it; whereas in the mechanism, the activity originates in, and is sustained by, the heat. Remove the coals which generate the steam, and you immediately arrest the action of the mechanism; but long after all the food has disappeared, and become transformed into the solids and liquids of the living fabric, the organism continues to manifest all the powers which it manifested before.

There is of course a limit to this continuance, inasmuch as vital activity is dependent on the destruction of tissue. The man who takes no food, lives, like a spendthrift, on his capital, and cannot survive his capital. He is observed to get thin, pale, and feeble, because he is spending without replenishing his coffers; he is gradually *impoverishing* himself, because Life is waste.

II. THE EFFECTS OF HUNGER.—In a future chapter we shall inquire minutely into the structure and composition of the Blood; for the present, it must suffice to say that the Blood carries in its current certain bodies called blood-discs, which perform the chief part in nutrition. They are of two kinds, the red and the colourless. Here is the figure of the red discs, as seen under the microscope.

Fig. 1.



BLOOD-DISCS.

If we examine the blood of a starving man, we shall find its elementary composition to be precisely similar to that of the same man in his healthy state, but the *proportions* of that composition will be greatly altered; the discs—which have been denominated the nutritive solids of the blood—are much diminished in quantity, and all its inorganic constituents, which are the products of destroyed tissues, are much increased. In fact, these inorganic products, like the pawn-tickets found in the spendthrift's desk, are significant of the extravagance and the poverty which point to ruin.

We cannot say how long such a spendthrift life may continue, because Time has no definite relation to the phenomena of starvation; these phenomena depend on certain specific changes going on in the body, which may occur with indefinite rapidity. Within the same period of time the *whole cycle of change* necessary for destruction may have completed itself, or *only a few of the stages* in this cycle may have been gone through: a man under certain conditions will not survive six days' fasting, and under other conditions he will survive six weeks'.

But if we cannot with any precision say *how long* starvation will be in effecting its fatal end, we can say *how much* waste is fatal. From the celebrated experiments of Chossat on Inanition,* it appears that death arrives whenever the waste reaches an average proportion of two-fifths. That is to say, supposing an animal to weigh 100 lb., it will succumb when its weight is reduced to 60 lb. Life may of course cease before that point is reached, but cannot, in ordinary circumstances, be prolonged after it.

The average loss which can be sustained is 40 per cent; sometimes the loss is greater, especially if the animal be very fat: thus in the *Transactions of the Linnæan Society*, a case is reported of a fat pig which was buried under thirty feet of chalk for one hundred and sixty days; his weight fell in that period no less than 75 per cent. Curiously enough, as an illustration of what was just said respecting Time not being an index of the amount of change, fishes and reptiles were found by Chossat to perish at precisely the same limit of *weight* as that at which warm-blooded animals perished, but they required a period three-and-twenty times as long to reach it in: thus if the experiment be performed of starving a bird and a frog during the warm weather, although both will perish when their loss of weight reaches 40 per cent, the one will not survive a week, the other will survive three-and-twenty weeks.

---

* CHOSSAT: *Recherches Expérimentales sur l'Inanition.* 1843.

It appears from Chossat's observations that the animal body daily wastes one twenty-fourth of its entire weight; and this accords very closely with the experiments of Bidder and Schmidt, which show that the animal requires at least one twenty-third of its entire weight in daily assimilable food, otherwise it will lose in substance.[*]

III. Prolonged Fasting.—Having clearly fixed these principles, we may proceed to consider the many remarkable cases of *prolonged fasting* which appeal to the credulity of the public, and which find a place even in very grave treatises, as well as in the less critical columns of newspapers. Are we to believe these marvels, or reject them? and on what grounds are we justified in rejecting them? Such questions the reader will frequently be called upon to answer; and as a contribution towards the formation of a definite and philosophical judgment, here are some of the most striking cases on record, and the physiological principles implied in them.

---

[*] Bidder u. Schmidt: *Die Verdauungssäfte und der Stoffwechsel*, 1852.

The human body is in many respects so different from that of animals, especially in its complexity, that we can draw no very accurate conclusion from *their* powers of enduring abstinence; but after all, the differences will only be differences of degree, and the same physiological laws must regulate both, so that we may be certain of the effect of abstinence on man not being *essentially* dissimilar to that on all other warm-blooded animals.

Let us therefore first see how the case stands with animals. The experiments of Pommer establish that carnivorous animals resist starvation longer than the herbivorous; birds of prey longer than birds feeding on seeds and fruits.

I think we might *a priori* have deduced this conclusion from the known differences in the *intervals* of recurring Hunger, and in the different quantities of food eaten by the two classes. The carnivorous animal eats voraciously when food is within reach, but having satisfied his appetite, he remains several hours before again feeling hungry; and in a state of nature the intervals between his meals are necessarily variable, and often much prolonged, because his food is neither abundant nor easy of access. The herbivorous animal, on the other hand, has his food constantly within reach, and is almost always eating, because an enormous amount of vegetable food is needed to furnish him with sustenance. The lion, or the cat, becomes inured to long abstinence; the rabbit or the cow scarcely knows the feeling. It is clear, therefore, that the one will better endure long fasting than the other.

Chossat's experiments on eight-and-forty birds and animals, show that the average duration of life exceeded nine days and a half—the maximum being twenty days and a half, the minimum a little more than two days.

The young always die first, the adult before the aged; this is true of men as of animals.

Some of the simpler animals exhibit remarkable powers of endurance. Latreille pinned a spider to a cork, and after four months found it still alive. Baker kept a stag-beetle three years in a box without food, and at the end of that period it flew away. Müller relates that a scorpion not only survived the voyage from Africa to Holland, but continued without food for nine months afterwards. Rondelet kept a fish three years without food, and Rudolphi a *Proteus anguineus* five years! Snakes, we know, live for many months without eating; and Redi found that a seal lived, out of water and without food, four weeks.

In all these cases, except that of the fish kept by Rondelet, the animals were quiescent, and did not waste their substance by the ordinary activities; and with regard to the fish, some doubts may be entertained whether it did not find worms and larvæ in the water.

Passing from animals to man, we find that death arrives on the fifth or sixth day of total abstinence from food and drink. But this is a general statement to which various exceptions may be named. Much depends on the peculiar constitution of the individual, his age, health, and other conditions. Some die on the second and third days; others survive till the tenth, eleventh, and even sixteenth days. Again, considerable differences will result from the different situations in which the men are placed—such as those of quiescence or activity, of temperature, moisture, &c.

The examples of protracted fasting recorded are, as usual, deficient for the most part in that rigorous authenticity which is demanded by science; many of them are obviously fabulous exaggerations. M. Bérard has borrowed the following from Haller, adding some cases which came under his own knowledge. I give them as specimens—not as data.

" A young girl, ashamed to confess her poverty, went without food for seventy-eight days, during which she only sucked lemons.

" Another woman of the same place remained four months without food, and another fasted a whole year.

" Haller reports two other cases of fasting for three and four years.

" Mackenzie reports in the *Philosophical Transactions* the story of a young girl who had lockjaw for eighteen years, and had taken no food during four years.

" A Scotchwoman is reported in the *Philosophical Transactions*, vol. lxvii., to have lived eight years without taking anything except a little water on one or two occasions.

" A case of fasting for ten years is celebrated in many works. Fabrice de Hilden, who took precautions against deception, says that Eva Flegen neither ate nor drank during six years.

"But all these stories are surpassed by that of a woman who remained fifty years without food; it is added, however, that she sometimes took skimmed milk."

"Admitting," says M. Bérard, "that there has been deception in some of these cases, and that the love of the marvellous has presided over the narration of others, we cannot refuse to believe that some are authentic. Every year such cases are registered. In 1836, M. Lavigne invited me to visit a woman of fifty-two, who, after having reduced herself to a glass of milk daily during eighteen months, had taken nothing in the shape of food or drink during the last five months. In 1839, M. Parizot communicated to me the fact of a girl at Marcilly who had taken no solid nutriment for six years, and for the last five years no liquid or solid. In 1838, M. Plongeau wrote to me to say that he had seen a woman at Ayrens, aged eight-and-forty, who during the last eight years had received no nourishment whatever." *

It is rather startling to find so learned a physiologist as M. Bérard recording such cases, and trying to explain them. The possibility of deception and exaggeration is so great, that we are tempted to reject almost every one of these cases rather than reject all physiological teaching.

---

* BÉRARD: *Cours de Physiologie*, 1848, i. 538.

The following is one of the most extraordinary of the cases which are repeated by modern writers with confidence. Janet M'Leod, after epilepsy, and fever, remained five years in bed, *seldom speaking*, and receiving food only by constraint. At length she obstinately refused all sustenance, her jaws became locked, and in attempting to force them open two of her teeth were broken. A small quantity of liquid was introduced by the aperture, none of which she swallowed, and dough made of oatmeal was likewise rejected. *She slept much*, and her head was bent down on her breast. In this deplorable state she continued four years, without her relatives being aware of her receiving any aliment except a little water; but after a longer interval she revived, and subsisted on crumbs of bread with milk, or water sucked from her hand.

Attention is called to the two facts of Janet's seldom speaking and sleeping much, because, supposing the case to be true, they materially affect the question. In a state of such quiescence as is here implied, the waste of the body would be reduced to a minimum, consequently the need of food would be minimised. Nevertheless, in the present state of Physiology, I think we are justified in asserting that some deception or exaggeration, not now ascertainable, is at the bottom of this as of all similar cases; and until a case free from all suspicion shall have been produced for the satisfaction of Science, we are bound to deny the probability of such stories; since that which all our knowledge shows to be in itself contradictory, must necessarily have the highest improbability, and can only be accepted on the most rigorous evidence. Either we must give up our Physiology altogether, or we must reject these stories.

For observe, on the one hand, several of the reported cases of long fasting have been subsequently proved to be impostures, and this naturally throws a suspicion over all similar cases. On the other hand, physiological laws, established by induction from thousands of facts tested by every variety of method, pronounce these cases to be not possible; and we are called upon to decide whether it is more probable that these inductions should be wrong, or that some imposture or exaggeration should lie at the bottom of the narrated marvels. There cannot be a moment's hesitation as to which alternative we must accept; but the reader will naturally desire a clear conception of the physiological contradictions which I have asserted to be implied in these marvellous narratives—the more so as many professed physiologists do not seem to be fully aware of them.

Supposing the *waste* of the body to be reduced to a minimum by the perfect quiescence in which the patients remained, we must still bear in mind that this *diminution* is not *total arrest* of waste. The patient scarcely moves, seldom speaks, and sleeps much. Very little destruction of tissue will take place, compared with the amount destroyed by the same person in ordinary activity, and very little food will be needed to repair such waste; but although *comparatively* small, the amount of waste will be *absolutely* large; we cannot say how large it will be, we can only say that it must be large.

Let us fix our attention on only two sources of this waste, and the proof will be evident. The production of animal heat is only possible through a large amount of chemical change going on in the organism; it is produced by "direct combustion" (according to the chemical school of physiologists), by "the disengagement of heat in chemical compositions and decompositions" (according to another school), and according to all schools the high temperature of the body depends on organic processes, which necessarily imply waste of tissue. The warmth of the bed in which the patient lies is not sufficient to preserve her temperature at its proper height; she must burn her own substance to keep up her animal heat; and when we think of the high degree of temperature maintained during a period of four years, solely by the combustion of the body itself, we shall see at once that it is utterly *impossible* any organism, during so long a period, could sustain such waste without repair.

Here, then, is the dilemma: either Janet M'Leod *did* maintain the ordinary temperature of the body during these four years, in which case she must have destroyed more tissue to produce that heat than the body could lose without cessation of life; or she did *not* maintain the ordinary temperature, in which case she would have died from the very want of this animal heat, since all organisms perish when their normal temperature is considerably lowered.

Let us now consider the second source of waste. Janet breathed during these four years ; gently, we may suppose, and with no deep inspirations, yet constantly, day and night without interruption. Now, what does this breathing depend on ? It depends on the constant interchange between carbonic acid in the blood, and oxygen in the air. Unless there were carbonic acid in the blood, no exchange could take place, no breathing could be effected.* Every moment, therefore, some small portion of carbonic acid must be separated from the blood, and replaced by oxygen. Whence came this carbonic acid ? From destruction of tissue. Directly, or indirectly, carbonic acid was produced in the act of waste. Its presence implies waste, and the act of breathing implies a continuous supply of such waste.

That this is no hypothesis, but the simple expression of the facts, every physiologist knows. It may be rendered generally intelligible by referring to what is observed with the hybernating animals. The dormouse begins its winter sleep well clothed with fat. It never moves for months ; its respiration is slow and feeble, but it does breathe, and the waste of its tissues which this breathing causes is very noticeable at the close of winter. Now, if we suppose Janet to have been in a state of "suspended vitality" analogous to that of the dormouse, we shall still have to admit that her breathing alone would gradually waste her substance ; and however slow that waste may be supposed, it cannot have been such that four years would not have exhausted the whole amount of available material. Every time she moved in bed, every time she spoke, every time she raised her hand, the rate of waste would be accelerated.

---

* This will be shown further on in the Chapter on RESPIRATION and SUFFOCATION.

It is found that a slug kept without food loses one-eleventh of its weight in six weeks. We cannot admit that, even in a bedridden girl, the vital activity would be slower than in a slug; and we know from Chossat's experiments that the loss of two-fifths of their weight destroys all animals.

From these general considerations, which might be multiplied, I affirm that, unless all Physiology is a delusion, the marvellous stories of four years' fasting, and the like, are impostures; and this affirmation is strengthened by all the cases in which the motive and possibility of deception are eliminated. Thus when men have voluntarily starved themselves to death, they have never survived three months. Granié, who murdered his wife, starved himself in the prison of Toulouse, and expired on the sixty-third day, during which time he drank water, and occasionally ate a little. The religious enthusiast, whom Dr Willan refers to, lived only two months, although he occasionally sucked an orange.

These men only survived thus long, because in abstaining from solid food, they did not also abstain from liquid. Life is considerably prolonged if liquid be taken. Redi found that birds kept without water as well as food lived only nine days; those to whom he gave water lived twenty days.* I cannot, however, agree with those physiologists who, like Burdach and Bérard, attribute this sustaining power of water to the organic particles suspended in it, because such an amount must necessarily be quite inadequate to supply the loss of an organism whose waste is rapid; and we must remember that an animal dies of *Thirst* even more rapidly than of Hunger; so that when water is withheld, the death is hastened by the complication of two causes. Now Janet M'Leod, and other persons said to have lived without food or drink, were under the pressure of these two causes, and sustained that pressure, we are told, four years!

We are thus forced to reject all narratives of absolute fasting prolonged over three months.

Having considered the effects of total abstinence, we may now inquire into the effects of partial abstinence.

An animal deprived of food perishes whenever its loss of weight reaches a certain point; and, curiously enough, *insufficiency of food* causes death at precisely the same point as total want of food, *i. e.* as soon as the original weight is reduced to six-tenths. Men, therefore, reduced to an insufficient allowance, whether from famine, shipwreck, or siege, will inevitably perish unless the allowance be increased; it will be as if they had received no food at all, only they will be longer before they succumb. An important lesson is contained in this fact, and one which should never be forgotten in the management of prisons, schools, or workhouses.

---

* REDI: *Osservazioni intorno agli animali viventi;* quoted by BURDACH.

IV. ASPECTS OF STARVATION.—Terrible are the aspects of starving men; and it is well that we should know these aspects, lest we be the dupes of impostors, or confound the truly wretched with the professional mendicant. The first noticeable point is the excessive thinness of starving men, which is not the leanness of lean men, but manifests itself as unmistakable emaciation. The face is always lividly pale, the cheeks are sunken, the eyes—what an expression in the eyes! never to be forgotten by those who have once seen it: all the vitality of the body seems centred there, in feverish brightness; the pupil is dilated, and the eye is fixed in a wild stare, which is never veiled by the winking lids. All movements of the body are slow and difficult: the hand trembles; the voice is feeble; intelligence seems gone; the wretched sufferers, when asked what they feel, have but one answer, "We are hungry."

There is one remarkable fact with reference to starvation which may here be noted, and that is the resistance opposed by the nervous substance to the effects of emaciation. Instead of being the first to suffer from deficient food, as its complexity and the lateness of its appearance in the animal series would lead us to suppose, the nervous tissue is the last to be affected. From the experiments of Chossat we learn that, in every 100 parts of *each* of the following substances, 93 are lost of the fat, 52 of the liver, 42 of the muscles, 16 of the bones, and only 2 of the nerve-substance, by the time starvation has terminated in death. The idea of our solid bones, principally composed of inorganic matter, losing eight times as much as our semi-liquid nerves, which are so predominantly organic in their structure, will seem very paradoxical; and the paradox is increased when we learn that, in spite of fat being beyond all proportion the most destructible tissue in the body, Von Bibra finds the fat in the brain scarcely affected in starvation, although the fat in the muscles has been greatly wasted.*

It is this which enables us hypothetically to explain the sleeplessness of men and animals suffering from hunger. A starving man has been known to remain seven days and nights without sleep. This nervous excitability, which often manifests itself as delirium, probably arises from the disengagement of the brain from those organic activities which in the normal state call so largely on its energies; for the energies of the brain are not expended only on intelligence and emotion, but likewise, and to a great extent, on the functions of nutrition and locomotion. Considering the nervous system as a centre or fountain of influence, we may detect three streams in which the influence flows—a nutritive stream, a locomotive stream, and a sensitive stream. If the demand from the nutritive stream be large, the supply to the sensitive and locomotive streams will be proportionately reduced. Deep thought, or anxiety, disturbs the digestion and circulation; violent and protracted exercise, amounting to fatigue, incapacitates for thinking: the habitually trained athlete is nearly an idiot, the over-eater little better. When, therefore, a man is starving, the amount of nervous activity usually expended on his nutritive system is disengaged; and as his feebleness diminishes his muscular activity, the amount of nervous influence usually expended on locomotion is reduced, leaving the brain, with all this surplus activity, to prey upon itself: sleeplessness and madness naturally result from this over-excitement.

---

* Canstatt: *Jahresbericht*, 1854, p. 119.

V. AGONIES OF STARVATION.—Respecting the agonies endured by starving men, we have little accurate information. When those who have undergone the horrors of starvation are preserved, and attempt to recount them, they cannot do more than give vague indications; for there is nothing more difficult to describe than the sensations of the digestive organs, even during the continuance of the sensation; and how difficult it is to describe them when past, may be conceived by any one who attempts to do so in his own case. Most of the narratives we have are recorded by men little accustomed to analyse their sensations, and we must be content to fix our attention on the general characteristics of these narratives. From these cases two may be selected.

Goldsmith says that the captain of a wrecked vessel told him that "he was the only person who had not lost his senses when they received accidental relief. He assured me his pains at first were so great that he was often tempted to eat a part of the men who died, and which the rest of his crew actually lived upon. He said that, during the continuance of this paroxysm, he found his pains insupportable, and was desirous at one time of anticipating that death which he thought inevitable. But *his pains gradually ceased after the sixth day* (for they had water in the ship, which kept them alive so long), and then he was in a state rather of languor than desire; nor did he much wish for food except when he saw others eating. The latter part of the time, when his health was almost destroyed, a thousand strange images rose upon his mind, and every one of his senses began to bring him wrong information. When he was presented with food by the ship's company that took him up, he could not help looking at it with loathing instead of desire; and it was not till after four days that his stomach was brought to its natural tone, when the violence of his appetite returned with a sort of canine eagerness."*

It will doubtless seem very strange to the uninitiated that a man after prolonged fasting, when his system is in such need of food and his appetite so keen, should be nevertheless in no proper condition to eat that food, and can only arrive at the proper condition by degrees, by eating a little at a time. The fact is, however, that, like all other organs, the stomach suffers for want of regular work. In fasting, the glands no longer secrete; the blood quits the stomach; the regular activity is interrupted; and when food again calls upon the stomach to do its old work, there is not the old vigour at command. Gradually the stimulus of food recalls the vigour of the secreting glands, and then appetite may be safely indulged.

---

* *History of the Earth*, ii. 126.

The next case is peculiarly valuable, as being the daily record of a man who voluntarily starved himself. He was a merchant, whose losses so preyed upon his mind that he resolved on suicide; and after roaming about the country from the 12th to the 15th of September 1818, he dug himself a grave in the wood, and remained there till the 3d of October, when he was found, still living, by an innkeeper. Hufeland, who records the case, says that, after an abstinence of eighteen days, the man still breathed, but expired immediately after a little soup had been forced down his throat. On his person they found a diary, written in pencil, from which the following are extracts :—

"*Sept.* 16.—The generous philanthropist who may find my corpse is requested to bury it, and to repay himself for the trouble by my clothes, my purse, my pocket-book, and knife. I have not committed suicide, but I die of starvation because bad men have deprived me of my fortune, and I do not choose to be a burden on my friends. It is unnecessary to open my body, since I have said I die of starvation.

"*Sept.* 17.—What a night I have passed! It has rained; I am wet through. I have been so cold.

"*Sept.* 18.—The cold and rain forced me to get up and walk; my walk was very feeble. Thirst made me lick up the water which still rested on the mushrooms. How nasty that water was!

"*Sept.* 19.—The cold, the length of the nights, the slightness of my clothing, which makes me feel the cold more keenly, have given me great suffering.

"*Sept.* 20.—In my stomach there is terrible commotion; hunger, and, above all, thirst, become more and more frightful. For three days there has been no rain. Would that I could lick up the water from the mushrooms now!

"*Sept.* 21.—Unable to endure the tortures of thirst, I crawled with great labour to an inn, where I bought a bottle of beer, which did not quench my thirst. In the evening I drank some water from the pump, near the inn where I bought the beer.

"*Sept.* 23.—Yesterday I could scarcely move, much less write. To-day thirst made me go to the pump; the water was icy cold, and made me sick. I had convulsions until evening; nevertheless, I returned to the pump.

"*Sept.* 26.—My legs seem dead. For three days I have been unable to go to the pump. Thirst increases. My weakness is such that I could scarcely trace these lines to-day.

"*Sept.* 29.—I have been unable to move. It has rained. My clothes are not dry. No one would believe how much I suffer. During the rain some drops fell into my mouth, which did not quench my thirst. Yesterday I saw a peasant about ten yards from me. I bowed to him. He returned my salutation. It is with great regret I die. Weakness and convulsions prevent my writing more. I feel this is the last time . . . . ."

This pathetic case illustrates, as indeed all other cases do, the truth that Thirst is far more terrible than Hunger. The man's resolution was not strong enough to resist the desire for drink, yet he never seems to have faltered in his determination to refrain from food. It will be further noticed that he ceases to complain of the cold when thirst sets in fiercely, because then fever had also supervened.

VI. THE SENSATION OF HUNGER.—The sensation of Hunger is at first rather agreeable, but it quickly becomes unpleasant if prolonged. The sense of keen appetite is delightful, but that "sinking in the stomach" which ensues, quickly passes from an uneasy sensation into positive pain. The pain soon becomes acute; and if food be still withheld, we feel as if the stomach were being torn by pincers. This is followed by a state of general exhaustion, feverishness, headache, light-headedness, often flaming into madness. The whole being seems possessed by one desire, before which even the energetic instinct of maternity has been known to give way, and mothers have actually disputed with their companions for the flesh of their dead children.

But let us avert our eyes from such scenes, and turn them on that of the eight colliers, who were shut up in a pit for one hundred and thirty-six hours.* The first day they shared between them half a pound of bread, a morsel of cheese, and two mugs of wine, which one of them had brought into the mine, and refused to keep for himself alone. Two of the men had eaten before descending into the mine, and they generously declared that they should not die sooner than the others, and would not share the small supply of food. It is very remarkable that these men, who for five days had no nourishment whatever, declared, when they were rescued, that their abstinence had not greatly inconvenienced them. If we knew more of the circumstances, we might perhaps explain this now inexplicable fact.

---

* This case is quoted by LONGET in his *Traité de Physiologie*, 1857.

What causes the sensation of Hunger ?

It has been seen that the absence of food needed to repair the waste of tissue is the primary cause ; but it has also been seen that this primary cause may exist without the existence of that sensation known to us as Hunger. All animals need food, but we have no ground for supposing that polypes, jelly-fish, and other simpler animals destitute of a nervous system, feel the *sensation* of Hunger ; we must therefore seek for some more proximate cause of this sensation.

The popular notion is that Hunger arises from emptiness of the stomach, which emptiness, according to some physiologists, allows the walls of the stomach to rub against each other, and then the friction causes the sensation.

It is easy to show the inaccuracy of this hypothesis, but two facts will suffice here: first, the stomach is always empty some time before Hunger is felt ; secondly, it may be empty for days together—in illness—without the slightest sensation of Hunger being felt.

Another notion is that the gastric juice accumulates in the stomach, and attacks its walls. Such a cause would certainly be ample for the effect, and I know but of one objection to our accepting it, namely, that the fact on which the explanation rests is unfortunately a fiction; the gastric juice does *not* accumulate in the empty stomach, but is only secreted after the stimulus of food.

A more ingenious explanation has been propounded by Dr Beaumont, whose name is always cited when Digestion is under discussion, because he was enabled to enrich science with many valuable observations made on a patient who had a hole in his stomach, produced by a gunshot wound. "During the hours of fasting," says Dr Beaumont, "the gastric juice is slowly being secreted in the follicles * and retained in their tubes, thereby distending them: this distension, when moderate, produces the sensation of Appetite; when more powerful, of Hunger."

There are several analogies which give colour to this explanation. Thus, milk is slowly accumulated in the breast, and the sense of fulness, if unrelieved, soon passes into that of pain. But ingenious as the explanation is, a closer scrutiny causes us to reject it.

Out of many arguments which might be urged, I will mention only two—one anatomical, and one physiological. If the gastric juice were accumulated in the tubes, there is no anatomical obstruction to its immediate passage into the stomach, and the distension would at once be obviated (see Fig. 5). Nor have we any good ground for supposing that an accumulation does take place; for Dr Beaumont's argument that it *must* take place, because it flows so abundantly on the introduction of food into the stomach, would equally prove that tears must be accumulated in advance, because they gush forth so copiously on the first stimulus of grief; or that saliva must be accumulated, because it flows so freely whenever a stimulus is presented.

---

* The *follicles* are small secreting glands in the lining membrane of the stomach. They will be described and figured in the chapter on DIGESTION and INDIGESTION, fig. 5.

While Dr Beaumont's explanation wants an anatomical basis, it is still more directly at variance with the physiological fact, that when food is injected into the veins or the intestines, the sensation of Hunger disappears, although the stomach is as empty as it was before, and the tubes as distended as they were before.

The fact last named would dispose us to believe that want of food was, after all, the proximate as well as the primary cause of Hunger, did we not know that tobacco, opium, and even inorganic substances, introduced into the stomach, will remove the sensation. In a succeeding chapter (see FOOD and DRINK) we shall learn that there are nations who eat clay to allay their hunger; and we all know how the first mouthful of food takes away the sharpness of the sensation, although two or three hours must elapse before the food will really have *entered* the body. For we must remember that food in the stomach is as much *outside* the organism as if it were in the hand. The digestive canal is nothing but a folding-in of the general envelope, like the inverted finger of a glove; and until the absorbent vessels carry the food from the stomach *into* the circulating system, the food remains *outside*.

Here, then, are two noticeable facts: we may relieve the sensation of Hunger without directly acting on the stomach, the mere supply of food to the blood sufficing; and we may relieve the sensation simply by acting on the stomach, the want of food being as great as before. Do not these facts indicate that Hunger must be related to the *general* state of the system, and to the *particular* state of the stomach? If we once regard the subject in this light, we shall be easily led to perceive that although the general state of the system, under deficiency of food, is the *primary cause* of Hunger, it is only so in as far as it produces a certain *condition of the stomach*; and this condition of the stomach is the *proximate cause* of the sensation.

I think this mode of viewing it will extricate us from the difficulties which have been brought forward in the many discussions as to whether the stomach, or one part of the nervous system, is the *seat* of Hunger. The stomach is the seat of the sensation, just as the eyes are the seat of the sensation of sleepiness: the general state of exhaustion causes the eyes to droop heavily, and the general state of the system causes the stomach to produce the sensation of hunger; and as in sleepiness we may relieve the sensation by bathing the eyes with cold water, yet this will not relieve the general exhaustion; so in hunger, we may relieve the sensation by opium, or even clay, but this will not relieve that general state of the system which produced the sensation.

It is evident that the general state of the system must be felt, and to it we owe those daily variations in comfort which we express in the terms "vigour," "gladness," "lassitude," "depression," &c.; nevertheless physiologists have not assigned a name to such sensations.

In a future chapter (FEELING and THINKING) I shall enter more fully into these, which I propose to call *Systemic* Sensations, because they arise in the system at large, or are not localised by consciousness in any one organ. But for the purpose of this chapter, current terms must be accepted; and although, therefore, strict accuracy would lead us to say that Hunger, as a Systemic Sensation, is caused by want of food to repair the waste of tissue, and, as a Local Sensation, is caused by a specific condition of the stomach; yet, following popular language, we must say that Hunger is a sensation having its seat in the stomach; and all the arguments or experiments which attempt to prove that its seat must be elsewhere, have reference to the general state of the system, but not to the specific sensation known to us as Hunger.

If we examine the stomach of a fasting animal, we shall find it pale, and in a condition of obvious *atony.* The blood has retreated from the smaller vessels, and circulates only in the larger channels. But no sooner is the organ stimulated by the introduction of food, or any irritant substance, than this pale surface becomes visibly congested, turgescent, and its secretions pour forth abundantly. With this rush of blood to the stomach the sensation of uneasiness is carried away. Hence we may conclude that Hunger is in some way dependent on the state of the circulation in the stomach.

VII. THIRST closely resembles Hunger in being a general or Systemic sensation, although it is usually considered only as a local sensation, arising from the dryness of the mouth and throat. This dryness of the throat and mouth, so familiar to us all, is produced by a deficiency of liquid in the body; but it may be, and often is, produced when there is no deficiency in the general system, nothing but a local disturbance, this disturbance producing a local sensation. Wines, coffee, and spices create a strong feeling of thirst, yet the two first increase the quantity of liquid instead of diminishing it. And we know how ineffectual liquid in any quantity is to quench the feeling of Thirst under some conditions, especially after long suffering.

Andersson, in his travels in Africa, describes the sufferings of his men and cattle, adding, "even when the thirsty men and animals were let loose in the water, although they drank to repletion, the water seemed to have lost its property, for our best endeavours to slake our thirst proved unavailing."* The long continuance of Thirst had produced a certain feverish condition which could not be immediately relieved when the system received its necessary supply of liquid; this shows that although deficiency of liquid is the *primary* cause of Thirst, the *proximate* cause must be some local affection which has been induced.

---

* ANDERSSON: *Lake Ngami*, p. 38.

On the other hand, this local sensation is so dependent on the system, that if water be injected into the veins or the intestines, Thirst disappears, although the mouth and throat have not been touched. A humid atmosphere prevents Thirst; a bath relieves it, becaus the water is absorbed through the skin.

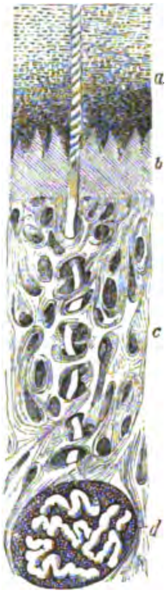
On this principle, Franklin grounds his advice t men who are exposed to scarcity of drink: they shoul bathe themselves in tubs of salt water, he says. Thi would undoubtedly relieve their thirst, but it is a pla which would be excessively dangerous in shipwreck unless food were abundant, since the abstraction of s much heat as would follow a bath, would in all proba bility be fatal.

VIII. Cause of Thirst.—As deficiency of food t supply the waste of tissue is the primary cause c Hunger, so deficiency of water to supply the wast which goes on incessantly in the excretions, respira tion, and perspiration, is the primary cause of Thirst.

Every time we breathe we throw from our lungs quantity of water in the form of vapour. We a made sensible of this when the breath condenses o the colder surface of glass or steel, and when, as in winter, the atmosphere is sufficiently cold to condense the vapour on its issuing from our mouths.

This is only one source of the waste of water: a more important source is that of perspiration, which in hot weather, or during violent exercise, causes the water to roll down our skins with obtrusive copiousness. But even when we are perfectly quiescent, the loss of water, although not obvious, is considerable.

Fig. 2.



SWEAT-GLAND.

a is the cuticle or scarfskin (epidermis), the deeper layers, dark in colour, being the *rete mucosum*; *b* are the papillæ; *c* the cutis or true skin (derma); *d* the sweat-gland, in a cavity of oily globules.

To render this intelligible, let attention be fixed on the following diagram, which represents one of the glands that secrete this perspiration. It represents the vertical section of the sole of the foot (after Todd and Bowman).

The gland at *d* is seen to possess a twisted duct, which passes upwards to the surface. From this tube comes the perspiration, sensible and insensible.

It is calculated that there are no less than twenty-eight miles of this tubing on the surface of the human body, from which the water will escape as *insensible perspiration*; and although the amount of water which is thus evaporated from the surface must necessarily vary with the clothing, the activity, and even the peculiar constitution of the individual, an average estimate has been attained which shows that from *two to three pounds of water* are daily evaporated from the skin. From the lungs it is ascertained that every minute we throw off from four to seven grains of water, from the skin eleven grains. To these must be added the quantity abstracted by the kidneys, a variable but important element in the sum.

It may not at first be clear to the reader why an abstraction of water daily should profoundly affect the organism unless an equivalent be restored. What can it matter that the body should lose a little water as vapour? Is water an essential part of the body? Is it indispensable to life?

Not only is water an essential part of the body, it might be called the *most* essential, if pre-eminence could be given where all are indispensable. In quantity, water has an enormous preponderance over all other constituents: it forms 70 per cent of the whole weight! There is not a single tissue in the body—not even that of bone, not even the enamel of the teeth—into the composition of which water does not enter as a necessary ingredient. In some of the tissues, and those the most active, it forms the chief ingredient. In the nervous tissue 800 parts out of every 1000 are of water; in the lungs 830; in the pancreas 871; in the retina no less then 927.

Commensurate with this anatomical preponderance, is the physiological importance of water. It is the carrier of the food, the vehicle of waste. It holds gases in solution, dissolves solids, helps to give every tissue its physical character, and is the indispensable condition of that ceaseless change of Composition and Decomposition on which the continuance of life depends.

From the elaborate experiments of Bidder and Schmidt, it is clear that the changes which go on in the organism are far more concerned with its water than with its solids. In the digestive fluids, for example, one-fifth to one-fourth of the water contained in the body will be found passing and repassing; whereas only one-sixtieth or one-seventieth of the solids of the body pass into them.*

Such being the part played by water in the organism, we can understand how the oscillations of so important a fluid must necessarily bring with it oscillations in our feelings of comfort and discomfort, and how any unusual abstraction of it must produce that disturbance of the general system which is known under the name of Raging Thirst—a disturbance far more terrible than that of Starvation, and for this reason: During abstinence from food, the organism can still live upon its own substance, which furnishes all the necessary material; but during abstinence from liquid, the organism has no such source of supply within itself.

Men have been known to endure absolute privation of food for some weeks, but three days of absolute privation of drink (unless in a moist atmosphere) is perhaps the limit of endurance. Thirst is the most atrocious torture ever invented by Oriental tyrants. It is that which most effectually tames animals. Mr Astley, when he had a refractory horse, always used thirst as the most effective power of coercion, giving a little water as the reward for every act of obedience. The histories of shipwreck paint fearful pictures of the sufferings endured from thirst; and one of the most appalling cases known is the celebrated imprisonment of one hundred and forty-six men in the Black Hole at Calcutta—a case frequently alluded to, but which must be cited here at some length on account of its physiological bearing :—

---

* BIDDER u. SCHMIDT: *Die Verdauungssäfte*, p. 287.

The Governor of Fort-William at Calcutta having imprisoned a merchant—the well-known Omychund,—the infamous Nabob of Bengal, Surajah Dowlah, on the look-out for a pretext, marched against Fort-William with a considerable force, besieged and took it, and imprisoned the surviving part of the garrison in the barrack-room named the Black Hole. The letter in which Mr Holwell, the officer in command, describes the horrors of this imprisonment is printed in the *Annual Register* for 1758, and from it the following extracts are made :—

" Figure to youself the situation of a hundred and forty-six wretches, exhausted by continual fatigue and action, crammed together in a cube of eighteen feet, in a close sultry night in Bengal, shut up to the eastward and southward (the only quarter whence air could reach us) by dead walls, and by a wall and door to the north, open only to the westward by two windows strongly barred with iron, from which we could receive scarce any the least circulation of fresh air. . . . We had been but a few minutes confined before every one fell into a perspiration so profuse, you can form no idea of it. This brought on a raging thirst, which increased in proportion as the body was drained of its moisture. Various expedients were thought of to give more room and air. To gain the former it was moved to put off their clothes; this was approved as a happy motion, and in a few moments every one was stripped—myself, Mr Court, and the two young gentlemen by me, excepted. For a little while they flattered themselves with having gained a mighty advantage; every hat was put in motion to gain a circulation of air, and Mr Baillie proposed that every man should sit down on his hams. This expedient was several times put in practice, and at each time many of the poor creatures, whose natural strength was less than that of others, or who had been more exhausted, and could not immediately recover their legs when the word was given to rise—fell to rise no more, for they were instantly trod to death or suffocated. When the whole body sat down, they were so closely wedged together that they were obliged to use many efforts before they could get up again. Before nine o'clock every man's thirst grew intolerable, and respiration difficult. Efforts were made to force the door, but in vain. Many insults were used to the guard to provoke them to fire on us. For my own part, I hitherto felt little pain or uneasiness, but what resulted from my anxiety for the sufferings of those within. By keeping my face close between two of the bars I obtained air enough to give my lungs easy play, though my perspiration was excessive, and thirst commencing. At this period so strong a urinous volatile effluvia came from the prison that I was not able to turn my head that way for more than a few seconds at a time.

"Now everybody, except those situated in and near the windows, began to grow outrageous, and many delirious. *Water! water!* became the general cry. An old Jemmantdaar, taking pity on us, ordered the people to bring us some skins of water. This was what I dreaded. I foresaw it would prove the ruin of the small chance left us, and essayed many times to speak to him privately to forbid it being brought; but the clamour was so loud it became impossible. The water appeared. Words cannot paint the universal agitation and raving the sight of it threw us into. I flattered myself that some, by preserving an equal temper of mind, might outlive the night; but now the reflection which gave me the greatest pain was, that I saw no possibility of one escaping to tell the dismal tale. *Until the water came I had not myself suffered much from thirst, which instantly grew excessive.* We had no means of conveying it into the prison but by hats forced through the bars; and thus myself, and Coles, and Scott supplied them as fast as possible. But those who have experienced intense thirst, or are acquainted with the cause and nature of this appetite, will be sufficiently sensible it could receive no more than a momentary alleviation: the cause still subsisted. Though we brought full hats through the bars, there ensued such violent struggles and frequent contests to get it, that before it reached the lips of any one, there would be scarcely a small tea-cupful left in them. These supplies, like sprinkling water on fire, only seemed to feed the flame. Oh! my dear sir, how shall I give you a just conception of what I felt at the cries and cravings of those in the remoter parts of the prison, who could not entertain a probable hope of obtaining a drop, yet could not divest themselves of expectation, however unavailing, calling on me by the tender considerations of affection and friendship. The confusion now became general and horrid. Several quitted the other window (the only chance they had for life) to force their way to the water, and the throng and press upon the window was beyond bearing; many, forcing their way from the further part of the room, pressed down those in their passage who had less strength, and trampled them to death.

"From about nine to eleven I sustained this cruel scene, still supplying them with water, though my legs were almost broke with the weight against them. By this time I myself was near pressed to death, and my two companions, with Mr Parker, who had forced himself to the window, were really so. At last I became so pressed and wedged up, I was deprived of all motion. Determined now to give everything up, I called to them, and begged them, as a last instance of their regard, that they would relieve the pressure upon me, and permit me to retire out of the window to die in quiet. They gave way, and with much difficulty I forced a passage into the centre of the prison, where the throng was less by the many dead, amounting to one-third, and the numbers who flocked to the windows; for by this time they had water also at the other window. . . . I laid myself down on some of the dead, and, recommending myself to Heaven, had the comfort of thinking my sufferings could have no long duration. My thirst now grew insupportable, and the difficulty of breathing much increased; and I had not remained in this situation ten minutes before I was seized with a pain in my breast, and palpitation of heart, both to the most exquisite degree. These obliged me to get up again, but still the pain, palpitation, and difficulty of breathing increased. I retained my senses notwithstanding, and had the grief to see death not so near me as I had hoped, but could no longer bear the pains I suffered without attempting a relief, which I knew fresh air would and could only give me. I instantly determined to push for the window opposite to me, and by an effort of double the strength I ever before possessed, gained the third rank at it—with one hand seized a bar, and by that means gained a second, though I think there were at least six or seven ranks between me and the window. *In a few moments the pain, palpitation, and difficulty of breathing ceased,* but the thirst continued intolerable. I called aloud '*Water for God's sake!*' I had been concluded dead; but as soon as the men found me amongst them, they still had the respect and tenderness for me to cry out, '*Give him water!*' nor would one of them at the window attempt to touch it till I had drunk. *But from the water I had no relief; my thirst was rather increased by it;* so I determined to drink no more, but patiently wait the event. I kept my mouth moist from time to time by sucking the perspiration out of my shirt-sleeves, and catching the drops as they fell like heavy rain from my head and face; you can hardly imagine how unhappy I was if any of them escaped my mouth. . . . . I was observed by one of my companions on the right in the expedient of allaying my thirst by sucking my shirt-sleeve. He took the hint, and robbed me from time to time of a considerable part of my store, though, after I detected him, I had the address to begin on that sleeve first when I thought my reservoirs were sufficiently replenished, and our mouths and noses often met in contact. This man was one of the few who escaped death, and he has since paid me the compliment of assuring me he believed he owed his life to the many comfortable draughts he had from my sleeves. No Bristol water could be more soft or pleasant than what arose from perspiration.

"By half-past eleven the much greater number of those living were in an outrageous delirium, and others quite ungovernable; few retaining any calmness but the ranks near the windows. They now all found that water, instead of relieving their uneasiness, rather heightened it, and Air! air! was the general cry. Every insult that could be devised against the guard was repeated to provoke them to fire on us, every man that could, rushing tumultuously towards the windows with eager hopes of meeting the first shot. But these failing, they whose strength and spirits were quite exhausted laid themselves down, and quietly expired upon their fellows; others who had yet some strength and vigour left, made a last effort for the windows, and several succeeded by leaping and scrambling over the backs and heads of those in the first ranks; and got hold of the bars, from which there was no removing them. Many to the right and left sunk with the violent pressure, and were soon suffocated; for now a steam arose from the living and the dead, which affected us in all its circumstances, as if we were forcibly held by our heads over a bowl of strong volatile spirit of hartshorn until suffocated; nor could the effluvia of the one be distinguished from the other. I need not ask your commiseration when I tell you that in this plight, from half an hour after eleven till two in the morning, I sustained the weight of a heavy man with his knees on my back, and the pressure of his whole body on my head; a Dutch sergeant who had taken his seat on my left shoulder, and a black soldier bearing on my right: all which nothing would have enabled me to support but the props and pressure equally sustaining me all round. The two latter I frequently dislodged by shifting my hold on the bars, and driving my knuckles into their ribs; but my friend above stuck fast, and, as he held by two bars, was immovable. The repeated trials I made to dislodge this insufferable encumbrance upon me, at last quite exhausted me, and towards two o'clock, finding I must quit the window or sink where I was, I resolved on the former, having borne truly, for the sake of others, infinitely more for life than the best of it is worth.

"I was at this time sensible of no pain and little uneasiness. I found a stupor coming on apace, and laid myself down by that gallant old man, the reverend Jervas Bellamy, who lay dead with his son, the lieutenant, hand in hand, near the southernmost wall of the prison. Of what passed in the interval, to the time of resurrection from this hole of horrors, I can give you no account."

At six in the morning the door was opened, when only three-and-twenty out of the hundred and forty-six still breathed. These were subsequently revived.

Although the principal cause of this mortality must be ascribed to the vitiated atmosphere rather than to Thirst, we nevertheless see some of the frightful phenomena of Thirst exemplified in this narrative. Death by asphyxia (from vitiated air) is generally peaceful, and not at all such as is described in the foregoing. Attention is moreover called to certain passages in italics. These show that the sensation of Thirst is not merely a sensation dependent on a deficiency of liquid in the system, but a local sensation dependent on a local disturbance: the more water these men drank, the more dreadful seemed their thirst; and the mere sight of water rendered the sensation, which before was endurable, quite intolerable. The *increase* of the sensation following a *supply* of water, would be wholly inexplicable to those who maintain that the proximate cause of Thirst is deficiency of liquid; but is not wholly inexplicable, if we regard the deficiency as the primary, not the proximate cause; for this primary cause having set up a feverish condition in the mouth and throat, that condition would continue after the original cause had ceased to exist. The stimulus of cold water is only a momentary relief in this case, and exaggerates the sensation by stimulating a greater flow of blood to the parts. If, instead of cold water, a little lukewarm tea, or milk-and-water, had been drunk, permanent relief would have been attained; or if, instead of cold water, a lump of ice had been taken into the mouth, and allowed to melt there, the effect would have been very different—a transitory application of cold increasing the flow of blood, a continuous application driving it away.

If, therefore, the reader is ever suffering from intense thirst, let him remember that tepid drinks are better than cold drinks, ice is better than water.

We must not, however, forget that although, where a deficiency of liquid has occasioned a feverish condition of the mouth and throat, no supply of cold liquid will at once remove that condition, the relief of the Systemic sensation not immediately producing relief of the local sensation, nevertheless, so long as the system is in need of liquid, the feeling of thirst must continue. Claude Bernard observed that a dog which had an opening in its stomach drank unceasingly, because the water ran out as fast as it was swallowed; in vain the water moistened mouth and throat on its way to the stomach, Thirst was not appeased because the water was not absorbed. The dog drank till fatigue forced it to pause, and a few minutes afterwards recommenced the same hopeless toil; but no sooner was the opening closed, and the water retained in the stomach, from whence it was absorbed into the system, than thirst quickly vanished.*

After learning the physiological importance of water, and remembering how the water is continually being removed from the body in respiration, perspiration, and the various excretions, we are greatly puzzled by the great variations which animals exhibit in the quantity they drink. The difficulty is not explained by a reference to the food of the animals, for some vegetable feeders require large quantities of water, while others subsist for months without drinking, the supply they receive in the vegetables they eat being sufficient for their wants.

Dr Livingstone found the elands on the Kalahari Desert, although in places where water was perfectly inaccessible, with every indication of being in splendid condition, and their stomachs actually contained considerable quantities of water. "I examined carefully the whole alimentary canal," he says, "in order to see if there were any peculiarity which might account for the fact that these animals can subsist for months together without drinking, but found nothing. Other animals, such as the diiiker (*Cephalopus mergens*), the steinbuck (*Tragulus rupestris*), the gemsbuck (*Oryx capensis*), and the porcupine, are all able to subsist for many months at a time by living on bulbs and tubers containing moisture. Some animals, on the other hand, are never seen but in the vicinity of water. The presence of the rhinoceros, buffalo, and gnu, of the giraffe, zebra, and pallah (*Antelope melampus*), is always a certain indication of water being within seven or eight miles."*

---

* CLAUDE BERNARD: *Leçons de Physiologie Expérimentale*, ii. 51.

The only solution of the difficulty which presents itself to my mind is, that animals which can subsist long without drinking, do not lose more water by evaporation and excretion than can be replaced by their vegetable food, since that they *require* the same amount of water as other animals for the performance of all their functions is physiologically certain. It has been observed that, in persons who voluntarily abstain from drinking, the excretions were diminished to a minimum. Sauvages, in his *Nosologia Medica*, mentions the case of a member of the University of Toulouse who never knew what thirst was, and passed several months, even in the heat of summer, without drinking. Another case is cited by the same author of a woman who took no liquid for forty days. M. Bérard thinks that the marvellousness of these facts disappears when we remember how much liquid is contained in all food;† but I am rather disposed to doubt the accuracy of the facts than to accept such an explanation: at any rate, they are facts so very exceptional as to have little bearing on our general argument.

---

* LIVINGSTONE: *Missionary Travels in South Africa*, p. 56.

† BÉRARD: *Cours de Physiologie*, vol. ii. p. 504.

The effects of Thirst are first a dryness of the mouth, palate, and throat; the secretions become less copious; the mouth is covered with a thick mucus, the tongue cleaves to the palate, the voice becomes hoarse. Then the eyes flash fire, the breathing becomes difficult, a feverish excitement, often passing into delirium, comes on.* Sleep is fitful, and distressed by dreams akin to the torments of Tantalus. The men shipwrecked in the "Medusa" dreamt constantly of shady woods and running streams.

It is to be noticed that the sensation of Thirst is never agreeable, no matter how slight it may be, and in this respect is unlike Hunger, which, in its incipient state of Appetite, is decidedly agreeable. The bodies of those who have perished from Thirst show a general dryness of all the tissues, a thickening of the humours, a certain degree of coagulation of the blood, numberless indications of inflammation, and sometimes gangrene of the principal viscera. According to Longet, Thirst kills by an inflammatory fever, Hunger by a putrid fever.†

---

* From some experiments by Professor E. HARLESS, in Munich, at which I assisted, it appeared that the nerves gain an extraordinary *increase* in their excitability as their proportion of water decreases. (For a fuller account of this remarkable fact, see BIRKNER, *Das Wasser der Nerven*, 1858.) The nervous excitability, delirium, and sleeplessness, occasioned by Thirst, may be greatly owing to this cause.

† LONGET: *Traité de Physiologie.*

Such are Hunger and Thirst, two mighty impulses, beneficent and terrible, monitors ever vigilant, warning us of the need there is for Food and Drink—sources of exquisite pleasure and of exquisite pains, motives to strenuous endeavour, and servants to our higher aims. We are all familiar with them in their gentler aspects; may we never know them in their dreadful importunities!

CHAPTER II.

FOOD AND DRINK.

SECTION I.—ON THE NATURE OF FOOD.

Varieties of food—The eaters of clay—The chemical and physiological methods of investigation—"One man's meat another man's poison," illustrated—A caution to parents—Wholesome and unwholesome food—Relation of food to the organism—Vital process incapable of chemical explanation—Difference between chemistry and physiology—A canon of philosophy—Is the family a type of the State?—Chemical problems are quantitative; physiological problems qualitative—Liebig's classification of food into plastic and respiratory not acceptable—Liebig's error arises from his taking the chemical point of view—Facts against him—Experiments on animals—The rice-eating Hindoos—The food in cold countries—Results of inquiry—Inorganic food—Nutrition in plants and animals misunderstood—Phosphate of lime in the body; amount of water in the body; amount of salt—Uses of salt—The law of endosmosis.

An Irish peasant, in a windowless hut, dining off a meal of potatoes and skimmed milk, flavoured by the aroma of a lively imagination as each mouthful is "pointed" at the side of bacon hanging against the wall, and a London Alderman seated at a Guildhall feast, are two figures presenting an impressive contrast of the varieties of Food with which, in the restless activity of life, the human organism repairs its incessant waste. Potatoes and skimmed milk, and it may be a little sea-weed, supply the wants of the one; before the other there is spread a wasteful profusion of turtle captured on the North American coasts, of turkey reared in quiet farmyards, of mutton grazed upon the downs of Sussex, of beef fed on the rich pasture-lands of Herefordshire, of pheasants shot in a nobleman's preserves, of turbot from the Atlantic Ocean, and salmon from the Scotch and Irish rivers, of cheese from France and Switzerland, oil from Italy, spices from the East, and wines from Portugal, Germany, and France — a gathering from all nations, assorted with exquisite culinary skill. Yet, in spite of these differences in the things consumed by the two men, the dinner of the one and the dinner of the other become transmuted by vital processes into similar flesh and blood, into the same organic substance and organic force.

However various the articles of Food and Drink may be, it is clear that there must be a process by which all differences are annulled, and a similar result attained. Whatever characters these substances may have *outside* the organism, must be altered shortly after their entrance *into* it, specific differences must vanish, and all varieties be merged in a vital unity.

The hunter on the Pampas subsists on buffalo beef, with scarcely a particle of vegetable food to vary his diet. The Hindoo is content with rice and rancid butter, and cannot be induced to eat flesh. The Greenlander gorges himself with whale oil, and animal fats of any kind he can secure; the moderate Arab has his bag of dates, his lotos-bread, and dhourra. On the coast of Malabar we find men regarding with religious horror every species of animal food; while the native of New Holland has not a single edible fruit larger than a cherry on the whole surface of his vast island. The Englishman considers himself ignominiously treated by fortune if he cannot get his beef or bacon; the peasant of the Apennines is cheerful with his meal of chestnuts.

Besides varieties in the staple articles of Food, there are the infinite varieties of fancy. Our Chinese friends make delicacies of rats and of birds' nests; our French allies are epicurean over the hind-legs of frogs. The ancients, who carried epicureanism to lengths never dreamed of by Guildhall, thought the hedgehog a titbit, and had a word to say in favour of the donkey, which they placed on an equality with the ox; dogs they considered equal to chickens; and even cats were not to be despised. The pork, which we eat with great confidence, they considered, and not untruly, the least digestible of animal meats, fit only for artisans and athletes. They ate snails, at which we shudder, with the gusto we acknowledge in eating oysters. It would be difficult to persuade the British stomach to dine, in full consciousness, off a "sirloin of donkey," flanked by "ribs of dog, with fried toadstools." Is this repugnance only prejudice, or were Greek dogs and donkeys more succulent than ours?

The Clay-eaters.—The varieties just rehearsed are at any rate easily accepted as probable aliments, but what will the reader say on hearing that in many parts of the world even clay is a respectable and respected food? Travellers, who see strange things, are very positive in their assertions on this head. Humboldt, a man whose word justly carries with it European authority, confirms the statement of Gumilla, that the Otomacs of South America, during the periods of the floods, subsist entirely on a fat and ferruginous clay, of which each man eats daily a pound or more. The well-known botanist and explorer, Martius, informed me that the Indians of the Amazon eat a kind of loam, even when other food is abundant. Molina says the Peruvians frequently eat a sweet-smelling clay; and Ehrenberg has analysed the edible clay sold in the markets of Bolivia, which he finds to be a mixture of talc and mica. The inhabitants of Guiana mingle clay with their bread; and the negroes in Jamaica are said to eat earth when other food is deficient. According to Labillardière, the inhabitants of New Caledonia appease their hunger with a white friable earth, said by Vauquelin to be composed of magnesia, silica, oxide of iron, and chalk. The same writer asserts that at Java a cake is made of ferruginous clay which is much sought for by women in their pregnancy. To conclude this list we must add Siam, Siberia, and Kamtschatka as countries of clay-eaters.*

This is rather a staggering accumulation of assertions, which we cannot dismiss altogether, even if we suppose a large allowance of scepticism justifiable. Granting the fact that certain kinds of earth are really nutritious—and it is difficult to escape such a conclusion—we are completely at a loss for an adequate explanation of it. Little light is thrown on it by the assumption, probable enough, that the earth must contain organic matters; because we should imagine that in ten pounds of such earth there could scarcely be contained sufficient organic matter to supply the demands of an adult.

---

* BURDACH: *Traité de Physiologie*, ix. 260.

Nor will it get rid of the difficulty to say that the earth only appeases hunger without nourishing the system; because, in the first place, Humboldt's testimony is that the Otomacs *subsist* on the clay at periods when other food is deficient; and, in the second place, although the *local* sensation of Hunger may be appeased by introducing substances into the stomach, the more imperious *systemic* sensation of Hunger is not thus to be appeased.*

We must, therefore, be content, at present, with accepting the fact, which the science of a future day may possibly explain.

Omitting clay, as not explicable for the present, we propose to take the reader with us in an inquiry, having for its object to ascertain what Science can tell us positively respecting the relation of alimentary substances and the organism—to see what is known respecting Food and its varieties. If in the course of this survey we detain the reader to consider certain generalities, when he is impatient to arrive at the details, let him be assured that these generalities, seemingly too abstract and remote for immediate practical objects, are essential to a right comprehension of the details; and that our most practical and pressing objects, whether of feeding cattle, or feeding ourselves, do inevitably rest upon abstract philosophic principles, and are determined by scientific hypotheses. We promise him abundant detail, but must ask him to approach the question through such avenues as we shall open, and not to try any short cut of his own.

---

* See the Chapter on HUNGER AND THIRST.

I. HOW TO INVESTIGATE FOOD.—Assured as we are that all alimentary substances must be transformed into the organic unity we name Blood, and assured also that the substances so transformed are really various in kind, specifically distinct *before* they have undergone this transformation, it is clear that our chief attention should be withdrawn from the alimentary *substances* to fall with greater emphasis on the alimentary *process;* that is to say, we must less consider what the Foods are in themselves, than *what relation* they bear to the organism which they nourish.

Obvious as this may seem, it has generally been disregarded, especially of late years. The researches into the nature of Food have been extensive and minute, but they have been almost exclusively confined to alimentary substances, which have been analysed, weighed, and tabulated with great labour, and in a chemical point of view with considerable results;

but in a physiological point of view—the only one really implicated—with scarcely any results at all. No one doubts that Food is a physiological question, inasmuch as it relates to an organism. Nevertheless, it has fallen into the hands of the chemists; and our treatises, text-books, and popular works, have been encumbered by hypotheses which may amuse speculative ingenuity, but furnish very little positive result.

Against this vice of Method, and this misdirection of valuable labour, a voice should energetically be raised. The error is not a speculative error, simply; it is one carrying important consequences; it either leads physicians and farmers into serious mistakes, or leads them to throw up scientific guidance in disgust, because the hypothesis, so convincing on paper, turns out stubbornly irreconcilable with fact.

Let us not, however, be misunderstood. In declaring the chemical hypothesis on the subject of Food to which Liebig, Dumas, Boussingault, Payen, and others, have given the sanction of their names, to be more of an encumbrance than an illumination, we have not the remotest intention of undervaluing their labours. All real work is important, no genuine research is unworthy of our gratitude; but it is one thing to reverence power, and respect the work achieved, another thing to assign the nature and position of that work. With regard to the vast chemical researches into the subject of Food which have occupied a quarter of a century, it seems to me that their value has been almost exclusively *chemical*, and only in an indirect and limited degree *physiological.* Hence, in spite of the unanimity and apparent precision observable in the analyses and hypotheses offered by chemists, no important practical results have been attained, scarcely a single alimentary problem has been solved by them.

There may be readers who, failing to see the ground of this distinction between chemical and physiological investigations, will not understand the importance I attach to it; but they will perhaps come round to my point of view before this chapter reaches its close. The chemists, whatever we may think of them, will continue their labours, analysing, weighing, experimenting, and propounding hypotheses; and it is right they should do so: all honour and success to them! But if the question of Food is to receive any practical solution, it must no longer be left in their hands; or only such details of it left in their hands as properly belong to them. It must be taken up by physiologists, who, while availing themselves of every chemical result, will carry these into another sphere and test them by another Method. Not a step can the physiologist advance without the assistance of the chemist; but he must employ Chemistry as a means of *exploration,* not of *deduction*—as a pillar, not a pinnacle—an instrument, not an aim. The chemist may analyse fat for him; but he, on receiving this analysis, will request the chemist *not* to trouble him with hypotheses respecting the part played by fat in the organism; for although the chemist may accurately estimate the heat evolved in the oxidation of so much fat, the physiologist has to do with a vital laboratory, extremely unlike that in which the chemist works, and he has to ascertain how the fat comports itself *there.*

Alimentary substances are substances which serve as nourishment; but a great mistake is made when it is imagined that their nutritive value can chiefly reside in the amounts of carbon, nitrogen, hydrogen, oxygen, and salts, which they contain; it resides in the relation which the several substances bear to the organism they are to nourish.

Music is not harmonious to the deaf, nor is colour splendid to the blind. The substance which nourishes one animal affords no nourishment to another, nor will any table of "nutritive equivalents," however precise, convince us that a substance ought to nourish, in virtue of its composition, when experience tells us that it does *not* nourish, in virtue of some defective relation between it and the organism.

That "one man's meat is another man's poison" is a proverb of strict veracity. There are persons, even in Europe, to whom a mutton-chop would be poisonous. The celebrated case of the Abbé de Villedieu is a rare, but not unparalleled example of animal food being poisonous: from his earliest years his repugnance to it was so decided, that neither the entreaties of his parents nor the menaces of his tutors could induce him to overcome it. After reaching the age of thirty on a regimen of vegetable food, he was over-persuaded, and tried the effect of meat soups, which led to his eating both mutton and beef; but the change was fatal: plethora and sleepiness supervened, and he died of cerebral inflammation.[*]

In 1844, a French soldier was forced to quit the service because he could not overcome his violent repugnance and disgust towards animal food.

Dr Prout, whose testimony will be more convincing to English readers than that just cited, knew a person on whom mutton acted as a poison: "He could not eat mutton in any form. The peculiarity was supposed to be owing to caprice, but the mutton was repeatedly disguised and given to him unknown; but uniformly with the same result of producing violent vomiting and diarrhœa. And from the severity of the effects, which were in fact those of a virulent poison, there can be little doubt that if the use of mutton had been persisted in, it would soon have destroyed the life of the individual." Dr Pereira, who quotes this passage,[†] adds, "I know a gentleman who has repeatedly had an attack of indigestion after the use of roast mutton."

Some persons, it is known, cannot take coffee without vomiting; others are thrown into a general inflammation if they eat cherries or gooseberries. Hahn relates of himself that seven or eight strawberries would produce convulsions in him. Tissot says he could never swallow sugar without vomiting. Many persons are unable to eat eggs; and cakes or puddings having eggs in their composition, produce serious disturbances in such persons: if they are induced to eat them under false assurances of no eggs having been employed, they are soon undeceived by the unmistakable effects.

---

[*] *Journal de Médicine.* Août 1769, quoted by Lucas, *De l'Hérédité,* who is the authority for the next statement.

[†] Pereira: *Treatise on Food and Diet,* p. 242.

Under less striking forms this difference in the assimilating power of different human beings is familiar to us all; we see our friends freely indulging, with benefit instead of harm, in kinds of food which, experience too painfully assures us, we can eat only with certain injury.

To this fact the attention of parents and guardians should seriously be given, that by it they may learn to avoid the petty tyranny and folly of insisting on children eating food for which they manifest repugnance. It is too common to treat the child's repugnance as mere caprice, to condemn it as "stuff and nonsense," when he refuses to eat fat, or eggs, or certain vegetables, and "wholesome" puddings. Now, even a caprice in such matters should not be altogether slighted, especially when it takes the form of refusal; because this caprice is probably nothing less than the expression of a particular and temporary state of his organism, which we should do wrong to disregard. And whenever a refusal is constant, it indicates a positive unfitness in the Food.

Only gross ignorance of Physiology, an ignorance unhappily too widely spread, can argue that because a certain article is wholesome to many, it must necessarily be wholesome to all. Each individual organism is specifically different from every other. However much it may resemble others, it necessarily in some points differs from them; and the amount of these differences is often considerable. If the same wave of air striking upon the tympanum of two different men will produce sounds to the one which to the other are inappreciable —if the same wave of light will affect the vision of one man as that of red colour, while to the vision of another it is no colour at all, how unreasonable is it to expect that the same substance will bear precisely the same relation to the alimentary system of one man as to that of another! Experience tells us that it is not so.

A glance at the animal kingdom reveals the striking differences manifested by two closely allied organisms in their capability of assimilating the same substance. There are two species of Rhinoceros, the black and the white. The black species feeds on the graceful, but deadly, plant, *Euphorbia candelabrum*, and converts it into its own substance; but if the white species happen to eat thereof, it is inevitably poisoned. The Herbivora are divided into two classes, the first subsisting on a variety of plants, the second on one kind only. But even the various feeders will not touch certain plants eagerly devoured by others: thus the horse passes over almost all the cruciferæ; the ox all the labiates; goats, oxen, and lambs refuse almost all the solaneæ. The poisons are food to many; the rabbit devouring belladona, the goat hemlock, and the horse aconite. The dog will feed on bread, or biscuit, which his ancestor the wolf would starve rather than touch. The cat, although preferring animal food, will eat bread and milk, which the tiger will not look at.

We have brought these facts forward for the sake of giving distinct relief to the importance which must inevitably belong to physiological considerations in every question of Food; and to indicate the necessity of fixing our attention on the *organism to be nourished*, rather than on the *chemical composition of the substances* which nourish it.

When we are building a bridge, or making a machine, we can accurately guide ourselves by estimates of the strength of the wood and iron, because these substances do not lose their properties under new arrangements; but in building the mysterious fabric of the body, we have little or no guidance from our estimates of the properties of substances *out* of the body, because the body itself is an important factor in the sum, acting on the substances as well as being acted on by them, annulling or exalting their ordinary properties in a way quite peculiar to itself. And it is because this has been overlooked, or not sufficiently estimated, that our text-books are at once so precise, and so erroneous. Open almost any work on Physiology or Organic Chemistry, and you will meet with expositions of the theory of Food, and the nutritive value of various aliments, which are so precise and so unhesitating in their formulæ, that you will scarcely listen to me with patience when I assert that the precision is fallacious, and the doctrines demonstrably erroneous. Nevertheless I hope, before concluding, to convince you that Chemistry is itself in too imperfect a condition to give clear and satisfactory answers to its own questions in this direction—as Mulder and Lehmann frankly avow*—and further, that Chemistry, even supposing it to be perfect, must ever be incompetent to solve physiological problems, to which, indeed, it must always afford indispensable *aid*, without hope of doing more.

Vital processes depend on chemical processes, but are not themselves chemical, and cannot, therefore, be explained by Chemistry. There is something *special* in vital phenomena which necessarily transcends chemical investigation. We need not pretend to settle *what* vitality is, or on what the speciality of its phenomena ultimately rests, to be assured that it is something different from what goes on in laboratories, and demands other tests than those furnished by Chemistry. The philosophic poet warns us—

> "From higher judgment-seats make no appeal
> To lower;"

and such appeal, from higher to lower, is the appeal of Physiology to Chemistry. No analysis of a nerve will ever throw light on Sensibility; no arrangement of chemical formulæ will explain the form and properties of a cell. You may take a mechanism to pieces, and explain by physical laws the action and interaction of each wheel and chain; but you cannot take an organism to pieces, and explain its properties by chemical laws, such as are revealed in the laboratory. If an overwhelming illustration of this obvious truth be needed, we find it in the egg of an animal: here is a microscopic sphere, composed of substances well known to chemists, which contains potentially an animal, and which will reproduce not only the form, features, stature, and specific attributes of the parent animals, but also many of their acquired habits, tendencies, and tricks; has Chemistry, in the whole extent of its domain, anything analogous to this? Can Chemistry furnish us with even an approach to an explanation of it? Chemical analysis may conduct us to the threshold of Life, but at the threshold all its guidance ceases. There, a new order of complications intervenes, a new series of laws has to be elicited. Chemistry confesses its inability to construct complex organic substances, or even to say *how* they are constructed; it can, at present, only say *of what* they are constructed. This being so, it is clear that every attempt to explain chemically the nutritive value of any aliment by an enumeration of its constituents, must belong to what Berzelius admirably styles "the physiology of probabilities."

---

* MULDER: *Versuch einer Physiol. Chemie.* LEHMANN: *Lehrbuch der Physiol. Chemie*, 2d edit.

There is one cardinal rule which can never be violated with impunity, and which is, nevertheless, perpetually violated in our gropings towards the light. It is this: *Never attempt to solve the problems of one science by the order of conceptions peculiar to another.*

There is an order of conceptions peculiar to Physics, another peculiar to Chemistry, a third peculiar to Physiology, a fourth peculiar to Psychology, a fifth peculiar to Social Science. While all these sciences are intimately related, each has its sphere of independence which must be respected. Thus Chemistry presupposes Physics, and Physiology presupposes Chemistry; but physical laws will not alone explain chemical phenomena, chemical laws will not alone explain vital phenomena; nor, conversely, will Chemistry solve physical problems, nor Physiology solve chemical problems. In every vital process physical and chemical laws are implied, and the knowledge of these becomes indispensable; but over and above these laws, there are the specific laws of Life, which cannot be deduced from Physics and Chemistry.*

An illustration drawn from social science may serve to render this canon intelligible, and at the same time to uproot a widespread fallacy. Few errors have gained more general acceptance than that which declares the Family to be the perfect type of the State. This fallacy would have us regulate polity by domestic rules. A paternal government, in which the monarch is the head of the family; and a social government, in which all men are united as brothers, are the ideals of absolutists and socialists, who are pitiless in scorn of all other political schemes. When we see how a well-conducted household is harmoniously governed, each member fulfilling his proper office, and each assisting all; when we see how the farmer administers his affairs without any one to question his absolute will; the idea of so managing a nation naturally suggests itself; for, What is a nation but an extension of the family? ask the theorists.

---

* What is said in the text is by no means to be interpreted as indicating belief in the so-called "Vital force;" but the question will be fully discussed in the final chapter of this work.

I answer, the Family is specifically different from the Nation: it is no type of the State, because, not to mention other points, it has the bond of personal affection, and the bond of personal interest, which two puissant influences can never operate to anything like the same extent on the State. The father dearly loves his children, and his despotism may be absolute because it is truly paternal: his tender vigilance and forgiving love will soften all the harshness of absolute rule. But no philanthropist will be romantic enough to expect that king or kaiser can by any possibility feel this affection for his subjects; and thus one *essential* element of the family disappears. Again, the father's personal interest is bound up with his administration (as the farmer's is), and every false step he makes will be made feelingly evident to him. But the sovereign's personal interest is not in any such manner directly bound up with the goodness of his administration; if he can keep secure upon his throne, if neither revolutions nor assassins are provoked, it can make little difference to his welfare that the streets are filled with lamentations, and the battle-field with corpses. And even supposing him to be tender-hearted and conscientious, really desirous of the good of his subjects, yet his own personal interest is not so directly and obviously bound up with theirs as that of the father is with his household. Thus, on the supposition that the despot is the best and wisest of men, and his subjects are really desirous of universal brotherhood (two rather extraordinary assumptions always quietly made), the Family could offer no proper type of the State, because the two most puissant elements in the Family must be wanting in the State.

The application of the canon just laid down is easily seen: while, on the one hand, the Family must necessarily enter into the State, which is in truth an aggregation of families, it can never furnish the typical laws for the State, because the actions of *individual* men cannot be the standard for the actions of *masses*, and the mere aggregation of families brings about such a complication of interests, passions, and opinions, that a totally new set of relations is evolved.

Thus precisely as Polity presupposes Domesticity, but is not embraced by it—precisely as the State is dependent on the Family, and is, nevertheless, belonging to a higher jurisdiction, so does Physiology presuppose Chemistry, but is not included in it, cannot be wholly regulated by its laws. Domestic life furnishes the basis for political life, as chemical actions furnish the basis for vital actions.

Whatever the future progress of Chemistry may effect in the way of simplifying physiological problems (and no one doubts that it must greatly aid us), there is one radical distinction which must ever keep the two sciences separate. It is this: Chemical laws are *quantitative*, because chemical actions are *definite* combinations; whereas physiological laws can never become quantitative, but only *qualitative,* because vital substances are *indefinite* in composition; that is to say, while chemical substances are formed by combinations of unvarying quantities, never more, never less—so much acid to so much base always forming the same salt, so many atoms of one substance always uniting with so many of another to form a third; the substances on which vital actions specially depend are never precisely and accurately definite: they vary in different individuals, and at different ages of the same individual; and as every variation in composition necessarily affects the properties of each substance, it is impossible that physiological actions can be reduced to those exact quantitative formulæ on which Chemistry is founded. Chloride of sodium is the same substance, having precisely the same composition and properties, whether taken from the sea, from the earth, from the plant, or manufactured in the laboratory. But nerve-tissue is never *precisely* the same in two men; the blood of two men is never precisely alike; the milk of two women is never identical in composition—they vary (within certain limits), and sometimes the variation is considerable. It is on this that depends what we call the difference of "temperament," which makes one twin so unlike his brother, and makes the great variety of the human race.

If in this digression the reader's assent has been gained, he will see that, from the radical incompetence of Chemistry to settle any true physiological question whatever, all the laborious efforts of later years have been barren, or nearly so, as regards the important subject of Food, because they have been only chemical reasonings on Physiology. Plausible and brilliant as some of the theories have been, they are all at fault when reduced to practice. They have gained general acceptance, because of the simplicity with which they *seemed* to solve abstruse problems; and the human mind is so eager to have explanations, that any logical plausibility is sure to captivate it.

II. LIEBIG'S CLASSIFICATION OF FOOD.—Of all current hypotheses on this subject, none claims a closer scrutiny than that which Liebig has made familiar to all Europe, and which, winged by the two qualities of simplicity and plausibility, has been carried into the lecture-room and study, where it continues to hold its place, in spite of the growing conviction that it is untenable.

To render the classification intelligible to those unfamiliar with chemistry, it is needful to premise that organic substances are chiefly composed of the four elements: oxygen, hydrogen, carbon, and nitrogen. Some few of these substances—such as the essential oils—contain only carbon and hydrogen. A far greater number, including the fixed oils, starch, sugars, vinegar, and alcohol, contain oxygen as well as carbon and hydrogen. One of the chemical peculiarities of the starchy, or saccharine, group is, that these substances all contain oxygen and hydrogen in exactly the proportions which would form water; and, although it is not supposed that these elements exist under the form of water in these substances, the chemists have named this group the *hydrates of carbon,* or *carbo-hydrates.* Besides these two classes there is a third, which contains nitrogen, in addition to the oxygen, hydrogen, and carbon. This group includes the seeds of plants and the tissues of animals. The amount of nitrogen in these substances is small compared with the amount of carbon: nevertheless the mere presence of nitrogen has caused them to be distinguished as the *nitrogenous* group; while the other classes are placed together as the *non-nitrogenous,* or *carbonaceous,* group.

Bearing this in mind, we shall understand Liebig's division of Food into two classes:

The first is *Plastic,* or tissue-making, and comprises those organic substances into the composition of which nitrogen enters largely. These *nitrogenous* substances are—1°, *Vegetable* Albumen, Fibrine, and Caseine; and, 2°, *Animal* Flesh and Blood. They are said to be plastic, or tissue-making, because they are capable of being converted into Blood; and from the Blood all the tissues are formed. According to this hypothesis, no other substance has any *nutritive* value; but inasmuch as many other substances are employed for Food, there must be some purpose served by them. Liebig groups them together in his

Second class: the *non-nitrogenous* or *heat-making* substances. These substances are the Fats, Gums, Sugars, Starch, Pectine, Bassorine, Wines, Beers, and

Spirits. They are all assumed to be incapable of forming Blood, or of entering into the composition of any tissue, consequently they cannot nourish, or build up the body. But they are all capable of being "burned" in the body, and thus they furnish the needful animal heat, without which no organic function could be performed. When these substances are said to be "burned," we are to understand that burning here means simply *oxidation*—or the union of the oxygen derived from the atmosphere during Respiration, with one of these non-nitrogenous substances in the blood. The oxidation (rusting) of iron, and the burning of a candle, are one and the same process, differing only in degree. The union of oxygen with fat, is, in like manner, said to *burn* it. And because this oxygen is furnished in Respiration, all the articles of food which are burned by it are called *Respiratory, i.e.*, supporters of the respiratory process; and because in their burning they evolve heat, they are called heat-making.

Besides these two main classes of nitrogenous, or nutritive, and non-nitrogenous, or heat-making, there is also a class of *inorganic substances*, such as Water, Salts, Iron, &c., which we shall hereafter see to be truly entitled to rank as Food.

The classification just expounded is certainly a brilliant effort of genius, and has given an immense impulsion to science. Indeed *that* may be said of all Liebig's views: right or wrong, they have agitated Europe, and inspired laborious research. It is not wonderful that he should have missed the truth on this subject, for the subject is far too complex to be embraced by any chemical hypothesis; but the gratitude of Europe is due to him (and is freely given) for having rendered the solution of the problem possible. There where he found a wilderness, he has left a pathway. On this pathway thousands of explorers are now hurrying; he has made it easy for them to find new ways.

Neque ego illi detrahere ausim,
Hærentem capiti cum multa laude coronam.[*]

In rendering this tribute to Liebig's services, I must also explicitly declare that the classification he has proposed seems to me founded on a misconception. It has been much criticised,[†] sometimes with more asperity than befits the calm heights of science. It has made the tour of Europe, and become stereotyped in lectures and text-books. It has caused a vast amount of experimental research, which would have been more valuable had it been better directed. And it is now, more or less explicitly, relinquished by all advanced physiologists.

" It is indeed upon the assumption of this broad and fundamental classification of the constituents of food," write Messrs Lawes and Gilbert, " according to their varied offices in the animal economy, that a vast series of analyses of foods have of late years been made and published; whilst, founded upon the results of these analyses, numerous tables have been constructed, professing to arrange the current articles of diet both of man and other animals, according to their comparative values as such."*

---

[*] HORACE.

[†] See MULDER: *Physiologische Chemie.* MOLESCHOTT: *Kreislauf des Lebens;* and ROBIN et VERDEIL, *Traité de Chimie Anatomique.*

The question is one of very great importance, and the reader's patience is requested for an examination of it. So long as we place ourselves at the *chemical* point of view, Liebig carries us along with him; but no sooner do we perceive that we are dealing with a vital problem, not a chemical problem, than the necessity of abandoning the chemical and taking up the *physiological* point of view is at once evident, and we then quit our ingenious guide.

Let us state the question. Man requires food which is both tissue-making and heat-making, to repair the fabric, and sustain the temperature of his body. This much is true. But it is demonstrable that nitrogenous substances are *not* the only plastic materials, not even the chief materials, whereas they *are* also heat-producing. Conversely, it is demonstrable that non-nitrogenous substances *are* tissue-making as well as heat-producing; so that any distinction between them, founded on their supposed offices nutritive and respiratory, falls to the ground; not to mention that it rests on the assumption of Respiration being the source of Animal Heat—an hypothesis we shall hereafter have to consider.†

The division of Food into Nitrogenous and Non-

---

* *Report on Foods in Relation to Respiration and Feeding*, in *Reports of Brit. Assoc.* 1852.

† See the Chapter WHY WE ARE WARM, AND HOW WE KEEP SO.

Nitrogenous is a chemical division to which no objection need be made, for it expresses a chemical fact. But when the fact that albuminous substances form a necessary proportion of organised tissues, is made the ground for specially distinguishing them as plastic ; and when the presence of nitrogen in these substances is made the ground for specially distinguishing nitrogen as *the* plastic element, the per-centage of which is to afford the standard of nutritive value, we see a striking example of chemical reasonings applied to Physiology, which a confrontation with nature suffices to upset. For observe : while it is true that "albumen is the foundation, the starting-point of the whole series of peculiar tissues which are the seat of vital actions" (LIEBIG)—while it is true that the *peculiar* characteristic of organised tissues is that they contain albuminous substances as necessary ingredients ; not less is it true that the *other* substances, thus arbitrarily excluded from the rank of tissue-makers—namely, the fats, oils, and salts, all destitute of nitrogen—are *as essential* as albumen itself. *Not a cell, not a fibre can be formed, nor can subsist, without a certain amount of fat and salts.* Not a tissue can come into being, nor continue its functions, without a large proportion of non-nitrogenous materials—a proportion greatly *exceeding* that of the nitrogenous.

This is an anatomical fact which must surely discredit the idea of selecting one element out of several, all indispensable, and assigning to it alone the character of nutritive. If tissues were composed of albumen, or any other nitrogenous substance, *without* the admixture of fats, water, and salts; and if albumen did not likewise disengage heat in its transformations, Liebig's classification would be strictly accurate; but in the face of anatomical evidence which shows that *no* such tissue exists, and in face of the physiological evidence that even albumen undergoes chemical changes accompanied by the disengagement of heat, the classification must be rejected. Indeed the anatomist must ask with surprise, whether what he calls the adipose tissue is, or is not, chiefly composed of fat? Is the fat which exists in the muscles, cartilages, and bones an accident—a thing not worthy of being taken into account? The answer cannot be dubious. In 100 parts of muscle there are only 25.55 parts solid matter, and of these no less than 4.25 are fat. In 100 parts of the white substance of the brain, fat bears the large proportion of 13.9, whereas albumen is only 9.9; in the grey substance of the brain, the proportion of fat is 4.7 to albumen 7.5. If after this it be said that fat does not help to form tissue, is not an essential integral element of tissue, and consequently *plastic*, in the most rigorous sense of the word, the anatomist must confess that he fails to understand the language employed.

The reader need not be informed that Liebig is fully aware of the facts which can be brought against him, and that if he errs it is from theoretic bias. He is a chemist, and views these questions in their chemical light. To the chemist muscle is not a compound substance; for all those substances over and above albumen which he finds in it, he regards as mere *accessories;* the essential and characteristic element for him is the albuminous substance. But to the anatomist *all* the substances are essential, because it is by their *united* properties that the muscle performs its peculiar offices. A muscle deprived of its water will no longer act *as* a muscle: the anatomist therefore regards water as *essential* to the muscle; the chemist regards it as an accessory. In the laboratory the water may be disregarded: it will not interfere with chemical reactions. In the organism, the water is just as essential to the functional activity of the muscle as the albumen itself. Liebig, however, regards water and fat as having only a physical influence:—

"Many of the physical properties of organs, or tissues, depend on the presence of their non-nitrogenous constituents—namely, of water and fat. These bodies assist in the changes and processes by which the organised structures are formed. Fat has a share in the formation of cells; and on water depends the fluidity of the blood, and of all other juices. So also the milk-white colour of cartilage, the transparency of the cornea (of the eye), the softness, plasticity, flexibility, and elasticity of muscular fibre and of membranes, all depend on a fixed proportion of water in each case. Fat is a never-failing constituent of the substance of the brain and nerves; hair, horn, claws, teeth, and bones, always contain a certain amount of water and fat. But in these parts water and fat are only mechanically absorbed, as in a sponge, or enclosed in drops, as fat is in cells, and they may be removed by mechanical pressure, or by solvents, *without in the least affecting the structure of the parts.* They never have an organised form peculiar to themselves, but always take that of the parts, the pores of which they fill. They do not therefore belong to the plastic constituents of the body or of the food."* A little further on he repeats the statement that "they take no direct share by their elements in the formation of organs," and that they have "no vital properties."

By these expressions it is clear that, in his view, organs are not formed out of fat and water, *as well as* out of albumen; but that these substances are merely accessory, and afford the requisite physical conditions; just as, when we use chloride of potassium, the water in which the salt is dissolved counts for nothing in the chemical agency of the salt. And from several conversations I had with this distinguished philosopher, in which he stated his point of view, I am persuaded that this is the explanation of all the differences which exist between him and those physiologists who oppose his hypothesis. My answer then was, what it is now—namely, that to the anatomist, nerve-tissue without fat is no longer vital nerve; and blood without water is no longer vital blood. To suppose that water simply gives fluidity to blood, when in truth it is as much an integral constituent of blood as albumen itself, is equivalent to saying that heat only gives expansion to steam—steam itself being, as all know, the product of the operation of heat on water. If fat has no vital properties in itself, neither has albumen in itself. To say that fat and water are "mechanically absorbed," is to contradict anatomical evidence, which shows them to be *structurally combined,* and always in constant quantities, varying within very small limits.

---

* LIEBIG: *Chemical Letters,* 1851, p. 346; ed. 1859, p. 379. As these sheets are passing through the press, a fourth edition of the *Chemical Letters* has appeared, but without any modification of its views on the subject having been produced by the criticisms to which they have been subjected. For convenience of readers I shall give double references.

A classification of Food, more or less imperfect, would not trouble us did it not lead to important errors, as in the present case. No sooner do we accept the idea of nitrogenous food being *the* plastic material, than we are landed on the proposition that "only those substances are in a strict sense nutritious articles of food, which either contain albumen, or a substance capable of being converted into albumen"*—a proposition elsewhere expressed in even a cruder form: "Only nitrogenous substances are capable of conversion into blood."† Such passages as these are only intelligible, coming from so eminent a writer, when we remember that the chemical point of view dictates them. To the chemist, indeed, only nitrogenous substances are capable of conversion into Blood; but to the anatomist every substance which enters into the normal structure of Blood must be reckoned among those capable of conversion into it. He cannot separate one part of the Blood from another, as the chemist does—he must take the whole structure as he finds it, for it is with the whole structure that the functions are performed. Now what does he find? Examination of the structure of the Blood shows that, so far from being composed exclusively of nitrogenous substances, it is composed of a variety of substances, among which the nitrogenous albumen and fibrine amount to less than 80 in 1000 parts, including what must be added for the globulin and hæmatin of the blood-discs: that is *all* the nitrogen in the blood said to be composed solely of nitrogenous substances. No one knows this better than Liebig himself; yet his argument entirely overlooks it.

---

* LIEBIG: *Chemical Letters,* 1851, p. 346; ed. 1859, p. 371.

† Ibid., p. 497; ed. 1859, p. 506.

Let it not be supposed that this discussion is without a practical bearing: the whole question of Food depends on its decision. The chemical point of view is incapable of directing us to a single rule in practice: and to show how entirely it fails we may be content with one fact: The very substances said to be alone capable of conversion into blood—the only "strictly nutritious substances," are, when taken alone, utterly unable to nourish.

"Muscular flesh," says Majendie, in the celebrated Report of the Gelatine Commission, "in which gelatine, albumen, and fibrine are combined, according to the laws of organic nature, *and where they are associated with other matters, such as fats, salts, &c.,*

suffices, even in very small quantity, for complete and prolonged nutrition. Thus dogs fed for 120 days solely on raw meat, from sheep's heads, preserved their health and weight during this period, the daily consumption never exceeding 300 grammes, and often less. But 1000 grammes of isolated fibrine, with the addition of some hundred grammes of gelatine and albumen, were insufficient to support life. What, then, is this peculiar principle which renders meat so perfect an aliment? Is it the odorous and sapid matter which has this function, as seems probable? Do the salts, the trace of iron, the fatty matters and the lactic acid, contribute to the nutritive effect, notwithstanding that they constitute so minute a portion of meat?"*

The minuteness in quantity would be no argument against their potency of influence; but far more important will be the state of combination of the various elements. "The albumen of egg, and the fibrine separated from the blood, may to the chemist be identical with the fibrine and albumen which concur in the formation of muscle, incorporated there by a process of nutrition; but they are not the same for the organism which has to assimilate them, and which requires that they should be in a special state of elaboration, which they have undergone in another organism; it is muscular flesh which the organism demands, and not the elements of which flesh is composed: it needs aliments not chemical products."†

---

* Quoted by PEREIRA: *Treatise on Food and Diet*, p. 241.

† LEVY: *Traité d'Hygiène*, ii. 85, quoted by LONGET, *Physiologie*.

It has been found that dogs perish of starvation when liberally supplied with albumen, or with white of egg, or with fibrine, or with mixtures of albumen and fibrine—if these substitutes constitute their sole diet—whereas they flourish when fed on gluten alone, although, according to the chemists, gluten is identical with albumen and fibrine: a sufficient proof that the nutritive value of a substance cannot be determined by its chemical composition.

But this kind of proof awaits us on all sides. While Chemistry determines the nutritive value of foods according to their per-centage of nitrogen, experience flatly contradicts the application of such a standard, for it shows us that wheat contains only 2.3 per cent of nitrogen, whereas beans contain as much as 5.5 per cent, lentils 4.4, and peas 4.3; and yet, with this remarkable inferiority in its per-centage of nitrogen, wheat is remarkably superior in nutritive value to beans, lentils, or peas. The discrepancy here is so glaring that Liebig has attempted to explain it. "The small quantity of phosphates which the seeds of the lentils, beans, and peas contain *must* be the cause of their small value as articles of nourishment, *since* they surpass all other vegetable food in the quantity of nitrogen which enters into their composition. But as the component parts of the bones (phosphate of lime and magnesia) are absent, they satisfy the appetite without increasing the strength."[*] The argument might be reversed, and the whole nutritive value assigned to the phosphates with equal justice. If nitrogen is *the* plastic element, and its per-centage afford the true nutritive standard, the presence or absence of the phosphates can have nothing to do with it; and if their presence or absence is all-important, then we are certain that nutritive value does not admit of being estimated by the per-centage of nitrogen, but by the conjunction of nitrogen with other substances.

---

[*] LIEBIG: *Chemistry in its Application to Agriculture and Physiology*, p. 147.

It is noticeable that when Liebig has to explain the nutritive inferiority of beans and peas, he finds the cause to lie in the absence of phosphates, which, as he truly says, are component parts of the bones; whereas, when denying any nutritive quality to fat, he refused to admit that *it* was a component part of tissues. Into such contradictions he is forced by his theory of nitrogenous substances being the only plastic materials—a theory incessantly at variance with fact.

Messrs Lawes and Gilbert call especial attention to one series of their experiments, in which sheep fed on succulent unripe turnips "lost weight notwithstanding the *very high* per-centage of nitrogen;"* and, without laying any stress on the fact that vegetable poisons are highly nitrogenous, let us ask the dispassionate reader to reflect on the chaotic condition of a doctrine which, while proclaiming nitrogen to be the true standard of nutritive value, declares that gelatine, a substance richer in nitrogen than even flesh or blood, has no nutritive value at all. We do not, indeed, attach much credit to this opinion, which we shall examine by-and-by, but it is certainly in flagrant contradiction with the chemical hypothesis of nutritive values.

---

* *Report*, p. 336.

In spite, therefore, of what is so confidently asserted, we have the admission of chemists themselves that nitrogen is only nutritive in peculiar combinations. The consequence is inevitable. We must direct our attention towards substances which *do* nourish, and disregard the chemical formulæ which proclaim what substances *ought* to nourish. Inquiries so directed yield little that is satisfactory to the chemical hypothesis.

We find, for example, thousands of Irish subsisting chiefly on potatoes and skimmed milk; and millions of Hindoos subsisting chiefly on rice and rancid butter—substances which, in a chemical analysis, exhibit very little of the so-called plastic material. Payen gives the following proportions in 100 parts of rice:—Starch, 89.15; Nitrogenous matters, 7.05; Dextrine, &c. 1.; Fats, 0.80; Cellulose, 1.10; Mineral matters, 0.90. And Liebig himself calculates the proportion of plastic to non-plastic material in rice, as only 10 to 123; whereas in beef it is 10 to 17, and in veal 10 to 1.

Respecting the rice-eating Hindoos, it may be observed that although Dr Forbes Watson's elaborate investigations have dispelled the very general notion of rice forming the *entire* bulk of the diet of the inhabitants of the East, they also give an unequivocal refutation of the chemical theory of Food.

In the work on the "Food Grains of India," which is at this moment passing through the press, Dr Watson remarks that "the stomach of the poor Ryot, large as it is, could not with impunity take in and digest the quantity of rice which would be sufficient day by day to supply the wants of his system."* In a private communication he informs me that where rice is cheap, as at the deltas of rivers and in other water-abounding situations, it forms the *chief* food of the inhabitants; although disease ensues unless a certain proportion—say $\frac{1}{4}$ to $\frac{1}{8}$ of *pulse*, or dried fish, be added to supply the required nitrogenous elements. In some parts where rice forms almost the entire food of the population, "pot bellies" and the results of starvation are visible in every direction. In all cases rice, when procurable, forms two-thirds of the food, and may therefore be considered the staple food of a considerable population. "Nature, however, has taught the native to add pulses, or flesh, to his rice—just as she has suggested to the Irishman the free use of buttermilk to his potatoes."

The facts are important. While admitting the proved nutritive quality of nitrogenous food, such as the pulses, I am far from clear that the Hindoo is forced to mix them with his rice merely on account of the nitrogen they contain. The fact that rice alone is insufficient to the effective sustainment of the organism, and that "pot bellies," diseases, and even signs of starvation are the consequences of its exclusive employment, is only one among a series of facts which prove that man cannot flourish on *one* kind of food. Pulses are even worse than rice when eaten alone. Albumen itself, nay, milk itself, would not suffice alone. We are justified, therefore, in assigning this effect to some other cause than the mere deficiency in nitrogen ; and we are at all events certain that rice suffices to sustain the waste and repair of the organism, if not in a very admirable manner. And Dr Watson further informs me that the other grains of the Hindoos are all highly carbonaceous ; although the native, as a rule, combines these, *when he can*, so as to make a food which contains about eight or nine parts of carbon to one of nitrogen—which Dr Watson regards as the proper standard for tropical *as well* as temperate climates.*

---

* WATSON: *The Food-Grains of India*, 1859, p. 25.

Now let us observe the bearing of these facts on the chemical theory of Food. Rice is confessedly extremely deficient in plastic, nitrogenous, elements ; abundant in heat-making, carbonaceous, elements. Rice is therefore among the very substances which, on the chemical hypothesis, the Hindoos would seem least to require.

We are told that non-nitrogenous matters are incapable of entering into the composition of tissues, or of furnishing plastic material : "they only serve to keep up the temperature of the body, being rapidly burnt in the body." We are further told that the demand for such substances is necessarily much greater in cold than in hot countries, because of the greater amount of heat required to keep the body at its proper point.

---

* LIEBIG'S calculation of a standard food is four parts of non-nitrogenous to one part of nitrogenous matter.—*Chemical Letters*, 1859, p. 391. Dr WATSON finds the heat-making elements double this quantity—and in a hot climate too.

"In winter, when we take exercise in a cold atmosphere, and when, consequently, the amount of inspired oxygen increases, the necessity for food containing carbon and hydrogen increases in the same ratio; and by gratifying the appetite thus excited, we obtain the most efficient protection against the most piercing cold. The oxygen taken into the system is given out again in the same form, both in summer and winter: we expire more carbon at a low than at a high temperature, and require more or less carbon in our food in the same proportion; and consequently more is respired in Sweden than in Sicily; and in our own country, an eighth more in winter than in summer. If an equal weight of food is consumed in hot and cold climates, Infinite Wisdom has ordained that very unequal proportions of carbon shall be taken in it. The fruits used by the inhabitants of southern climes, do not contain, in a fresh state, more than 12 per cent of carbon, while the blubber and train-oil which feed the inhabitants of the polar regions, contain 66 to 80 per cent of that element."*

Considering the importance of the idea, one cannot but be struck with the singular meagreness of these illustrations. That fruits are eaten in southern climates which are less heat-producing, because they contain much less carbon, than the train-oil eaten in polar regions, would be a tolerable argument, if *only* fruits were eaten in the South; but the Sicilian and Neapolitan eat more oil than the Swede, and their macaroni is a highly carbonised substance; and the Hindoo often subsists on rice and butter—substances highly carbonised, and classed as chiefly Respiratory, furnishing in superabundance that very heat which his climate renders so undesirable.

---

* LIEBIG: *Chemical Letters*, 1851, p. 320; ed. 1859, p. 347.

According to *theory*, the Hindoo should eat very little non-nitrogenous food, and be content with plastic substances, since he wastes his tissues in daily labour, but does not stand in need of any surplus heat; whereas, according to *fact*, he eats very little nitrogenous food, and a great deal of "heat-making" food.

It is remarkable that such a contradiction to the chemical theory should have been overlooked, especially by those who laid so much stress on the large consumption of nitrogenous food by the inhabitants of cold countries—a consumption said to be caused by the need of rapid combustion to sustain the animal heat. For my own part I see no reason whatever to believe in the current hypothesis that food is "burned" in the organism; and elsewhere (see chapter on "Why we are Warm"), will give reasons for that dissent, and for believing that it is the tissues which are burned (if burning there be) and not the food itself. Waiving this objection, however, for the present, let us consider merely the important conclusion to which Dr Watson's researches have led him, namely, that the proper standard, chemically speaking, is the same for tropical and for temperate climates.

And this damaging fact is brought into even greater relief by the experiments of Messrs Lawes and Gilbert, as thus recorded by them: "The weather, during part of the period of this second series of experiments, was exceedingly hot; from this several of the animals suffered considerably; and some, either from this or other causes, became quite ill, and died, or were 'killed to save their lives.' Nevertheless it is seen that there was upon the whole *a larger amount of respiratory food consumed* in relation to weight in this series than in the previous one, during the cooler season."* Against such evidence as this, the respiratory nature of non-nitrogenous food is more than equivocal.

It is a fact that, in cold countries, fat and oil are greedily devoured; and it is the most striking fact that can be adduced in favour of the hypothesis now under discussion. But we have yet to learn that fat is simply so much "combustible" material. The demand for fat in cold countries may arise out of various conditions. Increase of cold causes increased activity of respiration, and increased activity of muscular exertion. These cause a greater waste of tissue; consequently increased repair is needed; and as fat is indispensable to such repair, we can therein see one source of the demand for fat.

The reader will bear in mind, that we are not in *this* place disputing the position that fat is burned in the body, or that it is not one important source of animal heat; the point disputed is, whether fat is *only* a heat-producer, and the demand for it in cold countries *only* a demand for combustible material. On this point it is well worthy of remark, that Schmidt's researches prove *fat to be less easily combustible in the organism than the carbo-hydrates, and even than albuminates;*[*] so that the Hindoo, in his rice, eats a substance more easily oxidisable in the organism than the tallow eaten by the Esquimaux.

---

* *Report*, p. 340.

What has been already said will perhaps suffice to show how untenable a position is that which denies nutritive value to fats, sugars, starch, water, &c., throwing the whole burden of nutrition on the albuminous substances; it may complete the overthrow of that position if I now show that, while the fats are tissue-makers and heat-producers, the albuminates are heat-producers and tissue-makers.

No one doubts that heat is evolved in the chemical changes which albuminates undergo; the doubt raised can only be as to the amount. Liebig says: "If the combustible elements of the plastic constituents of food served for the production of heat, the whole amount of the substances consumed by the horse in his hay and oats, by the pig in its potatoes, could only suffice to support their respiratory process, and consequently their animal heat, in the horse for 4½ hours daily, in the pig for 4 hours daily; or if confined to plastic food, they would require to consume five or six times as much of it. But even in this last case it is exceedingly doubtful whether these substances, considering their properties, would in the circumstances under which they are presented to oxygen in the organism produce the necessary temperature of the body and compensate for the loss of heat; for of all organic compounds, the plastic constituents of food are those which possess in the lowest degree the properties of combustibility, and of developing heat by their oxidation." *

---

[*] See on this point BIDDER and SCHMIDT, *Die Verdauungssäfte*, p. 360-63, and LEHMANN, *Lehrbuch der Physiol. Chemie*, 2d edit. vol. iii. pp. 203, 386.

Every chemist would echo this statement, because Chemistry teaches that of all the elements of the animal body nitrogen has perhaps the feeblest attraction for oxygen; not only so, but it even deprives other substances, with which it combines, of their tendency to unite with oxygen. Phosphorus, for example, has an eager affinity for oxygen, as we know from its ready combustibility in atmospheric air at ordinary temperatures; but when combined with nitrogen its combustibility is so difficult that it can only be effected at red heat and in oxygen gas. Liebig hence concludes, and from the chemical point of view is justified in concluding, that precisely the same relations are preserved in the blood. The albuminous (nitrogenous) bodies have, he says, but a very slight affinity for oxygen. "If the albumen of the blood, which is derived from the plastic portion of the food, possessed in a higher degree the power of supporting respiration, it would be utterly unfit for the process of nutrition. Were albumen as such destructible, or

---

* LIEBIG: *Chemical Letters*, p. 372; ed. 1859, p. 394.

liable to be altered in the circulation, by the inhaled oxygen, the relatively small quantity of it, daily supplied to the blood by the digestive organs, would quickly disappear. As long as the blood contains, besides albumen, other substances which surpass it in attraction for oxygen, so long will the oxygen be unable to exert a destructive action on this the chief constituent of the blood; and the significance of the non-nitrogenous food is thus made clear."

It is not surprising that a theory so logical should have gained general acceptance; and as a specimen of chemical reasoning on physiological problems it is very brilliant. Nevertheless, when we study what takes place in the organism, we find direct and unequivocal contradiction given to each separate clause of the theory. We find races of men living always on vegetable food, containing little nitrogen, and in climates where a superabundance of animal heat is not needed; so that to them non-nitrogenous food must be sufficient for the chief supply of nutrition.

And not only do numerous facts overpower the chemical hypothesis, but even Chemistry itself, when interrogating the facts of organic life, discovers that, however weak the affinity of albuminates for oxygen, *out* of the organism, their affinity, *in* the organism, surpasses that of fat. Schmidt, to whose experiments science is so deeply indebted, found that on feeding cats now with flesh alone, and now with fat alone, or with much fat and little flesh, the *albuminates* *were always more rapidly destroyed than the fat*, which was at first stored up in the body to be afterwards gradually oxidised;* and these experiments are confirmed by those of Persoz in fattening geese with maize: the blood of the fattened geese was very rich in fat, but notably impoverished in albumen; the quantity of muscular substance was much diminished, and when the fattening was rapid the weight of the whole body was absolutely diminished. It is therefore an error to say that if albumen were liable to oxidation, life would be impossible. Experiment proves it to be more liable than fat.

To the chemist these results will be paradoxical, if not inconceivable, and he will doubtless point to the well-ascertained fact that in starvation it is the fat which disappears first, the muscles only yielding up their elements to destruction when most of the fat has been oxidised. This point has already been dwelt on by us when treating of HUNGER and THIRST. All that can here be said is, that it needs to be reconciled with the seemingly contradictory facts. How slow we should be in drawing conclusions from what takes place out of the organism, as to what takes place in it, is taught us in a hundred physiological facts: thus the fat which can be decomposed into fatty acid and glycerine by means only of the most energetic acids and alkalis in the laboratory, is changed in the organism by the pancreatic juice, which has but feeble chemical properties, but which brings about the result by means of an organic substance acting as a ferment.[*]

---

* BIDDER and SCHMIDT, p. 363.

We might multiply to a great extent the objections which present themselves to the chemical classification of Food; but those already stated are sufficient to show that it is erroneous in every particular, in spite of its logical dependence and plausibility.

The only extensive series of experiments on feeding, with which we are acquainted, as immediately serviceable, are those instituted by Messrs Lawes and Gilbert, and to them the reader is referred; because, although they are by no means such as, from the nature of the experiments, can give Physiology any accurate data, they are valuable as practical results. They show among other things, that "although pigs were satisfied to eat a smaller proportion of food in relation to their weight, in those pens where the proportion of nitrogen was comparatively large, yet the proportion of increase to the food consumed was *less* than where the amount of *non*-nitrogenous food consumed was greater." And further, that "whilst the non-nitrogenous substance consumed to produce 100 lb. increase in weight is *very nearly equal* in the two series; yet that of the nitrogenous constituents varies in the proportion of from three to two!" Again: "In the fourth pen, where there was by far the *largest amount of nitrogen* consumed, the animals *lost weight;* and in the other three pens, the productiveness of the food is in the *inverse order of the*

---

[*] CLAUDE BERNARD: *Leçons de Physiologie Expérimentale. Cours de* 1854-55, p. 391.

amounts of nitrogen taken in the food. Indeed, we believe that an unusually high per-centage of nitrogen in succulent produce is frequently a pretty sure indication of immaturity and innutritious qualities." Summing up the results of their whole series of experiments, the largest yet instituted, they declare that it is "their available non-nitrogenous constituents, rather than their richness in nitrogenous ones, that measure both the amount consumed to a given weight of animal, in a given time, and the increase in weight obtained." * And they refer to the instinctive preference given by the under-fed labouring classes to fat meat, such as pork, over those meats which are leaner and more nitrogenous.

Long as we have tarried over this part of our subject, the time will not have been misspent if it have clearly impressed the conviction that nitrogenous food is *not* the exclusively plastic food, and that per-centages of nitrogen afford *no* nutritive standard—the conviction that Liebig's classification, except in a chemical point of view, is erroneous—and the conviction that Chemistry is incompetent to solve the problem of Food.

III. INORGANIC FOOD.—As soon as we relinquish the seductive notion of physiological deduction from chemical laws, and place ourselves at the proper point of view, namely, that of the organism to be nourished, our classification of Food speedily falls under two main divisions — Inorganic and Organic substances; and, doubtless to the reader's surprise, the Inorganic turns out to be the *more* important of the two, supposing always that a question of degree can lawfully be entertained where both kinds are indispensable.

---

* *Report*, p. 344. Compare also their *Report on the Equivalency of Starch and Sugar in Food.* British Assoc., 1854.

We are not, indeed, accustomed to consider minerals as food, or water as highly nutritious; but that is because we are not accustomed to consider the subject with the needful accuracy. Tell the first man you meet that water is on the whole more nutritious than roast-beef, and that common salt, or bone-ash, is as much an edible as the white of egg, and it is probable that he will throw anxious glances across the streets to assure himself that your keeper is at hand. Make the same statements to the first man of science you meet, and the chances are, that he will think you very ignorant of organic chemistry, or that you are playing with a paradox.* Nevertheless, it is demonstrably true, and never would have worn the air of a paradox, if men had steadily conceived the nature of an alimentary substance.

That is an aliment, which nourishes; whatever we find in the organism, as a constant and integral element, either forming part of its structure, or one of the conditions of vital processes, that, and that only, deserves the name of aliment.

But men have been seduced from this simple conception, partly by vain endeavours to ascertain in analysis of food and excreta what are the truly nutritive substances, and partly by misconceptions of the processes of Nutrition.

---

* "Minerals are not in the least alimentary, although many animals often eat them mixed with or combined with their food."—TIEDEMANN: *Physiologie*, i. 230.

Nutrition in Plants and Animals.—Of these misconceptions there is one, widely spread, which declares, that while Plants are able to nourish themselves directly by inorganic materials furnished them in the air, earth, and water, Animals are incapable of thus drawing nourishment from inorganic materials, but depend solely on the organic materials prepared for them by Plants. The Plants feed on minerals, the Herbivora feed on Plants, and the Carnivora on the Herbivora. The cycle is complete, the symmetry of nature seems perfect.

One feels a kind of pity in having to disturb so elegant a formula; yet the truth must be told, and the truth is, that not a single statement so expressed is altogether correct. Certain it is that Plants can, and do, convert inorganic substances into organic, but it is not less certain that this power is very limited, all except the simplest (perhaps not even these) needing organic principles to be yielded by the soil in which they grow. This destroys the distinction between Plants and Animals, by showing that both, more or less, depend on organic substances. It is this inability in Plants to dispense with organic matter, that renders manure necessary.[*]

---

[*] VERDEIL and RISLET have ascertained that all fertile soils contain a soluble organic substance resulting from the decomposition of vegetable matter.—*Comptes Rendus de la Société de Biologie,* iv. 111, 112.

While so much is certain, the general assumption is, that Animals are altogether incapable of converting any inorganic materials into organic; and are rigorously dependent on Plants for every organic substance met with in their bodies. This assumption seems to me wholly unwarranted by any decisive knowledge yet obtained. The main argument on which it rests, namely, that unless organic substances be given in the food, and in certain proportions, the animal perishes of starvation, has no longer any coercive force when we reflect that starvation as inevitably follows if inorganic substances be withheld.* Organic substances—of a low order it is true—have been manufactured by the chemist out of inorganic substances; and if urea, alcohol, and camphor are already capable of being made in the laboratory, I see no reason for supposing that even more complex substances may not be made in the vital organism, which is the seat of such incessant chemical transformations. †

Be this as it may, the distinction between Animals and Plants falls to the ground when we see that Plants *do* require organic substances, and that Animals *do* nourish themselves with inorganic substances taken directly from earth, air, and water. We hew salt from the quarry to cast it in handfuls upon our stews and soups, or in pinches on our meat and potatoes. We draw water from the spring to drink; and, like the plants, we draw gases (oxygen, perhaps also nitrogen) from the air, to enter into those various combinations without which no life is possible.

---

* The necessity for organic food will be better understood when we come to speak of Digestion, and the action of nitrogenous substances as ferments.

† LEHMANN is of the same opinion.—*Physiol. Chemie*, iii. 180.

It may be unusual to call these nutritive principles, but if unusual, it is not unscientific. If "to nourish the body" mean to *sustain its force* and *repair its waste*—if food enters into the living structure—and if all the integral constituents of that structure are derived from food—there can be nothing improper in designating as nutritious, substances which have an enormous preponderance among the integral constituents. People who think it paradoxical to call water Food, will cease their surprise on learning that water forms two-thirds of the living body; and they will perhaps cease to marvel at the nutritive value here attributed to minerals, on learning that when all the water is eliminated, and the solids which form the remaining one-third are analysed, they are found to contain no less than one-third of mineral substances which remain as ashes. Nor must the presence of these mineral substances be regarded as accidental or unimportant. They are constant, constituent, essential. Blood is not blood without its salts and iron; bone is not bone without its phosphates; muscle is not muscle without its salts.

Let us glance at one or two of these inorganic elements; and, first, at *phosphate of lime*. There is not a single humour, nor a single tissue in the body, which is without a certain proportion of this salt. By removing it, the integrity of the tissue is destroyed, an all characteristic properties are as infallibly altered a if the organic elements were removed. If the needful quantity be withheld or withdrawn, the bones become weakened, as we see in pregnant women, whose fractured limbs are with difficulty healed (sometimes not at all), simply because their phosphate of lime has been diminished by the demands of the child.

A similar effect is noticeable in infants during teething, a period when the "rickets" often make their dreaded appearance.

But still more fatal is the effect of withholding this salt from the food, as we learn in the striking experiments of Chossat, who withheld it from pigeons, allowing them to eat no more than was contained in the grain and water on which he fed them: they all perished miserably, after attacks of diarrhœa and softening of the bones.[*]

The absolute necessity of a supply of inorganic materials in Food is further illustrated in one of the experiments of Messrs Lawes and Gilbert, who note that "the pigs in the pen, where Indian meal alone was given, had become affected with large tumours breaking out on their necks, their breathing and swallowing becoming at the same time difficult. We, in order to test the question as to whether this arose from the defect of nitrogen or from other causes, supplied them with a trough of *mineral* substances: they soon recovered from their complaint, and eventually proved to be among the fattest and best of the entire series of pigs; at least a dealer in pork, with a practised eye, purchased, by preference, one of these animals from among the whole set of carcasses. The mineral mixture supplied to them was composed of twenty parts coal-ashes, four parts common salt, and one part superphosphate of lime; and for it they seemed to exhibit considerable relish." *

---

[*] Von Bibra found that the quantity of phosphate of lime contained in the bones was determined by their ratio of work; those of the legs and feet containing more than the arms and hands, and both these more than the ribs or the passive bones.

The point is forcibly put by Liebig:—"In the two preceding letters, there has been ascribed to certain constituents of seeds, tubers, roots, herbs, fruits, and flesh, the power of supporting the processes of nutrition and respiration; and it will appear as a very striking contradiction when it is stated, that no one of these substances by itself, neither caseine alone, nor the substance of muscular fibre, nor the albumen of eggs or of the blood, nor the corresponding vegetable products, are able to support the plastic or formative processes; that neither starch, sugar, nor fat, can sustain the process of respiration. Nay, it may excite still greater astonishment to add, that these substances, even when mixed, no matter in what proportions, are destitute of the property of digestibility without the presence of certain other substances; so much so, indeed, that if these other conditions be excluded, the above-named compounds are utterly unable to effect the continuance of life and the vital phenomena." *

---

* *Report*, p. 339.

He then proceeds to explain that these matters are the salts of the blood, and examines, with his usual acuteness when dealing with chemical phenomena, the part played by the alkalis in the nutritive process.

We must remark, however, that even here the absence of the true anatomical point of view renders his teaching incomplete; for he only takes into account the part played by the inorganic substances as *conditions* of vital phenomena (such as promoting digestibility and nutrition), entirely overlooking their part as integral *elements of tissue,* on which many of the properties of tissues depend.

It is from this mistaken view, we imagine, that he omits *water* from the list; yet anatomy assures us that water is an essential element of tissue; and its enormous preponderance in quantity is the expression of its pre-eminence in nutritive quality, and explains the paradox of water being, *longo intervallo,* the most sustaining of all articles. Life, we know, may be prolonged for weeks without any organic food being taken, if water be freely supplied; but life will not continue many days when water is withheld. If, therefore, the purpose of Food be to sustain the organism, that article which sustains it longest, and can with least immunity be withheld, must be the most nutritive of all; and thus water claims pre-eminence over beef.

Water is so abundant around us, and it passes in and out of the system with such freedom, that we are naturally disposed to overlook the fact of its forming a *constituent,* tolerably constant in amount. Many of its uses are accurately known. It dissolves gases, without which Respiration would be impossible; and gives the tissues their elasticity, the humours their fluidity. It is the great vehicle of chemical change. If the lungs were formed precisely as they are, with the single exception of having no moisture on their surfaces, Respiration could not be effected; as we see when the fish is taken out of water, and its gills become dry by evaporation. The cornea of the eye owes its transparency to water, and the removal of that small quantity would render vision a mere perception of a local change in temperature.

---

* *Chemical Letters,* 1851, p. 382; ed. 1859, p. 404.

But it is unnecessary to rehearse the manifold properties of water in the vital organism; we have said enough to show its eminence as Food.

Common Salt (chloride of sodium) is another constant and universal substance which claims rank as Food. It forms an essential part of all the organic fluids and solids, except the enamel of the teeth;[*] a statement to which attention is called, because Liebig, in one passage,[†] seems to deny that it forms part of the tissues, declaring that in muscle chloride of potassium is abundant, but no chloride of sodium exists; but the analyses of Von Bibra, Barral, and others, declare the existence of this salt in muscle.

---

[*] Robin and Verdeil: *Traité de Chimie Anatomique,* ii. 175; and Lehmann, ii. 404; iii. 80.

[†] *Chemical Letters,* 1851, pp. 405, 406; ed. 1859, p. 426. That this statement should be left unmodified is the more surprising since it relates to a purely chemical question. Was it overlooked by Liebig in his revision of the Letters, or does he maintain his original opinion in spite of

Common salt is always found in the blood, in quantities which vary within extremely narrow limits, forming less than a half per cent (0.421) of the entire mass, water included, and as much as 75 per cent of the ashes of the blood. This quantity is wholly independent of any surplus contained in food; for the surplus is either not absorbed, or is carried away in the excretions and perspiration;* and this shows salt to be an anatomical constituent, not an accident. If too little salt be taken in the food, instinct forces every animal to supply the deficiency by eating it separately. "The wild buffalo frequents the salt licks of North-Western America; the wild animals in the central parts of Southern Africa are a sure prey to the hunter who conceals himself beside a salt spring; and our domestic cattle run peacefully to the hand that offers them a taste of this luxury. From time immemorial it has been known that without salt man would miserably perish; and among horrible punishments, entailing certain death, that of feeding culprits on saltless food is said to have prevailed in barbarous times." †

---

the analyses made by others? Enough for us that the latest authorities—to which may be added Professor MILLER: *Elements of Chemistry*, 1857, iii. 681—proclaim the existence of chloride of sodium in muscle as well as other tissues.

* DE BLAINVILLE has noticed that people living on the coast, or eating salted meats, have a decided increase of salt in their perspiration.

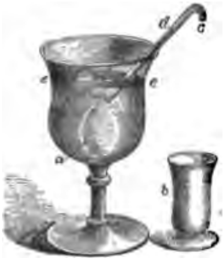
† JOHNSTON: *Chemistry of Common Life*, p. 400.

When Cook and Forster landed in Otaheite they astonished the natives who saw them eating white powder with every morsel of meat; and every one remembers Man Friday's expressive repudiation of salt. But the savages who ate no "white powder," ate fish; and cooked their flesh in sea-water, rich in salt. In several parts of Africa men are sold for salt; and on the Gold Coast it is the most precious of all commodities. On the coast of Sierra Leone a man will sell his sister, his wife, or his child, for salt, not having learned the art of distilling it from the sea.

The properties of salt are manifold. It forms one of the essential conditions of vital processes. It renders albumen soluble, and is necessary for digestion, being decomposed in the stomach into hydrochloric acid for the gastric process, and soda for the bile.

Law of Endosmosis.—Salt has also a most important property, namely, that of aiding to effect the interchange of fluids through the walls of the vessels, in accordance with that law of *endosmosis*, on which so many vital processes depend—a law which we must pause for a moment to consider, since we shall hereafter have frequent occasion to invoke it. If a bladder be filled with alcohol, or any other liquid, and placed in an empty glass vessel, none of the alcohol will pass through the bladder into the vessel. If we pour alcohol into the vessel—or if we pour any other liquid into it having the same density as the liquid contained in the bladder—none will pass from the vessel into the bladder, nor from the bladder into the vessel. We say the bladder is "impermeable" by fluids; and because of this impermeability, we employ it to hold fluids. Nevertheless we make a great mistake in saying so. The bladder is perfectly permeable, and will allow fluids to pass in or pass out, provided the conditions be slightly changed. Thus, if we place this bladder of alcohol in a vessel of water, the water being a liquid *of different density* from the alcohol, a surprising phenomenon occurs—the water penetrates rapidly into the bladder, and a little of the alcohol passes into the vessel. This action goes on till a complete equality of density is established between the fluid in the bladder and that in the vessel. This *rushing in of the water* is called *endosmosis;* this *rushing out of the alcohol* is called *exosmosis.* The following figure represents the endosmometer of Dutrochet, the discoverer of this surprising law.

Fig. 3.



ENDOSMOMETER.

The small bladder *a* is tied to a glass-tube *d*, open at both ends, one of which is bent, *c.* The bladder is filled with alcohol till it reaches the point of the tube *d;* it is then placed in a vessel of water *e;* and almost at once the alcohol, having its volume increased by the water which has penetrated the bladder, will be seen rising in the tube and falling drop by drop into the glass *b.* This continues till the alcohol and water are completely mixed in bladder and vessel.

We must not enter further upon this subject, interesting though it is. Enough if we bear in mind that *whenever an animal membrane separates two fluids of different densities, a mutual interchange between those fluids will take place.* Thus although the minute blood-vessels, called capillaries, are closed tubes, and would keep the blood in them as a bladder holds water, they suffer gases and liquids to pass through their walls *from them,* and *into them,* whenever the gas or liquid outside is of a different density to that inside.

We may now return from our digression to the consideration of Salt, which, by increasing the density of the fluids into which it enters, is one of the many agents in endosmosis. Indeed, so great are the services of salt, that we may confidently accept the statement of Dr Bence Jones, that it is "a substance as essential to life as nitrogenous food, or non-nitrogenous food and water;"* and if so essential, then assuredly Food.

It would lead us too far, and the excursion would be unnecessary, to examine separately all the inorganic substances taken as Food; enough has already been said to justify the classification, which places the inorganic beside the organic substances, as one of the two great divisions into which the subject naturally falls. If we do not dine off minerals, nor find ourselves pleasantly munching a lump of chalk, as we should munch a lump of bread; if, as a general rule, we eat mineral substances only in combination with organic substances, and not separately; the rule is absolute which forces us to eat organic substances in combination with inorganic, because in fact no pure organic substance can be found. It may seem absurd to talk of eating inorganic food, because we rarely eat it separately; but in that sense it is absurd to talk of eating organic food, because organic substance, free from all admixture of the inorganic, has never been eaten by any man.

---

* BENCE JONES: *On Gravel, Calculus, and Gout,* p. 46.

CHAPTER II.

(*Continued.*)

FOOD AND DRINK.

SECTION II.—ALIMENTARY PRINCIPLES.

Effect of different conditions in substances—Life interferes to disturb ordinary chemical actions—Effect of Form—Food considered chemically and physiologically—Blood as a standard of food—Milk as a standard—Albumen—Fibrine—Caseine—The protein bodies—Gluten—Gelatine, is it nutritious?—History of the inquiry—Fats and oils; their digestibility in various conditions—Starch as food—Sugar—Does sugar injure the teeth?—Alcohol as food—Teetotalism—Iron as food—Acids, do they prevent scurvy?—Does vinegar keep people thin?

HAVING in the preceding section argued some of the general questions relative to food, we must now review, in a brief manner, the various Alimentary Principles of which food is composed; and then we may pass on to the various articles of food consumed by man.

1. *Effect of different conditions.*—The water which drowns us as a fluent stream, can be walked upon as ice. The bullet which, when fired from a musket, carries death, will be harmless if ground to dust before being fired. The crystallised part of the oil of roses, so grateful in its fragrance—a solid at ordinary tem-

peratures, though readily volatile—is a compound substance containing exactly the same elements, and in exactly the same proportions, as the gas with which we light our streets. The tea which we daily drink, with benefit and pleasure, produces palpitations, nervous tremblings, and even paralysis, if taken in excess; yet the peculiar organic agent—called theine—to which tea owes its qualities, may be taken by itself (as theine, not as tea) without any appreciable effect.* The water which will allay our burning thirst, augments it when congealed into snow; so that Captain Ross declares the natives of the Arctic regions "prefer enduring the utmost extremity of thirst rather than attempt to remove it by eating snow."† Yet if the snow be melted, it becomes drinkable water; and it *is* melted in the mouth. Nevertheless, although, if melted before entering the mouth, it assuages thirst like other water, when melted *in* the mouth it has the opposite effect. To render this paradox more striking, we have only to remember that ice, which melts more slowly in the mouth, is very efficient in allaying thirst.

These facts point to an important consideration, which has been little regarded by the majority of those who have written on Food: the consideration of the profound differences which may result from simple differences in the *state* of substances. The chemist, in his elementary analysis, necessarily gives no clue to such differences. He tells us *of* what elements an article of Food is composed; but he cannot tell us *how* those elements are combined, nor in what state the substance is. Even when he has ascertained the real composition and properties of any substance, he has still to ask the physiologist what are the *conditions* presented by the organism in which this substance is to undergo chemical transformation.

---

* SCHLEIDEN: *Die Pflanze*, 1858, p. 205.

† ROSS: *Narrative of Second Voyage*, p. 366.

We know that a change in the conditions will cause a change in the manifestation of a force; so that often what ordinarily takes place in the laboratory will not at all take place in the organism. Chlorine and hydrogen are gases having a powerful affinity for each other —that is to say, they will unite when brought together in the daylight; but if we change the conditions—if we bring them together in the dark—their affinity is never manifested; and thus, while in the sunlight they rush together with explosive force, producing an intense acid, they will remain quiescent in the darkness, and there for all eternity would form no combination. Again, this same chlorine decomposes water in the sun's rays; but in darkness it has no such power.

If such are the effects of so simple a change in the *conditions*, it is easy to imagine how various must be the differences between the phenomena which occur in the laboratory, and those which the same substances present under the *complex conditions of the organism.*

2. *Interference of Life.*—The chemist employs vessels of glass, in which he isolates the substances he examines, keeping them free from the interference of other substances, because he knows that, unless such

interference be avoided, his experiment will be nullified. He knows, for example, that a drop of water which, if poured into a red-hot crucible, flies up into his face as *steam*, will rapidly pass into *ice* if a little liquid sulphurous acid happen to be present. He knows, in short, that the stronger affinity prevents the action of the weaker affinity; and, to be sure of his experiment, he must isolate his substances.

But in the vital laboratory no such isolation is possible. The organism has no glass vessels, no air-tight cylinders. Vital processes go on in tissues which, so far from isolating the substance introduced—so far from protecting it against interference, do inevitably interfere, and are *themselves involved* in the very changes undergone by the substance. Thus, while it is true that an alkali will neutralise an acid out of the organism, we must be cautious in applying such a chemical principle in the administration of drugs, because the alkali may stimulate a greater secretion of the gastric acid; so that, over and above the amount neutralised, there will be a surplus of acid free, owing to the interference of the organism in which the process takes place.

Besides the complications which occur from the inevitable interference of the organism itself, and from the differences resulting from differences in the state of bodies, there are other complications arising from causes peculiarly vital. In Biology, questions of Form are scarcely less important than questions of Composition. Spread out a cell into a layer, and you will find, that in ceasing to be a cell, it has ceased to act as such—it has lost all the properties which distinguished it as a cell.

Thus, the green cells of the plant decompose carbonic acid. Even the torn leaf will equally fix the carbon and liberate the oxygen, provided its cells are preserved in their integrity of form. But if these cells are crushed, or otherwise injured, this vital property ceases, because the cell alone is capable of manifesting it.*

Under the influence of yeast, sugar is decomposed into alcohol and carbonic acid; but if the yeast-cells be crushed and disorganised, their action on the sugar is said to be quite different: instead of converting it into alcohol and carbonic acid, they convert it into lactic acid.

We must acknowledge, then, that when certain combinations of carbon, oxygen, hydrogen, nitrogen, and salts, assume the form of a cell, the phenomena manifested are no longer simply chemical, but assume the speciality of vital action.

Such considerations need all our attention in dealing with so complex a question as that of Food. They show the radical incompetence of chemical laws alone to solve the questions of Physiology, and urge us to reject, as misdirected labour, all attempts at establishing anything more than chemical facts in the "Chemistry of Food."

It was undoubtedly a great discovery which Mulder made in 1838, that the albumen of plants was identical, or nearly so, with the albumen of animals; and consequently, that, when the ox ate grass, and the lion ate the ox, both derived their nutriment from the same chemical substance. A great discovery; but I cannot agree with Moleschott[*] in thinking this discovery first settled the basis of a science of Food. It was a chemical triumph, fruitful in results to chemistry; but its physiological bearing has been greatly exaggerated, and has given increased impetus to that chemical investigation of Food, which, as we have seen, cannot, in the nature of things, be other than misleading. And although Mulder has shown the inaccuracy of the notion, that vegetable albumen is *identical* with the fibrine of the blood, and vegetable caseine with the caseine of the blood[†] — although he energetically repudiates, as unphilosophical, the idea of a chemical analysis furnishing any true standard of nutritive value—yet even he has not clearly stated what the true method of investigation must be.

---

* MULDER: *Versuch einer Physiol. Chemie,* i. 193. LEHMANN: *Lehrbuch der Physiol. Chemie,* iii. 170.

To the chemist there may be little or no difference between plant and flesh, as Food; to the physiologist the difference is profound: he sees the lion perishing miserably of inanition in presence of abundant herbage, which to the elephant or buffalo furnishes all that is needful. The ox eats the grass, and the tiger eats the ox, but will not touch the grass. The flesh of the ox may contain little that is not wholly derived from the grass; and the chemist, analysing the flesh of both, may point out their identity; but the physiological question is not: What are the chemical constituents of nutritive substances? it is: What are the substances which will nourish the organism? If the animal will not eat,—or, having eaten, cannot assimilate,—a certain substance, that substance is no food for that animal, be its chemical composition what it may.*

---

[*] MOLESCHOTT: *Kreislauf des Lebens*, 1857, p. 101.

[†] MULDER: *Physiol. Chemie*, p. 917.

We thus see that *digestibility* is an important element in the estimate of Food: unless the substance can be digested, it cannot be assimilated, cannot nourish; although, perhaps, *if* assimilated, the substance might have a high value. A pound of beefsteak contains an enormous superiority of tissue-making substance over that contained in a pound of cabbage; yet to the rabbit the cabbage is the superior food, while to the dog the cabbage is no food at all.

3. *The Blood as a standard of Food.*—When we consider the part played by Food, as furnishing the materials out of which the organic fabric is constructed, and its actions facilitated, it seems natural to assume that the Blood is the proper standard we should have in view; and that we should designate those substances as Aliments which, directly or indirectly, go towards the formation of Blood.

Yet, on a deeper scrutiny, this is seen to lead us a very little way. An analysis of Blood will neither give us a complete list of alimentary substances, nor indicate the alimentary value of each special substance. True it is that all the tissues are formed from the Blood, and that all alimentary substances, in their final state previous to assimilation, make their way into it. But we will briefly point out why, in spite of all this, the Blood can never furnish us with the desired standard.

---

* It is curious that carnivora feed chiefly, sometimes exclusively, on herbivora, and *not* on carnivora, whose flesh most resembles their own.

In the first place, while Blood is truly the vehicle of nutrition, it is at the same time the vehicle of many products of decay and disintegration. It carries in its torrent the materials for the use of to-day and to-morrow, but it also carries the materials which were vital yesterday, and are effete to-day, unfit to be retained, and hurrying to the various issues of excretion.* Blood is thus at once purveyor-general and general sewer, carrying life and carrying death. We shall therefore always find in it substances which are not alimentary, mingled with those which are; and we cannot separate these, so as to make our analysis of use.

In the second place, among the substances normally current in the circulation we do not find several which are notoriously serviceable as aliments. Some of these, as theine, caffeine, alcohol, &c., are not present in the blood; and others, as fats and sugars, are present in quantities obviously too small for the amounts consumed as food.

Finally, although substances are nutritive, or blood-making, in proportion to their resemblance to blood, yet this resemblance must exist *after*, not before, the substances have gone through the process of digestion; since no sooner is any substance taken into the stomach than a series of changes occurs—changes indispensable for its admission into the circulation, but which impress on it a very different character from the one it bore on its entrance. A beefsteak is assuredly more nearly allied in composition to the blood of an ox than is the dewy grass of the meadow; yet the beefsteak will form no blood for the ox, because it cannot be properly digested, whereas the grass becomes converted into blood in the course of the changes impressed on it during digestion; and what was thus *unlike* becomes *like*, or, as we say, *assimilated*.

---

* See on this point JOHN SIMON, *Lectures on Pathology*, p. 23:—"*Mentally* we can separate these three kinds of blood, but experimentally we cannot. They are mixed together—past, present, and future—the blood of yesterday, the blood of to-day, and the blood of to-morrow—and we have no method of separating them."

The experiments of Claude Bernard are highly suggestive on this point. He found that if sugar, or albumen, were injected into the veins, it was not assimilated, but was eliminated, unchanged, by the kidneys; whereas, if either substance were injected into the veins together with a little gastric juice, assimilation was complete. In another experiment he found that if sugar and albumen were injected into the portal vein (which would carry them through the liver, where certain changes are always impressed on them), they would be assimilated; but if he injected them into the jugular vein, by which they would reach the lungs without passing through the liver, no assimilation would take place.

We here once more see the necessity of taking into account the organism and its vital acts, whenever we would attempt an explanation of Food.

The general considerations which *a priori* caused us to relinquish the idea of finding a proper standard in the composition of the Blood, are fully confirmed by the results of Payen's experiments, which show that Blood is not a good aliment. He fed pigs on equal proportions of flesh and blood, and found that they exhibited all the signs of starvation; whereas, when fed under similar conditions, except that blood was replaced by an equal amount of flesh, they fattened and grew strong.*

4. *Milk as a standard of Food.*—The Blood, then, must be given up. Shall we try Milk? Others have done so before us, making it the standard of Food, because it is itself an aliment which contains all the substances necessary for the nourishment of an organism during the most rapid period of growth. Out of milk, and milk alone, the young elephant, the young lion, or the young child, extracts the various substances which furnish muscles, nerves, bones, hair, claws, &c.; milk furnishes these in such abundance, that the increase of growth is far greater during the period when the animal is fed exclusively on it, than at any subsequent period of its career. "In milk," says Prout, "we should expect to find a model of what an alimentary substance ought to be—a kind of prototype, as it were, of nutritious elements in general."

The idea was so plausible that its acceptance was general. Nevertheless nothing is more certain than that milk is not this model food, since, however it may suit the young lion, or the young child, we cannot feed the adult lion, or the adult man on milk alone: we can feed the lion on bones and water, and the man on bread and water, but not on milk. A model food for the young, it ceases to be so for the adult; that relation which existed between the Food and the Organism in the one case, no longer exists in the other.

---

* PAYEN: *Des Substances Alimentaires*, p. 45.

If milk does not furnish us with an absolute standard (except for the young), it furnishes an approximative standard of great value. Its composition points out the proportions of inorganic and organic substances necessary in the food of the juvenile organism, and of course approximatively in that of the adult. In 1000 parts, milk contains—

Water, ...	873
Caseine (nitrogenous matter), .	48
Sugar of milk, ...	44
Butter, ...	30
Phosphate of lime, . . .	2.30
Other salts, ...	2.70
	1000

The reader may remark with some surprise, that in an aliment so notoriously high in nutritive value as milk, the proportion of nitrogenous matter is so very insignificant (48 in 1000) as to render the hypothesis of nitrogenous matters being pre-eminently *the* nutritive matters somewhat perplexing.

The foregoing analysis of milk aids our investigation, by proving the necessity of four distinct classes of principles in Food. These four classes are,—

1°. The Inorganic principles.
2°. The Albuminous principles.
3°. The Oils and Fats.
4°. The Sugars.

The proportions of these substances requisite will, of course, vary with the needs of the various organisms, as modified by race, age, climate, activity, and so forth; but Nutrition will be imperfect unless all four are present, either *as* such, or else under conditions of possible formation—thus fats and sugars can, we know, be formed in the organism with a proper allowance of materials; and I am strongly disposed to think that albuminous substances can also be formed, though not unless some albumen be present to act as a leaven.

We are thus, by the principle of exclusion, reduced to the one method of investigation which remains, and that is to interrogate the organism, not the laboratory.

> "Experience, daily fixing our regards
> On Nature's wants,"

must guide us in the search. To ascertain what substances are nutritious, we must ascertain those which really nourish; and the relative value of these can only be ascertained by extensive and elaborate experiments on the feeding of animals, conducted on rigorously scientific principles. In other words, we must adopt that very method which common sense has from time immemorial pursued; with this important difference, that instead of allowing it to be, as hitherto, wholly empirical, we must subject it to the rigour, caution, and precision which characterise Science.

And even when Science shall have established laws on this point, such as may accurately express the general value of each substance as food, there will always remain considerable difficulty in applying those laws, owing to that peculiarity of the vital organism, previously noticed—namely, that the differences among individuals are so numerous, and often so profound, as to justify the adage, "one man's meat is another man's poison."

Thus, while experience plainly enough indicates that, in Europe at least, meat is more nutritious than vegetables, those who eat largely of meat being stronger and more enduring than those who eat little or none; we must be cautious in the application of such a principle. Difference of climate may, and difference of temperament certainly does, modify this question. The Sepoy, who lives chiefly on rice, would, it is said, outrun, knock down, or in any other way prove superiority in strength over the Gaucho of the Pampas, who lives on flesh. And not only are some organisms ill adapted to a flesh diet, as we have seen; but Andersson says, that the strongest man he ever knew scarcely ever touched animal food: this was a Dane, who could walk from spot to spot carrying a stone, which was so heavy that it required ten men to lift it on to his shoulders; his chief diet was gallons of thick sour milk, tea, and coffee;* a diet which no ordinary man could support with success.

Having discussed the chief topics relating to Food in general, we may now ascertain what Science can tell us respecting the various Articles employed as nourishment by man. Our inquiry falls naturally under two heads — first, the Alimentary Principles, considered separately; and next, the Compound Aliments, or those articles of Food and Drink which make up the wondrous variety of human nourishment.

1. *Albumen.*—This substance, familiar to all as the white of an egg, constitutes an important element in Food. It exists as a liquid in the blood, as a solid in flesh. When raw, or lightly boiled, it is readily digested; less so when boiled hard, or fried. Majendie has observed that the white of eggs combines many conditions favourable to digestion, for it is alkaline, contains saline matters, especially common salt, in large proportions, and it is very nearly allied to the albumen found in the chyle and blood. It is liquid, but is coagulated by the acids of the stomach, forming flocculi having slight cohesion, and rendered easily soluble again by the intestinal juices.

Many people imagine that white of egg is injurious, or innutritious, and they only eat the yolk. To some this may be so; and when experience proves it to be so, white of egg should not, of course, be eaten; but, as a general rule, white of egg is agreeable and nutritious. Nevertheless, if given *alone*, neither white of egg nor albumen will continue to be eaten by animals ; they soon cease to eat it, and, during the period in which it is taken, they show unmistakable signs of starvation.

---

* ANDERSSON: *Lake Ngami*, p. 58.

Albumen, then, is highly nutritious ; and if we estimated the nutritive value of various articles *solely* according to their amounts of albumen, we should place caviare, ox-liver, and sweetbread at the top of the list, leaving the muscle of beef very far below them. The following table shows the proportions of albumen in 100 parts of various articles of food :—

Caviare,	31.00
Ox-liver,	20.19
Sweetbread,	14.00
Muscle of pigeon,	4.05
„ of veal,	3.02
„ of chicken,	3.00
„ of beef,	2.02

This table is very instructive, as showing the vanity of attempting by a chemical analysis to assign the nutritive value of any food. Beef is considered the most nutritive of all these articles, which, according to this analysis, should be least so. This discrepancy is lessened, but not removed, when we take into account the quantity of fibrine contained in these articles, namely—

Sweetbread,	8
Veal (muscle),	19
Chicken (muscle),	20
Beef (muscle),	20

2. *Fibrine* is liquid in the serum of the blood, and is very closely allied to albumen—indeed, for a long while was supposed to be identical with it and with the fibrine of muscle, which is now more accurately called *musculine* (by Lehmann *syntonin*).

When the blood is drawn from the body, fibrine passes from the liquid to the solid state, and coagulates into what is called the *clot*, which is nothing but solid fibrine enclosing some of the red corpuscles.* It was formerly supposed that this solidification was all that took place when blood-fibrine passed into muscular fibre; but recent investigations have shown that muscle-fibrine is really a different substance, allied to, but not identical with, blood-fibrine.

Albumen and fibrine are found abundantly in vegetables—the former being most abundant in wheat, rye, barley, oats, maize, and rice. Albumen is found also in the oily seeds, such as almonds, nuts, &c.; in the juices of carrots, turnips, cauliflowers, asparagus, &c.† Fibrine is also abundant in the cereals, grape-juice, and the juice of other vegetables.

Although closely allied to animal albumen and fibrine, vegetable albumen and fibrine are not identical with these substances, differing from them both in composition and properties; but the differences are so slight, that vegetable albumen easily passes into animal albumen in the process of digestion.

---

* See the chapter on THE STRUCTURE AND USES OF THE BLOOD for details respecting the serum, the fibrine, and the clot.

† Albumen forms three compounds—basic, acid, and neutral. In the white of egg, and in the serum of blood, it is a basic albuminate of soda. In certain diseases it is a neutral albuminate in the blood.

3. *Caseine* is another of the albuminous substances, and may be regarded as a modification of albumen, into which it readily passes. It forms the *curd*, or coagulable matter of milk. Unlike albumen, it does not coagulate by heat. If heated in an open vessel, an insoluble pellicle is formed on the surface, as we often see in the milk-jug brought up with our coffee; but this effect is produced by the action of the oxygen of the atmosphere.

The proportion of caseine in different kinds of milk is as follows:—

Cow's milk, . . .	4.48
Ewe's milk, . . .	4.50
Goat's milk, . . .	4.02
Asses' milk, . . .	1.82
Human milk, . . .	1.52

It thus appears, we hope without derogation to human dignity, that asses' milk is considerably more like that on which we were suckled, than any other milk. Asses' milk may be given to infants with safety; but goat's or cow's milk must be diluted—as all experienced nurses know.

Caseine forms the chief ingredient of cheese. It is an important element of food, as we see by its presence in milk. "The young animal receives, in the form of caseine, the chief constituent of the mother's blood. To convert caseine into blood, no foreign substance is required; and in the conversion of the mother's blood into caseine, no elements of the constituents of blood have been separated. When chemically examined, caseine is found to contain a much larger proportion of the earth of bones than blood does, and that in a very soluble form, capable of reaching every part of the body. Thus, even in the earliest period of its life, the development of the organs in which vitality resides is, in the carnivorous animal, dependent on the supply of a substance identical in organic composition with the chief constituents of its blood."*

Caseine is also found in beans, peas, lentils, almonds, nuts, and perhaps in all vegetable juices.

These three bodies—albumen, fibrine, and caseine—from the readiness with which they are transformed into each other, are not inaptly designated "protein-bodies," even now that Mulder's idea of an organic radical, named by him "protein," has been given up by all the leading chemists.

In the egg we see caseine arise from albumen, and in digestion caseine passes back again into albumen.

Fibrine, again, appears to be only albumen with an addition of oxygen; and it may be easily reconverted into albumen by nitrate of potash. It differs from albumen in assuming something of definite structure when coagulated: it forms itself into delicate fibres, which albumen never does.

There are many unexplained facts known respecting fibrine, which, when explained, may clear away other obscurities. Lehmann found, by experiments on himself, that animal diet produced more fibrine in his blood than was produced by vegetable diet—a fact seemingly at variance with another fact, namely, that, during starvation, the quantity of fibrine is increased, as it is also during acute inflammations. Thus, animal diet, known to be nutritious, produces one result known to be characteristic of inflammation and starvation.

---

* LIEBIG: *Animal Chemistry*, p. 52.

Nor does the difficulty cease here: the blood of the vegetable feeders, among animals, has more fibrine than that of the flesh-feeders; yet the carnivorous dog has *less* fibrine when fed on vegetable food, than when his diet has been exclusively animal. Finally, although herbivora have more fibrine than carnivora, birds have more than both.

4. *Gluten* is not found in animals, but exists abundantly in vegetables, and is the most important of all the nitrogenous substances, because, as we have seen, it is capable of supporting life when given alone. "It is the presence of gluten in wheaten flour that renders it pre-eminently nutritious; and its viscidity or tenacity confers upon that species of flour its peculiar excellence for the manufacture of macaroni, vermicelli, and similar pastes, which are made by a kind of wiredrawing, and for which the wheat of the south of Europe is peculiarly adapted."*

The following table, which is selected from Dr

---

* BRANDE'S *Chemistry*, quoted by PEREIRA. On this subject see the chapter "The Bread we eat," in JOHNSTON'S *Chemistry of Common Life*.

Pereira's work, gives the proportions of gluten in 100 parts of various vegetables:—

Wheat, Middlesex (average crop),	19. 0
„ Spring	24. 0
„ Thick-skinned Sicilian, .	23. 0
„ Polish, ...	20. 0
„ North American, . .	22. 5
Barley, Norfolk, ...	6. 0
Oats, Scotland, ...	8. 7
Rye, Yorkshire, ...	10. 9
Rice, Carolina, ...	3.60
„ Piedmont, ...	3.60
Maize, ...	5.75
Beans, ...	10. 3
Peas, ...	3. 5
Potatoes, ...	4. 0
Turnips, ...	0. 1
Cabbage, ...	0. 8

These four albuminoid * substances, namely albumen, fibrine, caseine, and gluten, are remarkable among other things for their extreme *instability,*—the readiness with which they are transformed, or decomposed. It is this alterability which renders them peculiarly apt to act as *ferments,* and to induce chemical changes in the substances with which they come in contact. It is on this alterability that their great value in nutrition depends.

Further, we must remark that, no matter what is the form in which they are eaten, whether as white of egg, fibrine, caseine, or gluten, these substances are all reduced by the digestive process to substances named *peptones*, under which forms only are they assimilable.[*]

---

* *Albuminoid* means having the character of albumen.

5. *Gelatine.*—There is perhaps no substance on our list which more interestingly illustrates the want of a true scientific doctrine presiding over the investigations into Food, than Gelatine: a substance richer in nitrogen than any of the albuminoid substances, yet denied a place among the plastic elements: a substance which, under the forms of jellies and soups, is largely given to convalescents, who get strong upon it, yet which, we are emphatically assured, has "no nutritive value whatever." Mulder says that no physician, who has had experience, could doubt the nutritive value of gelatine; and we may be pretty sure that common usage, in such cases, is founded upon some solid ground, and that no substance is largely used as food which has not a nutritive value.

Common usage, or what is called "common sense," must not indeed be the arbiter of a scientific question; but it has a right to be heard when it unequivocally contradicts the conclusions of Science; and it can only be put out of court on a clear exposition of the source of its error. In the present case, the savans pretend that Gelatine *cannot* be nutritive, common sense asserts that it *does* nourish; and unless the fact can be proved against common sense, it will be reasonable to suppose that the savans are arguing on false premises. False, indeed, are the premises, and false the conclusion. But let us see what has been the course of inquiry.

---

[*] See the ensuing chapter on DIGESTION AND INDIGESTION.

In 1682 the celebrated Papin discovered that bones contained organic matter; and he invented a method of extraction of this matter, which subsequently occupied the chemists and savans, in the early days of the French Revolution, with the laudable desire of furnishing food to the famished people. A pound of bones was said to yield as much broth as six pounds of beef, and, with the true fervour of inventors, the savans declared bone-soup to be better than meat-soup.

In 1817, M. d'Arcet applied steam on a grand scale to the preparation of this gelatine from bones, promising to make four oxen yield the alimentary value of five, as usually employed.

Great was the excitement, vast the preparations. In hospitals and poorhouses, machines were erected which made an enormous quantity of Gelatine. Unfortunately the soup thus obtained was found to be far from nutritious; moreover, it occasioned thirst, digestive troubles, and finally diarrhœa. The savans heard this with great equanimity. They were not the men to give up a theory on the bidding of vulgar experience. Diarrhœa was doubtless distressing, but science was not implicated in *that*. The fault must lie in the preparation of the soup; might not the fault be attributable to the soup-eaters? One thing only was positive —that the fault was *not* in the Gelatine.

In this high and unshaken confidence, the savans pursued their course. Thousands of rations were daily distributed; but fortunately these rations were not confined to the bone-soup, or else the mortality would have been terrific. Few men of science had any doubts until M. Donné positively assured the Academy that experiments on himself, and on dogs, proved Gelatine, thus prepared, to be scarcely, if at all, nutritious.

He found that employing a notable quantity in his own diet caused him rapidly to lose weight, and that during the whole experiment he was tormented with hunger and occasional faintness. A cup of chocolate and two rolls nourished him more effectually than two litres and a half of bone-soup accompanied by 80 to 100 grammes of bread.*

These statements were confirmed by other experimenters; and the confidence in Gelatine was rudely shaken, and would have been ignominiously overthrown, had not Edwards and Balzac published their remarkable memoir (1833), in which experiments conducted with great care and scientific rigour established the fact that although Gelatine is *insufficient* to support life, it has nevertheless nutritive value. Dogs fed on gelatine and bread became gradually thinner and feebler; but when fed on the same amount of bread alone, their loss was far more rapid.

At this period it became necessary to have the question definitively settled, and the French Academy appointed a Commission to report on it. This is the celebrated "Gelatine Commission" so often referred to. The report appeared in 1841. It showed that dogs perished from starvation in presence of the Gelatine extracted from bones, after having eaten of it only a few times. When, instead of this insipid Gelatine, the agreeable jelly which pork-butchers prepare from a decoction of different parts of the pig was given them, they ate it with relish at first, then ceased to eat it, and died on the twentieth day, of inanition; when bread, or meat, in small quantities, was given, the dogs lived a longer time, but grew gradually thinner, and all finally perished. A striking difference was observed between bone-soup and meat-soup: the animals starved on the first, and flourished on the second.

---

* A *litre* is a trifle more than a pint and a half; a *gramme* is about 15¼ grains.

The conclusion generally drawn from this Report is, that Gelatine is *not* a nutritive substance. But all that is really proved by the experiments is that Gelatine *alone* is insufficient for nutrition; a conclusion which is equally true of albumen, fibrine, or any other single substance. For perfect nutrition there must be a mixture of inorganic and organic substances: salts, fats, sugars, and albuminates.

When animals are fed on albumen alone, or white of egg alone, with water as the single inorganic element, they perish; but they live perfectly well on raw bones and water—the reason being that bones contain salts and small proportions of albumen and fats, to supplement the Gelatine, and *they contain these in the state of organic combination*, not in the state of chemical products.

The paramount importance of this last condition may be gathered from the experiments mentioned in the Gelatine Report—namely, that boiling the bones, or digesting them in hydrochloric acid, and thus resolving their cartilaginous tissue into Gelatine, *destroyed* this nutritive quality. The very bones which, when raw, supported life, failed utterly when boiled. This seems to show that Gelatine is so altered by heat as to lose its power of fermenting, and so passing through the transformation necessary to assimilation.

Attention is called to the fact of the very small proportions of Albumen which exist in the bones, as strikingly confirming my hypothesis respecting the power of the organism to form Albumen for itself, if a small amount be present to act as a sort of leaven. Moleschott also maintains, on other grounds, that Gelatine must be converted into Albumen, since the amount of Albumen in bones is in itself utterly insufficient for the demands of the tissues;[*] and Mulder points to the fact that, when an animal is fed on Gelatine, we never find this substance passing away in the excreta: a sufficient proof that it must in some way have been incorporated with the organism, or decomposed in it, to subserve the purposes of nutrition.[†]

Physiologists who admit *some* nutritive quality in Gelatine, have suggested that it is confined to the formation of the gelatinous tissues. This is one of those hypotheses which seduce by their plausibility; and accordingly it has been frequently adopted, although physiological scrutiny detects that this is precisely one of the uses to which Gelatine can *not* be turned. For on the one hand we see that the herbivora have gelatinous tissues, although they eat *no* Gelatine; and, on the other hand, we see that even the carnivora, who do obtain it in their ordinary food, cannot form their gelatinous tissues out of it, because Gelatine is never found in their blood, from which all their tissues are formed.

---

[*] MOLESCHOTT, *Kreislauf des Lebens*, p. 135.

[†] MULDER, *Physiol. Chemie*, p. 937.

Bernard has shown that part of the Gelatine is converted into sugar; and sugar, we know, is necessary to the organism. It may also be converted into fat; and, as has been said, there is much evidence to show that it may be converted into Albumen, among the complex processes of vital chemistry; but whatever may be the decision respecting this point, there can be no legitimate reason for denying that Gelatine ranks among nutritive principles.

6. *Fats and Oils.*—These are various and important, including suet, lard, marrow, butter, and fixed oils. Vegetables also yield a great variety of oils, fixed, and volatile or essential. The quantity procurable from 100 parts of vegetable and animal substances is as follows:—

Filberts, ...	60
Olive seeds, ...	54
Cocoa-nut, ...	47
Almonds, ...	46
White mustard, ...	36
Linseed, ...	22
Maize, ...	9
Yolk of Eggs, ...	28.75
Ordinary meat, ...	14. 3
Caviare, ...	4. 3
Ox-liver, ...	3.89

Milk, Cows', ...	3.13
„ Women's, ...	3.55
Milk, Asses', ...	0.11
„ Goats', ...	3.32
„ Ewes', ...	4.20
Bones of sheep's feet, . . .	5.55
„ of ox-head, . . .	11.54[*]

Fats and oils are all difficult of digestion—more so, indeed, than most other principles; but the degree in which they are digestible is very much a matter of individual peculiarity, some men digesting large quantities with ease, others being unable to digest even small quantities.

M. Berthé instituted an elaborate series of experiments on his own person, with the view of ascertaining the comparative digestibility of various fats and oils.[†] The following classification of his results is all we can find space for.

First class, comprising those difficult of digestion: Olive oil, almond oil, poppy-seed oil.

Second class, comprising those easy of digestion: Whale oil, butter and animal fats, colourless liver-oil.

Third class, comprising those very easy of digestion: Pure liver-oil.

It should be remembered that great differences are observable according to the state in which oils are eaten. If taken by themselves, they are scarcely affected by the digestive process, and act as laxatives; but if taken mingled with other substances, they may be reduced to an emulsion, and so absorbed.

---

[*] PEREIRA: *Treatise on Diet*, p. 167.

[†] BERTHÉ: *Moniteur des Hôpitaux*, 1856, No. 69. CANNSTATT: *Jahresbericht* 1856, p. 69-72.

Thus we eat olive oil with salad, or butter with bread, and the greater part is absorbed; but the same amount of olive oil administered alone would act as a purge. It is owing, moreover, to the minute state of subdivision and mixture of the oils in all vegetable substances that they are so much more digestible than animal fats.

Dr Pereira quotes the statement of Dr Beaumont, that "bile is seldom found in the stomach, except under peculiar circumstances. I have observed that when the use of fat or oily food has been persevered in for some time, there is generally a presence of bile in the gastric fluids." Upon which Dr Pereira remarks that the popular notion of oily or fatty foods "causing bile" is not so groundless as medical men have generally supposed. The reason of fat being indigestible is thus suggested: "In many dyspeptic individuals, fat does not become properly chymified. It floats on the contents of the stomach in the form of an oily pellicle, becoming odorous, and sometimes highly rancid, and in this state excites heartburn, nausea, and eructations, or at times actual vomiting. It appears to me that the greater tendency which some oily substances have than others to disturb the stomach, depends on the greater facility with which they evolve volatile fatty acids, which are for the most part exceedingly acrid and irritating. The unpleasant and distressing feelings excited in many dyspeptics by the ingestion of mutton-fat, butter, and fish-oils, are in this way readily accounted for, since all these substances contain each one or more volatile acids to which they owe their odour. Thus mutton-fat contains hircic acid; butter, no less than three volatile acids—viz. butyric, capric, and caproic acids; while train-oil contains phocenic acid."*

The effect of a high temperature on fat is to render it still more unsuitable to the stomach; and all persons troubled with an awful consciousness of what Digestion is, and not living in that happy eupeptic ignorance which only knows Digestion as a name, should avoid food in the cooking of which much fat or oil has been subjected to a high temperature—as in frying in butter or lard. Melted butter, buttered toast, pastry, suet-puddings, fat hashes and stews, are afflictions to the dyspeptic; and although the oil which is eaten with salad does not assist the digestion of the salad, as many writers and most salad-eaters maintain, it is assuredly far more digestible than any fat or oil which has been cooked, probably because it contains no free volatile acid.

Besides the fats and *fixed* oils, there are certain volatile (essential) oils employed as condiments. These are contained in the leaves and seeds of sage, mint, thyme, marjoram, fennel, parsley, anise, and caraway; to which may be added, mustard, horse-radish, water-cress, onions, leeks, and various spices. The volatile oil contained in each of these substances stimulates but does not incorporate itself with the organism, and is soon ejected, retaining its characteristic odour.

---

* PEREIRA, p. 171.

7. *Starch.*—The gentle housewife, familiar with starch only in its relations to the wash-tub, will be probably surprised at meeting with it among articles of food; yet under the various names of amylum, fecula, farinaceous matter, and starch, this substance, widely distributed over the vegetable kingdom, ranks as an important alimentary principle. It must, however, be cooked before it can be eaten. It is never found in the blood, nor in the tissues, so that we are certain it is transformed during the digestive process; and some of these transformations have been detected: first, as it passes into dextrine, and thence into sugar, and most probably fat.

The various starchy substances — sago, tapioca, arrowroot, and *tous les mois*, have been so amply treated of by Professor Johnston in his admirable *Chemistry of Common Life*, that our readers need only be directed to his pages.

8. *Sugar.*—Sugar exists abundantly in vegetables, and in some animal substances, notably milk and liver. Dr Pereira has compiled the following table, which exhibits the proportion of sugar in 100 parts:—

Barley-meal,	5.21
Oatmeal,	8.25
Wheat-flour,	8.48
Wheat-bread,	3. 6

Rye-meal,	3.28
Maize,	1.45
Rice,	0.29
Pease,	2. 0
Figs,	62. 5
Greengages,	11.61
Fresh ripe pears,	6.45
Gooseberries,	6.24
Cherries,	18.12
Apricot,	11.61
Peach,	16.48
Beet-root,	9. 0

That sugar is nutritious no one doubts. Although easily digested, there are persons with whom it disagrees, and in some dyspeptics it produces flatulency and acidity. There is no tissue into the composition of which it enters as a constituent, unless we make an exception in favour of muscle, in which Scherer has discovered a substance, by him named *inosite*, having the chemical composition of sugar ($C^{12}\underline{H}^{12}\underline{O}^{12}$), but having none of its characteristic properties, and existing, moreover, in extremely minute quantities. H^{22} O^{11}

The sugar we find in the blood and milk is not derived from the sugar we eat; *that* is transformed into fat, lactic acid, and other substances. The sugar of the blood is formed partly from the starchy substances of our food during the process of digestion; the rest is formed *by* the liver, and is formed *from* albuminous substances in their passage through the liver; the quantity being wholly independent of any amount of sugar taken in the food, and being the same in amount when *none* is taken in the food.*

Because sugar forms part of no tissue, and is a carbohydrate, it is classed by Liebig's school among heat-making foods. But we not only saw ample reason for rejecting such an idea when we considered the general question—we must even more peremptorily reject it, now that we come to grapple with the details. Against the supposition of sugar having no plastic property, it is enough to oppose the fact that many insects feed solely on sugar and saccharine juices; and in them, therefore, it is clear that something more than heat is evolved from sugar. Lehmann also bids us remember that in the egg a small quantity of sugar exists, and this quantity increases, instead of diminishing, as the development of the chick proceeds; whereas, if sugar only served for purposes of oxidation, it would be oxidised and disappear as development advanced.

In the *Chemistry of Common Life*, the subject of sugar is treated in detail, which renders repetition here superfluous. Two questions only need be touched on, Is sugar injurious to the teeth? Is it injurious to the stomach?

---

* CLAUDE BERNARD'S discovery of this sugar-forming function of the liver has been attacked by FIGUIER, LONGET, and others; but the discussion, after exciting considerable sensation, may now be said to be finally closed in BERNARD'S favour. See his masterly *Leçons de Physiol. Expérimentale*, 1854-5; and the *Mémoires* on both sides in the *Annales des Sciences Naturelles*, 1854-6; and the Report of the Commission appointed by the Académie de Medicine, reprinted in BROWN SÉQUARD'S *Journal de la Physiologie*, i. 549.

To answer the first, we have only to point to the Negroes, who eat more sugar than any other human beings, and whose teeth are of enviable splendour and strength.

To answer the second is not so easy; yet, when we learn the many important offices which sugar fulfils in the organism, we may be certain that, if injurious at all, it is only so in excess. The lactic acid formed from sugar dissolves phosphate of lime, and this, as we know, is the principal ingredient of bones and teeth. By its dissolution it becomes accessible to the bones and teeth; and as sugar effects this, its utility is vindicated. But a surer argument is founded on the instinct of mankind. If we all so eagerly eat sugar, it is because there is a natural relation between it and our organism.

Timid parents may therefore check their alarm at the sight of juvenile forays on the sugar-basin when not excessive; may cease to vex children by forbidding moderate commercial transactions with the lollypop merchant, and cease to frustrate their desires for barley-sugar by the never-appreciated pretext, that the interdict is "for their good."

9. *Alcohol.*—If it astonished the reader to see water and salts classed as alimentary principles, if it puzzled the housewife to see starch placed on the same list, it will we fear exasperate the members of Temperance Societies to see alcohol elevated to that rank. They are accustomed to call alcohol a *poison,* to preach against it as poisonous in large doses or small, concentrated or diluted. Nevertheless, in compliance

with the dictates of Physiology, and, let me add, in compliance also with the custom of physiologists, we are forced to call alcohol food, and very efficient food too. If it be not food, then neither is sugar food, nor starch, nor any of those manifold substances employed by man which do not enter into the composition of his tissues. That it produces poisonous effects when concentrated and taken in large doses, is perfectly true; but that similar effects follow when *diluted*, and taken in small doses, is manifestly false, as proved by daily experience.

Every person practically acquainted with the subject knows that concentrated alcohol has, among other effects, that of depriving the mucus membrane of the stomach of all its water—*i. e.* of hardening it, and destroying its powers of secretion; whereas diluted alcohol does nothing of the kind, but *increases* the secretion by the stimulus it gives to the circulation. The alcohol is always much diluted which is taken in wines or spirits.

An instructive illustration of the difference between a concentrated and diluted dose is seen in Bardeleben's experiment on dogs. He found that forty-five grains of common salt, introduced at once into the stomach through an opening, occasioned a secretion of mucus, followed by vomitings; whereas five times that amount of salt in *solution* produced neither of these effects. The explanation is simple, and will be understood by any one who has seen the salt, which was sprinkled over a round of beef, converted into brine, owing to the attraction exercised by the salt on the water in the beef: this attraction is incalculably smaller when the salt is in solution, and the salt is already saturated.

We might multiply examples of the differences which result from the use of *concentrated* and *diluted* agents, or from differences in the quantities employed; as when a certain amount of acid assists digestion, but, if increased, arrests it. But the demonstration of such a position is unnecessary, since no well-informed physiologist will deny it. The fallacy of concluding that whatever is true of a large quantity of concentrated alcohol is equally true, in a proportionate degree, of a small quantity of diluted alcohol, lies indeed at the basis of the Total Abstinence doctrine. But we need scarcely tell the student that the difference of effect is absolute: a difference in *kind*, and not simply in degree.

Another fallacy in the Teetotal argument, and one constantly invoked, is this: You must abstain altogether from alcoholic drinks, since, unless you abstain, you are certain to end in excess. Moderation necessarily becomes excess, because the frame gets habituated to stimulants; and thus, to produce the original effect, you must gradually increase the dose: according to Dr Carpenter, that which was "at first sufficient to whet the appetite and increase the digestive power, being no longer found adequate."

I have elsewhere* answered this, as well as the other fallacies of Teetotalism, and will venture to repeat a few sentences here: "He who drinks will drink again, and Moderation, we know, oils the hinges of the gate leading to Excess. Nobody doubts the danger. The only absolute preservative against taking too much, is to take none. But to suppose there is any necessary connection between moderation and excess, is to ignore physiology, and fly in the face of evidence. . . . . Men take their pint of beer, or pint of wine daily, for a series of years. This dose daily produces its effect; and if at any time thirst or social seduction makes them drink a quart in lieu of a pint, they are at once made aware of the excess. Men drink one or two cups of tea or coffee at breakfast with unvarying regularity, for a whole lifetime; but who ever felt the necessity of gradually increasing the amount to three, four, or five cups? Yet we know what a stimulant tea is; we know that treble the amount of our daily consumption would soon produce paralysis—why are we not irresistibly led to this fatal excess? Every time fresh oil is poured on fresh burning coal, the same phenomenon presents itself; every time an eel is skinned, he wriggles with ancestral vigour, and will *not* become 'used to it.' In like manner, every time a fresh stimulus is applied to fresh nerve tissue, the original effect ensues. For we must not forget this: the tissue burnt to-day is not the tissue that was burnt yesterday; the nerve particles stimulated by alcohol to-day will not be living tomorrow, when fresh stimulus is applied. Change—incessant change, is the law of our being. Fresh food renews fresh tissue for fresh stimulants to act upon. The basket is always wriggling with eels; but the eels are strangers, and can't get used to skinning."

---

* *Westminster Review.* New Series, vol. viii. Art. *The Physiological Errors of Teetotalism.* Compare, also, CHAMBERS: *Digestion and its Derangements*, p. 526-29, where some of the arguments are restated.

On the other hand, it is needless to dwell on the dangers which unhappily surround the use of alcohol. Terrible is the power of this "tricksy spirit;" and when acting in conjunction with ignorance and sensuality, its effects are appalling. So serious an influence does it exercise on human welfare, that we may readily extenuate the too frequent exaggerations of those zealous men who have engaged in a league for its total suppression. So glaring are the evils of intemperance, that we must always respect the motives of Temperance Societies, even when we most regret their exaggerations. They are fighting against a hideous vice, and we must the more regret when zeal for the cause leads them, as it generally leads partisans, to make sweeping charges, which common sense is forced to reject. All honour for the brave and sincere; all scorn for the noisy shallow quacks who make a trade of the cause!

No real gain can be achieved by any cause when it eludes or perverts the truth; and whatever temporary effect, in speeches or writings, may arise from the iteration of the statement that alcohol is poison—a poison in small quantities, as in large—always and everywhere poisonous—the cause must permanently lose ground, because daily experience repudiates such a statement as manifestly false. Alcohol *replaces* a given amount of ordinary food. Liebig tells us that, in Temperance families where beer was withheld and money given in compensation, it was soon found that the monthly consumption of bread was so strikingly increased, that the beer was twice paid for, once in money, and a second time in bread. He also reports the experience of the landlord of the Hôtel de Russie, at Frankfort, during the Peace Congress: the members of this Congress were mostly teetotallers, and a regular deficiency was observed every day in certain dishes, especially farinaceous dishes, puddings, &c. So unheard-of a deficiency, in an establishment where for years the amount of dishes for a given number of persons had so well been known, excited the landlord's astonishment. It was found that men made up in pudding what they neglected in wine. Every one knows how little the drunkard eats: to him alcohol replaces a given amount of food.

The general opinion among physiologists is, that alcohol is only heat-producing food, and that it thereby saves the consumption of tissue. Moleschott says that, although forming none of the constituents of blood, alcohol limits the combustion of those constituents, and in this way is equivalent to so much blood. "He who has little can give but little, if he wish to retain as much as one who is prodigal of his wealth. Alcohol is the savings' bank of the tissues. He who eats little, and drinks alcohol in moderation, retains as much in his blood and tissues as he who eats more, and drinks no alcohol."* But the physiological action of alcohol is still unexplained; we know that it does sustain and increase the force of the body; we know that it supplies the place of a certain quantity of food; but *how* it does this we do not know. It is said to be "burnt" in the body, and to make its exit as carbonic acid and water; but no proof has yet been offered of this assertion. Some of it escapes in the breath, and in certain of the secretions; but how much escapes in this way, and what becomes of the rest, if any, is at present a mystery.

---

* MOLESCHOTT: *Lehre der Nahrungsmittel*, p. 162.

10. *Iron.*—We are passing from surprise to surprise as we in turn arrive at substances undoubtedly claiming rank among alimentary principles, which nevertheless, in the ordinary conceptions of men, are the very opposites of Food. After water, phosphates, starch, and alcohol, are we now to celebrate the nutritive qualities of iron? Even so. That metal circulates in our blood, forming indeed an *essential* element of the blood-discs—existing in all pigments—in the bile—in various other places—notably in the hair, where it is abundant in proportion to the darkness of the colour.

The quantity of iron in the blood is but small; varying in different individuals, and different states of the same individual; those who are of what is called the sanguine temperament have more than those of the lymphatic temperament; those who are well-fed have more than those who are ill-fed. It is in almost all our animal and vegetable food, so that we do not habitually need to seek it; but the physician often has to prescribe it, either in the form of "steel-wine," or in that of chalybeate waters.*

11. *Phosphorus* and *Sulphur* are also indispensable, but they are received with our food.

12. *Acids* are received with vegetable food; but they are also taken separately, especially the acetic acid, or vinegar, which, according to Prout, has either by accident or design been employed by mankind in all ages—that is to say, substances naturally containing it have been employed as aliments, or it has been formed artificially.

It is owing to their acids that fruits and vegetables are necessary to man, although not necessary to the carnivora. Dr Budd justly points to the prolonged abstinence from succulent vegetables and fruits as the cause of the scurvy among sailors. Lemon-juice is now always given to sailors with their food; it protects them from scurvy, which no amount of vinegar is sufficient to effect.

We make cooling drinks with vegetable acids; and our salads and greens demand vinegar, as our cold meat demands pickles. Taken in moderation, there is no doubt that vinegar is beneficial, but in excess it impairs the digestive organs; and, as we remarked a little while ago, experiments on artificial digestion show that if the quantity of acid be diminished, digestion is retarded; if increased beyond a certain point, digestion is arrested.

---

* "It is quite certain that if iron be excluded from food, organic life cannot be supported."—LIEBIG.

There is reason, therefore, in the vulgar notion, unhappily too fondly relied on, that vinegar helps to keep down an alarming adiposity, and that ladies who dread the disappearance of their graceful outline in curves of plumpness expanding into "fat," may arrest so dreadful a result by liberal potations of vinegar; but they can only so arrest it at the far more dreadful expense of their health. The amount of acid which will keep them thin, will destroy their digestive powers.

Portal gives a case which should be a warning: "A few years ago a young lady in easy circumstances enjoyed good health; she was very plump, had a good appetite, and a complexion blooming with roses and lilies. She began to look upon her plumpness with suspicion; for her mother was very fat, and she was afraid of becoming like her. Accordingly, she consulted a woman, who advised her to drink a glass of vinegar daily: the young lady followed her advice, and her plumpness diminished. She was delighted with the success of the experiment, and continued it for more than a month. She began to have a cough; but it was dry at its commencement, and was considered as a slight cold, which would go off. Meantime, from dry it became moist; a slow fever came on, and a difficulty of breathing; her body became lean, and wasted away; night-sweats, swelling of the feet and of the legs succeeded, and a diarrhœa terminated her life."

Therefore, young ladies, be boldly fat! never pine for graceful slimness and romantic palor; but if Nature means you to be ruddy and rotund, accept it with a laughing grace, which will captivate more hearts than all the paleness of a Circulating Library. At any rate, understand this, that if vinegar will diminish the fat, it can only do so by affecting your health.

We have thus touched upon the chief Alimentary Principles, and will now review the Compound Aliments, or those articles of Food and Drink which constitute and vary our diet.

CHAPTER II.

(*Continued.*)

FOOD AND DRINK.

SECTION III.—ARTICLES OF FOOD.

Meats—The flavouring substance of meat, *osmazome*—The flesh of young animals—The age of animals in relation to the digestibility of their flesh—Effects of roasting, boiling, and baking—German and French meat — Digestibility of different meats — Sweetbread and tripe—Horse-flesh as food: experiments to establish the excellence of horse-beef—Quantities of horse-flesh consumed in various countries—The flesh of donkeys—Fish: digestibility of different kinds—Nutritive quality of fish—Are fish-eaters unusually prolific?—Eggs—Pastry: is it injurious?—Vegetables—Vegetarianism—Tea, coffee, and the narcotics: curious physiological paradox of their action—The quantity of food requisite for man—Individual varieties in the quantity needed—Error of applying arithmetic to vital problems—The gluttony of some races—Does a cold climate produce inordinate eating?—The rations of paupers and soldiers.

MAN is said, with but slight exaggeration, to be omnivorous. If he does not eat of all things, he eats so multifariously, that our limits would be insufficient to include even a superficial account of all the substances employed by him as Food. We must therefore be content to let attention fall on the principal groups.

1. *Meats.*—It is superfluous to dwell on the fact that the flesh of most herbivora, both wild and domestic, is both agreeable and nutritious; even the advocates of a purely vegetable diet do not dispute the flavour or the potency of flesh, whatever consequences they may attribute to the eating of it. It contains some of the chief alimentary principles: namely, albumen, fibrine, fat, gelatine, water, salts, and *osmazome.*

The last-named is a substance of reddish-brown colour, having the smell and flavour of soup (whence the name—δσμη, smell, and ζωμος, soup); it varies in various animals, increasing with their age. It is this osmazome, developed during the process of cooking, which gives their characteristic flavours to beef, mutton, goat-flesh, and birds, &c.

The flesh of young animals is tenderer than that of adults; and tenderness is one quality which favours digestibility. Nevertheless we shall err if, fixing our attention on this one quality, we assume that the flesh of young animals is always more digestible than that of adults; we shall find veal to be less so than beef, and chicken less so than beef. The reason given for the first of these exceptions is, that veal has less of the peculiar aroma developed in cooking; the reason given for the second is, that the texture of chicken is closer than that of beef, and, being closer, is less readily acted on by the gastric juice. Every one knows that veal is not very digestible, and is always shunned by the dyspeptic. On the other hand, in spite of chicken being less digestible than beef, it is more suitable for a delicate stomach, and will be assimilated when beef, or other meat, would not remain in the stomach,—an example which shows us that even the rule of nutritive value, being determined in a great measure by digestibility, is not absolute; and which further shows how cautious we should be in relying upon general rules in cases so complex.

The age of animals is very important. Thus the flesh of the kid is very agreeable; but as the kid approaches the adult period, there is so pronounced an odour developed from the hircic acid in its fat, that the flesh becomes uneatable. Whereas the ox and cow, fattened for two years after reaching full growth, have acquired the perfection of their aroma and flavour. The difference between lamb and mutton is very marked, especially in their fat, that of mutton containing more fatty acid, and being to many stomachs quite intolerable.

"All the savoury constituents of flesh are contained in the juice, and may be entirely removed by lixiviation (process of dissolving) with cold water. When the watery infusion of flesh thus obtained, which is commonly tinged red by some of the colouring matter of the blood, is gradually heated to boiling, the *albumen of flesh* separates, when the temperature has risen to 133° Fahr., in nearly colourless cheesy flocculi; the colouring matter of the blood is not coagulated till the temperature rises to 158°.

"The proportion of the albumen of flesh separated as a coagulum by heat is very various, according to the age of the animal. The flesh of old animals often yields no more than 1 or 2 per cent; that of young animals as much as 14 per cent.

"The infusion or extract of flesh, after being freed by boiling from the albumen and colouring matter of the blood, has the aromatic taste, and all the properties of soup made by boiling the flesh. The residue of flesh, after exhaustion with cold water, is of the same quality in different animals; so that it is impossible in this state to distinguish beef from poultry, venison, pork, &c. On the other hand, the soup made of the flesh of different animals possesses, along with the common flavour in which all soups resemble one another, in each case a peculiar taste, which distinctly recalls the smell or taste of the roasted flesh of the animal; so that if we add to the boiled and exhausted flesh of roe-deer the concentrated juice of beef or poultry, the meat thus prepared cannot be distinguished by the flavour from roast-beef or fowl.

"The fibre of meat is, as we see from these facts, in its natural state steeped in and surrounded by a liquid containing albumen; and the tender quality of boiled or roasted meat depends on the amount of albumen deposited in its substance, and there coagulating, whereby the contraction, toughening, and hardening of the fibres are prevented. Meat is underdone, or bloody, when it has been heated throughout only to the temperature of coagulating albumen, or 133° Fahr.; it is quite done, or cooked, when it has been heated through its whole mass to between 158° and 165°."*

We may now understand the effects of cooking. When meat is *Roasted*, the outer layer of its albumen is coagulated, and thus presents a barrier to the exit of all the juice. To have a good and juicy roast, it is therefore necessary that the heat should be strongest at first. Let your cook, therefore, be careful not to set the joint down until the fire is vigorous and red. The heat may afterwards be much reduced. Besides this effect on the albumen, roasting converts the cellular tissue into gelatine ready for solution, and melts the fat out of the fat-cells.

---

* LIEBIG: *Chemical Letters*, 1859, pp. 435-36.

In *Rapid Boiling* a somewhat similar result is effected, except that the albumen becomes less soluble. "If the flesh be introduced into the boiler," says Liebig, "when the water is in a state of brisk ebullition, and if the boiling be kept up for a few minutes, and the pot then placed in a warm place, so that the temperature of the water is kept at 158° to 165°, we have the united conditions for giving to the flesh the qualities which best fit it for being eaten."

In *Slow Boiling* a very different result is obtained. All the juices are extracted in the form of Soup, leaving a stringy mass of flesh behind. The thinner the piece of meat, the greater is its loss of savoury juices. To make the *best* soup we must chop the meat into fine pieces, and then, adding an equal weight of water, let the whole be slowly heated to the boiling point; it must be kept boiling for a few minutes, and then strained and pressed. We have then the strongest and most highly-flavoured soup that can be made from flesh; after which it may be diluted according to taste.

Baking exerts some unexplained influence on the meat, which renders it both less agreeable and less digestible. Those who have travelled in Germany and

France must have repeatedly marvelled at the singular uniformity in the flavour, or want of flavour, of the various "roasts" served up at the *table-d'hôte.* The general explanation is, that the German and French meat is greatly inferior in quality to that of England and Holland, owing to inferiority of pasturage; and, doubtless, this is one cause, but it is not the chief cause. The meat is inferior, but the cooking is mainly at fault. The meat is scarcely ever *roasted,* because there is no coal, and firewood is expensive. The meat is therefore *baked;* and the consequence of this baking is, that no meat is eatable, or eaten, with its own gravy, but is always accompanied by some sauce more or less piquant. The Germans generally believe that in England we eat our beef and mutton almost raw; they shudder at our gravy, as if it were so much blood.

I have ascertained that it is really the cooking, and not the meat, which is in fault; for at the tables of great people, or resident English, where *roast* meat is served, the flavour is excellent. Moreover, the game, at a *table-d'hôte,* is almost as tasteless as the poultry, partridges having little of their well-known flavour:

> "Longe dissimilem noto celantia succum;" *

and hare, that "well-flavoured beast," eulogised by Charles Lamb, is rendered undistinguishable from beef, except perhaps in tenderness; while venison may be mistaken for kid.

In the inferior eating-houses of London the meat is also baked, for the sake of economy, and is notoriously deficient in that agreeable flavour which roast-meat possesses. If it were baked as thoroughly as it is in Germany, the meat in such eating-houses would be as tasteless.

---

* HORACE, *Sat.* ii. 8, makes this complaint of bad cookery in his day.

Dr Beaumont has drawn up tables of the comparative digestibility of various substances, to which succeeding writers have referred, without always perceiving that Dr Beaumont's observations, being confined to what takes place in the stomach, which is only *one* part of the digestive process, do not throw any light upon what takes place in the intestines—by far the more important part of the process[*]—and can only have a limited value, because they can only apply to those substances which are in any degree influenced by the gastric juice. Bearing this in mind, and accepting the following figures as indications only, they will be found useful—

		Hour.	Min.
Venison steak, broiled, .	requires	1	35
Pig, sucking, roasted, .	„	2	30
Lamb, fresh, broiled, . .	„	2	30
Beef, with salt only, boiled,	„	2	45
Beef, fresh, lean, roasted, .	„	3	0
Beef-steak, broiled, . .	„	3	0
Pork, recently salted, raw, .	„	3	0
Pork, recently salted, stewed,	„	3	0
Mutton, fresh, broiled, . .	„	3	0
Mutton, fresh, boiled, . .	„	3	0
Pork, recently salted, broiled,	„	3	15
Pork steak, broiled, . .	„	3	15

---

[*] See the next chapter, DIGESTION AND INDIGESTION.

		Hour.	Min.
Mutton, fresh, roasted, .	requires	3	15
Beef, fresh, lean, dry, roasted,	,,	3	30
Beef, with mustard, &c., boiled,	,,	3	30
Beef, with mustard, &c., fried,	,,	4	0
Veal, fresh, broiled, . .	,,	4	0
Beef, old, hard, salted, boiled,	,,	4	15
Veal, fresh, fried, . .	,,	4	30
Pork, fat and lean, roasted, .	,,	5	15

As may be expected, the flesh of different parts has different qualities: the breast of birds, with its pectoral muscles, which move the wings, is tenderer than the legs; but the flesh of the legs, when the birds are young, is more juicy and savoury than that of the wings; and in the woodcock, old or young, the legs are always preferred, while in the partridge epicures select the wings.

The flesh of game is richer in osmazome than that of domestic birds; and when the bird has been kept till it is "high," it has—especially in the back—an aromatic bitter flavour very acceptable to epicures, but very nauseous to unsophisticated palates.

The flesh of all waterfowl, especially the goose, is penetrated with fat, which often becomes rancid and "fishy:" this renders the goose so notorious an offender, that he has to be "qualified" by a little brandy, euphuistically styled "Latin for goose." Dr Beaumont found no difference between the digestibility (in the stomach) of roast goose and roast turkey, both requiring two hours and a half; but it should be observed that the fats are not digested at all in the stomach, and it is on the fats that the real digestive difference between goose and turkey depends. Turkey, roasted, requires two hours and a half for digestion; fowl, roasted, four hours, and ducks the same.

Besides the meat (muscle), the brains, livers, kidneys, and sweetbread of various animals are eaten. On account of the fat and oil contained in brain and liver, they are unsuitable for delicate stomachs, especially when fried. Kidneys are very tough, and difficult of digestion. Sweetbread forms a favourite food with convalescents, when plainly dressed; its composition in 100 parts is as follows—

Albumen, ...	14.00
Osmazome, ...	1.65
Gelatine, ...	6.00
Animal fat, ...	0.30
Margaric acid,*	0.05
Fibrine, ...	8.00
Water, ...	70.00
	100.00

An excellent food, too much neglected, is Tripe, which is simply the stomachs of ruminant animals. As it contains a large proportion of albumen and fibrine, and requires not more than one hour for its digestion in the stomach, we see the justification of the practice popular in many families, of having Tripe for supper. There is no nightmare in it.

---

* Margaric acid is one of the fatty acids, and is produced from margarin, a pearly fat found in olive oil, goose grease and human fat.

2. *Horse-flesh.*—A Frenchman was one day blandly remonstrating against the supercilious scorn expressed by Englishmen for the beef of France, which he, for his part, did not find so inferior to that of England. "I have been two times in England," he remarked, "but I nevère find the bif so supérieur to ours. I find it vary conveenient that they bring it you on leetle pieces of stick, for one penny; but I do not find the bif supérieur." On hearing this, the Englishman, red with astonishment, exclaimed, "Good God, sir! you have been eating cat's meat."

It is very true, he *had* been eating cat's meat; but had he not at the same time been eating meat as succulent, savoury, and wholesome as the marbled beef of which the Briton is so proud?

Let the resonant shouts of laughter subside a little, and while you are wiping the tears from your eyes, listen to the very serious exposition we shall make of the agreeable and nutritive qualities of horse-flesh. We are not going to press into the service of our argument the immense mass of evidence collected by M. Isidore Geoffroy St Hilaire,* respecting the tribes and nations which habitually dine off horses; nor will we lay much stress on the fact, that in the Jardin des Plantes the carnivora are habitually fed on horse-flesh, which keeps them healthy in spite of many unfavourable conditions. The sceptic might not unreasonably ask whether our digestive power be *quite* as good as that of the lion; and he would remark that the condor is known to devour, with relish, food which Mr Brown would sturdily refuse. Unhappily no dietetic rules for men can be deduced from condors and lions. We must rely on the experience of human stomachs.

---

* *Lettres sur les Substances Alimentaires, et particulièrement sur la Viande de Cheval.* 1856.

Nor is this experience wanting. Without alluding to the rumours which attribute to the Paris restaurateurs a liberal employment of horse-flesh among their *filets de bœuf*, M. St Hilaire collects an imposing mass of evidence to show that horses have been eaten in abundance without suspicion, and without evil consequences.

Huzard, the celebrated veterinary surgeon, records, that during the Revolution the population of Paris was fed for six months on horse-flesh. It is true, that when the beef was known to be that of horses, some complaints were made; but in spite of the strong prejudices, and the terrors such a discovery raised, no single case of illness was attributable to this food.

Larrey, the great army-surgeon, declares, that on very many occasions during the campaigns, he administered horse-flesh to the soldiers; and what is more, he administered it to the sick in the hospitals. Instead of finding it injurious, he found that it powerfully contributed to their convalescence, and drove away a scorbutic epidemic. Other testimony is cited, and M. St Hilaire feels himself abundantly authorised to declare that horse-flesh is as wholesome and nutritious as ox-flesh.

Is horse-flesh as palatable as it is wholesome? Little will it avail to recount how there are tribes of hippophagists, or how soldiers during a campaign, and citizens during a siege, have freely eaten of the *filet de cheval:* under such extremities an old shoe has not been despised—nevertheless, *that* is not generally considered a toothsome morsel.

Feeling the necessity of having this point definitively settled, the advocates of horse-flesh have given banquets, both in Germany and France, at which the comparative merits of horses, cows, and oxen were to be appreciated. In 1835 the Prefect of Police chose a commission of eminent men to inquire into the quality of the flesh taken from horses which had died, or had been recently killed, in Paris and its environs. These commissioners all shared the general prejudice; yet in their report they avowed that "we cannot but admit this meat to be very good and very savoury; several members of the commission have eaten it, and could not detect any sensible difference between it and beef." In 1841, horse-flesh was openly adopted at Ochsenhausen (what irony in this name!) and Würtemberg, at both of which places it continues to be publicly sold, under the surveillance of the police; and five or six horses are weekly brought to market. In 1842, a banquet, at which a hundred and fifty persons assisted, inaugurated its public use at Königsbaden, near Stuttgard. In 1846 the police of Baden authorised its public sale; and Schaffhausen followed the example. In 1847, Weimar and Detmold witnessed public banquets of the hippophagists, which went off with *éclat;* in Karlsbad and its environs the new beef came into general use; and at Zittau two hundred horses are eaten annually. The innovation gained ground rapidly, and the public sale of horse-flesh is now general in Austria, Bohemia, Saxony, Hanover, Switzerland, and Belgium. In 1853, Berlin counted no less than five slaughter-houses, where three hundred and fifty horses were sold. In Vienna, during the same year, there was a riot to prevent one of these banquets; yet, in 1854, such progress had been made in public opinion that thirty-two thousand pounds' weight were sold in a fortnight, and now at least ten thousand of the inhabitants are hippophagists. A large quantity is also sold at the Lake of Constance, as I learned in 1858; and I *suspect* that I ate thereof on board the steamer.

These facts are very striking. When we consider, on the one hand, how strong is prejudice, and, on the other, how unreasoning the stomach, we must admit that horse-flesh could only gain acceptance in virtue of its positive excellence. Nor will it suffice to meet these facts with a sarcasm on German beef, in comparison with which horse-flesh may be supposed to hold no dishonourable rank: we have the testimony of men accustomed to the *Café de Paris* and *Philippe's,* invited expressly to pronounce judgment, and proved, on trial, incapable of distinguishing horse-beef from ox-beef.

M. Renault, the director of the great veterinary school at Alfort, had a horse brought to the establishment with an incurable paralysis. It was killed; and three days afterwards, on the 1st December 1855, eleven guests were invited to dine off it: they were physicians, journalists, veterinary surgeons, and *employés* of the Government. Side by side were dishes prepared by the same cook, in precisely similar manner, consisting of similar parts of the meat from this horse, and from an ox of good quality. The horse-soup was flanked by an ox-soup—the *bouilli* of horse by a *bouilli* of beef—the fillet of roast-beef by a fillet of roast-horse. The guests *unanimously* pronounced in favour of the horse-soup; the *bouilli*, on the contrary, they thought inferior to that of the ox, though superior to ordinary beef, decidedly so to cow-beef. The roast fillet, again, seemed to them very decidedly in favour of the horse.

Similar experiments have been subsequently repeated in Paris and the provinces, under varying conditions: the guests have sometimes been informed what they were going to eat; sometimes they have been totally unsuspecting; and sometimes they have been simply told that they were going to eat something quite novel. Yet in every case the result has been the same.

It is on this evidence that M. St Hilaire calls upon the French people to turn their serious attention to the immense mass of excellent animal food which lies within their reach, and which they annually suffer to waste, merely because of an absurd prejudice. Difficult as it may be to overcome a prejudice, no array of ignorance can prevent the establishment of a truth which is at once easily demonstrable and immediately beneficial. Prejudice may reject horse-flesh, as it long rejected tea and potatoes, the latter of which, Montaigne tells us, excited *l'estonnement et le dégoût*, but has nevertheless become European food.

If horses are eaten, why not donkeys? The Greeks ate donkeys, and we must suppose they had their reasons for it. Has any modern stomach been courageous enough to try?

Yes, the experiment has at least been made once. Dr John Beddoe of Clifton sends me the following statement:—"Several years ago, I entertained six or eight medical students with a dinner, at which the *pièce de resistance* was nothing else than the hind-leg of a donkey which had been sacrificed to a physiological experiment. One or two of my guests were in the secret: to the others I represented the meat to be part of a fawn. They all partook of it, and 'even asked for more.' In flavour and appearance it most resembled mutton; but though young it was far from tender, and I did not care to repeat the experiment, to which I had been instigated partly by a passage in an Arabian tale, in which one of two gastronomic disputants, in reply to a commendation of a shoulder of lamb, is made to say, 'You know nothing: what say you to the neck of a young ass carefully roasted?'" Although Dr Beddoe does not report very favourably of the tenderness, his guests found the meat sufficiently appetising, since they "asked for more." Besides, the animal may have been too fresh; another day or two might have given it the requisite tenderness. The point is worth investigating; for if the horse and donkey can be introduced among our meats, thousands who now rarely touch animal food may be supplied. So many horses are killed by accident, or killed because they are lame or vicious, that the supply would be enormous, without any necessity for killing horses expressly.

A new and noble kind of meat has recently been suggested by Prof. Owen, whose account of the flavour and capabilities of the Eland will doubtless induce noblemen to introduce that animal into their parks.

3. *Fish* is largely eaten by all classes, and is certainly nutritious. Great differences are noticeable in the different kinds. Many kinds have large quantities of oil —as the eel, salmon, herring, pilchard, and sprat; and these are therefore the least digestible. The oil is most abundant in the "thin" parts of salmon, which are consequently preferred by epicures. After spawning, the quality is very inferior. In the cod, whiting, haddock, plaice, flounder, and turbot, there is no oil except in their livers, so that these are easily digested, especially if they are not eaten with quantities of lobster or shrimp sauce, agreeable adjuncts very apt to exact large compensation from the delicate in the shape of acidity and flatulence.

Frying, of course, renders fish less digestible than boiling or broiling; and those whose digestions are delicate should avoid the skin of fried fish. They should also avoid dried, smoked, salted, and pickled fish; crabs, lobsters, prawns, and shrimps. The oyster is most digestible when raw, least so when stewed. Dr Beaumont found the raw oyster took 2 hours 55 minutes to digest, the roasted oyster 3.15, and the stewed 3.30. What is called "scalloping" gives oysters a delicious flavour, but the heat coagulates the albumen and hardens the fibrine; besides, the effect of heat on the butter in which they are cooked renders it very unfit for the delicate stomach.

Respecting the nutritive quality of fish, opinions are divided. Let us hear old Leeuwenhoek. "It is the opinion of many medical persons," he says, "that various disorders in the human frame are caused by acid in the stomach, which coagulates the juices (!); and some condemn the use of acids, and also of fish, as articles of food. But to these opinions I cannot subscribe; for at a town in my neighbourhood, where the people get their living by fishing, and feed principally on fish, especially when they are on the sea, the men are very robust and healthy even to a great age; and with respect to myself, I have experienced that when my habit of body has been indisposed, I have been greatly refreshed by eating fish with sauce composed of a mixture of butter and vinegar, and I never found acid sauces disagree with me. It is also my opinion that a fish diet is more wholesome than flesh, particularly to those persons who do not use much exercise, because fish is more easily comminuted and digested in the stomach and bowels than flesh."*

---

* LEEUWENHOEK: *Select Works*, i. 154.

But while fishermen are robust on a fish diet, it is notorious that those accustomed to meat find a certain debility follow the adoption of an exclusively fish diet —during Lent, for instance; and jockeys, when "wasting" themselves at Newmarket, take fish in lieu of meat. Lehmann cites the analyses of Schlossberger, which show "that the amount of nitrogen in muscular fibre is throughout the animal kingdom essentially similar. The flesh of fish contains the same amount as that of the higher animals; oysters, on the contrary, instead of containing more, as common experience would lead us to conjecture, actually contain less."* There is, however, as we have seen, a remarkable difference between being rich in nitrogen, and being good food. One reason why fish is less nutritious than flesh, in spite of the similarity in their composition, is said to be the absence of the osmazome which gives flavour to flesh.

One of the popular notions entertained even by some medical men is, that eating fish increases fertility, and that the fish-eating tribes are unusually prolific. We need not pause to refute the physiological arguments on which this opinion is founded, as the fact asserted, of fish-eating tribes being very prolific, is itself a fiction. Dr Pereira remarks:—

"There is, I think, sufficient evidence to prove that the ichthyophagous people are not more prolific than others. In Greenland and among the Esquimaux, says Foster, where the natives live chiefly upon fish, seals, and oily animal substances, the women seldom bear children oftener than three or four times: five or six births are reckoned a very extraordinary instance. The Pesserais whom we saw had not above two or three children belonging to each family, though their common food consisted of mussels, fish, and seal-flesh. The New Zealanders absolutely feed on fish, and yet no more than three or four children were found in the most prolific families."[*]

---

* LEHMANN: *Physiol. Chemie*, iii. 351.

4. *Eggs* are very nutritious, especially when poached or lightly boiled; when boiled hard, or fried in butter, they are difficult of digestion; and the same may be said of omelettes, pancakes, and fritters.

But here, as indeed in all other cases, only general empirical rules can be laid down—rules which individual experience must rectify or confirm. There are persons who cannot eat the white of egg; there are persons who cannot eat the yolk; and there are others who cannot eat egg in any shape whatever. To some persons of delicate digestion, eggs are found very suitable; while to others, whose digestion is generally good, they are hurtful. "In short," says Leeuwenhoek, "we can much better judge for ourselves as to what agrees or disagrees with us, than pretend to advise other people what is good diet, or the contrary."[†] Experience, enlightened by vigilant good-sense, can alone determine such questions for each person. It is idle to assure a man who finds eggs disagree with him, that "they are really very wholesome;" and not less idle to warn him against eggs, or anything else, which his experience pronounces beneficial. The blissful being who knows not, except by rumour, what is the difference between digestible and indigestible, may smile at Science and its exhortations; the miserable being whose stomach painfully obtrudes itself upon his consciousness by importunities not to be evaded, and by clamours not to be outargued, may gather some guiding light from general rules, and thus by vigilance arrive at positive results for himself.

---

[*] Pereira: *On Diet*, p. 282.

[†] Leeuwenhoek: *Select Works*, i. 158.

5. *Pastry.*—There are two kinds of pie-crust, called "puff" and "short" paste; of these, the latter is the most digestible, because the butter is thoroughly mingled with the dough, and is by this means in that state of minute subdivision which, when treating of Fats and Oils, we saw to be necessary for its proper digestion; morever, the starch is also thus more equally distributed.

In puff pastry this is not the case: the dough forms itself into thin and solid layers. "All pastry," according to Dr Paris, "is an abomination. I verily believe that one-half of the cases of indigestion which occur after dinner-parties may be traced to this cause."

A hard sentence, this, on juveniles and pastry-lovers; but in mitigation, one may suggest that the offences of pastry lie less in its own sinful composition, than in the fact of its succeeding a chaos of meats, made-dishes, and mingled vintages. The gentleman who was found reeling forlorn and helpless against the railings, on his way home after dinner with a friend, hiccuped energetic denunciations against that "knuckle of ham" which had taken the steadiness from his legs and the singleness from visual objects; in like manner the tart, which is innocent when following a simple joint, may become as guilty as the knuckle of ham, at the rear of an elaborate dinner. We are all apt to over-eat ourselves, and then we throw the blame of our imprudence on some article of food not in itself more objectionable than the others.

6. *Vegetables.*—The immense variety of vegetable food cannot, of course, even be indicated in so rapid a survey as this. A volume might be written on the bread-plants alone. The tropical: rice, plantain, yam, sweet-potato, chayote, arrow-root, cassava, bread-fruit, sago, cocoa-nut, taro, and date; and the extra-tropical: wheat, rye, barley, oats, buckwheat, and potatoes; with maize, which is common to both regions—these alone support millions of human beings, and are justly named "the staff of life."

The tropical plants yield more than the others; wheat yields on an average only five or six fold in northern Europe, and eight or ten fold in southern Europe; but rice yields a hundred-fold. The plantain yields 133 times as much food as wheat on the same area. With a small garden round his hut the peasant can support his family. And how easy is subsistence in the Asiatic Archipelago, where sago grows wild in the woods, and a man goes into the forest to cut his bread, as we to cut our firewood. He fells the tree, divides it into several pieces, scrapes the pith out, mixes it with water, strains it, and there is sago-meal ready for use.[*]

The bread-countries have been thus geographically indicated by Schouw: — "The *bread-line* extends furthest north in Scandinavia, for in Finmark we meet —only within the fiords, it is true—with barley and potatoes up to 70° N. latitude; from here it sinks both to the east and west. It is well known that neither Iceland nor Greenland possess bread-plants, although the south coast of the former lies in 63½°, and that of the latter in 60° N. latitude; and that in the Feröe Islands, although lying between 61½° and 62½°, there exists but an inconsiderable cultivation of barley. On the east side of North America the bread-line sinks still further to the south, for Labrador and Newfoundland have no bread-plants, and the limit can scarcely be put here higher than 50°, consequently much further south than in Denmark, where the plains abound in corn. It extends a little further north on the western coast of North America, which, as is well known, possesses a warmer climate than on the east side. The few data which we find here, render the determination of the north limit rather uncertain; it can scarcely be placed higher than 57° or 58°. Turning from Scandinavia towards the east, we find a depression of the bread-line even in European Russia, here coming by 67° northward of Archangel. The curve is considerable in Asiatic Russia; at Ob the north limit of bread comes to 60°, at Jenesi to 58°, at Lena 57½°, and in Kamtschatka, which has only a slight cultivation of corn in the most southern part, it sinks to 51°—thus to about the same latitude as on the east coast of North America. The bread-line has thus two polar and two equatorial curves, the former corresponding to the western, the latter to the eastern sides of the continent."[*]

---

[*] SCHOUW: *The Earth, Plants, and Man* (Trans.), p. 131.

Vegetarianism.—On surveying the list of nations and tribes whose food is principally, or entirely, vegetable, we are naturally led to ask what confidence is due to that party in America and England which proclaims Vegetarianism to be the proper creed for civilised men, and vegetable food the healthiest and suitablest in every way. Many years ago, I was myself a convert to this doctrine, seduced by the example and enthusiasm of Shelley; and, for the six months in which I rigidly adhered to its precepts, could find no sensible difference, except that I was able to study immediately after dinner. It soon became clear, however, that the arguments on which the doctrine rests for support would not withstand physiological scrutiny.

It is unnecessary to do more than allude to such fantastic arguments as that of Rousseau, who maintained vegetables to be our proper food, because we have two breasts, like the vegetable feeders; an argument as worthless as the counter-argument of Helvetius, that flesh is our only proper food, because we have the blind intestine short, like the flesh-feeders. The vegetarian theory is at variance with the plain indications afforded by our structure, and by the indications, no less plain, afforded by our practice. The structure of our teeth and intestinal canal points to a mixed diet of flesh and vegetable; and although the practice of millions may be to avoid flesh altogether, it is equally the practice of millions to eat it. In hot climates there seems little or no necessity for animal food; in cold climates it is imperatively demanded. In moderate climates, food is partly animal and partly vegetable. Against instinct, so manifested, it is in vain to argue; any theory of food which should run counter to it stands self-condemned.

---

[*] SCHOUW: *The Earth, Plants, and Man* (Trans.), p. 131.

Besides this massive evidence, we have abundant examples in individual cases to show how necessary animal food is for those who have to employ much muscular exertion. The French contractors and manufacturers who were obliged to engage English navvies and workmen, because French workmen had not the requisite strength, at last resolved to try the effect of a more liberal meat diet; and by giving the Frenchman as ample a ration of meat as that eaten by the Englishman, the difference was soon reduced to a mere nothing. It is worth noting that the popular idea of one Englishman being equal to three Frenchmen, was found by contractors to be tolerably accurate, one Englishman really doing the work of two and a half men; and M. Payen remarks that the consumption of mutton in England is three times as much as that in France, in proportion to the inhabitants.*

It is a fact of very great importance that the chemical constitution of the Blood is notably altered by an exclusively vegetable diet. Verdeil discovered that the blood of animals fed exclusively on flesh contained a quantity of phosphates, while the carbonates had disappeared; on the contrary, the same animals fed on vegetables had blood rich in carbonates, with but little of the phosphates. A dog was fed for a fortnight on flesh, and the ashes of its blood yielded as much as 12 per cent of phosphoric acid, combined with alkalies. The same dog fed on bread and potatoes showed only 9 per cent; and if it could have been fed on green vegetables, there would not have been more than 2 or 3 per cent—as in the blood of sheep and oxen.*

---

* PAYEN: *Des Substances Alimentaires*, p. 8.

There are many conclusions to be drawn from this fact. One only need be hinted at here. Those who have any predisposition to the terrible disease called "the stone" should abstain as much as possible from animal diet, bread, and peas, since it is thence that the phosphoric acid in excess is derived, which will form the phosphate usually characteristic of the "stone." By a vegetable diet the blood is rendered alkaline; this will enable the uric acid to remain soluble; and thus, even if a stone be already formed, its progress will be arrested.

Tea, Coffee, Chocolate, Wines, and *Beers,* have been so amply and lucidly treated of by Johnston in his *Chemistry of Common Life,* that we need say nothing of them in this place, except to remark that they are all undeniably nourishing, although seemingly incapable of entering into the composition of any tissue, so that their physiological value is still a mystery.

---

* VERDEIL in *Comptes Rendus de la Société de Biologie,* 1849, p. 71.

There are few facts better established than that Tea and Coffee, no less than Wine and Beer, and Narcotics, *increase* the activity and power of the organism, while they *diminish* its waste. In Dr Böcker's experiments on himself, it appeared that when for seven days, consecutively, he drank only water with *insufficient* food, he daily lost 12 ounces avoirdupois, more than when, with the *same* amount of food, he drank tea. Moreover, when he took a *sufficient* quantity of food he gained weight, if tea were added. He found tea very materially diminish the amount of urea and other excretions; and it also limited the amount lost in perspiration.

In Dr Julius Lehmann's experiments on himself, with coffee, similar results were reached. They are thus stated by Dr Chambers:[*]

1. That coffee produces on the organism two chief effects which it is very difficult to connect together—viz. the raising the activity of the circulating and nervous systems, and remarkably retarding the decomposition of the tissues.

2. That it is the reciprocal modifications of the empyreumatic oil and caffeine contained in the bean which call forth the stimulant effects of coffee.

3. That the lessening of the changes of decomposition which this beverage produces in the body is chiefly caused by the empyreumatic oil.

---

[*] CHAMBERS: *Digestion and its Derangements*, p. 249.

"What an important effect this is!" exclaims Dr Chambers. "The tea and coffee drinker may have less to eat and yet lose less weight—wear his body out less—than the water-drinker. At a comparatively small expense he may save some of the costly parts of his diet, those nitrogenised solids that entail so much thought, labour, and anxiety to obtain." The same is true of Alcohol, and all the Narcotics, as is irresistibly proved by Von Bibra in his comprehensive work.*

Let me call attention to a paradox which physiologists have seemingly overlooked, namely, that the physiological canon of "all activity being dependent on waste of tissue," seems here contradicted by a class of substances which notoriously increase the activity, and demonstrably decrease the waste. It is held that every evolution of force is produced by some chemical change; and every change tends towards final decomposition. In a vast variety of cases this is demonstrable. In the cases now under consideration nothing of the kind is apparent—the tissues are more active, and the waste is less. And what makes the paradox more striking is the fact that none of these substances enter, as such, into the composition of the tissues. They must be supposed, therefore, to act upon them; but how can they act upon them, evolving force, without producing decomposition?

I instituted some experiments with a view of determining, if possible, what the precise action was, and whether it was similar in each case. The fact that we ordinarily employ alcohol to *preserve* animal preparations—to protect them from that decomposition which would otherwise ensue if the substances were left to themselves—suggested the idea that when we drink alcohol, in wine, beer, or spirits, the action on our tissues may be of the same nature, only less energetic. If a *temporary* suspension of the inevitable process of decomposition were effected by alcoholic drinks—if the alcohol acted on the living as on the dead tissue, and arrested its molecular changes—we should then clearly understand how it is that alcoholic drinks diminish waste, how they are the "savings-bank of the tissues," and how they lessen the quantity of food which is needful. But the mystery would still remain how such an arrest of change could be coincident with an increase of power.

---

* VON BIBRA: *Die narkotischen Genussmittel und der Mensch.* Compare also JOHNSTON: *Chemistry of Common Life:* "The Narcotics we indulge in."

The point I endeavoured to establish was this: Are the tissues *preserved* by decoctions of Tea, Tobacco, and Coffee, in the same way as by Alcohol? The experiments gave a decided negative; and I am now disposed to doubt whether the action of Alcohol on the living tissues, when taken in the highly-diluted form of wine or spirits, has any notable resemblance to that which concentrated Alcohol has on dead tissue. Consequently, the mode of action of tea, coffee, and narcotics has yet to be explained; and when explained, there will still remain the paradox of increased activity with diminished waste.

We have thus surveyed the great varieties of Food, and have seen how far Science is from any accurate data respecting the nutritive value of separate substances. It is doubtful whether this last requisite will ever be attained, owing to the complexity of the problem, and the shifting nature of the data.

The nutritive value of any substance is necessarily dependent on the relation of that substance to the organism: but that relation cannot be constant, because the organism itself is frequently changing. Moreover, a substance which under ordinary circumstances will be very nutritious, suddenly fails to nourish, because some other substance is present, or some other substance is absent. Whenever the animal is a various feeder, variety in food becomes indispensable. Majendie found that rabbits could not subsist longer than a fortnight if fed on a single article of their ordinary food, such as carrots, or cabbages, or barley; and Ernest Burdach made the following experiment: Taking three rabbits not quite full-grown, but all three from the same litter, and as nearly alike as possible in size, strength, colour, form, and sex; to the one he gave nothing but water and potatoes, which were furnished in abundance; 'it ate seven ounces on the first day, six on the second, and gradually less and less; its weight, which on the seventh day was 161 *gros*, was reduced by the thirteenth day to 93 *gros*, when it died completely exhausted. The second was fed in the same way with barley; it ate 20 *gros* the first day, 14 the third, and so on less and less; in the fourth week it expired. The third rabbit was fed on alternate days with potatoes and barley, and its weight increased till the nineteenth day; and as its weight then remained stationary, in the third week both potatoes and barley were given together, upon which the weight continued to increase, and the animal retained its original vivacity."

VI. THE QUANTITY OF FOOD.—It has long been a question what quantity of Food is requisite for the proper sustainment and repair of the organism. Like most other questions of the kind, it can be answered only in a rough manner, precision being impossible. The differences of individual organisms, and the different conditions of these organisms, must always interfere with any attempt at accurate estimates. The same man must necessarily require more food when in activity than when in repose; in cold climates more than in hot climates; and although we may strike an average which shall be accurate enough as a matter of figures, of what use can an average be in Physiology? *The man to be fed is not an average.* A hundred men will consume an amount of food which may be accurately divided into a hundred parts; but these figures give us no real clue to the quantity needed by each individual; and rations founded on such estimates must necessarily be imperfect, one man receiving more, another less, than is required.

Individual experience can only be valid for the individual. Valentin, from experiments on himself, found that his daily consumption was rather more than six pounds of solid and liquid food; but Cornaro for fifty-eight years took no more than 12 ounces of solid food, and 14 ounces of light wine. Here are two individual experiences widely discrepant. It is clear to the physiologist that the very small amount of solid food taken by Cornaro was partly compensated by the nutritive value of the wine; and partly by the fact that his moderate activity caused a less demand than is usual among men; but even when due allowance is made for such elements, we are brought no nearer to a correct estimate, because we have not yet determined, and perhaps never shall determine, the nutritive value of the different articles of food; so that those elaborate arrays of *weights*, which many chemists and physiologists are fond of producing as evidence, are vitiated by the initial fallacy of supposing that vital phenomena can be satisfactorily reducible to arithmetical calculation.

We are tempted to pause here for a moment to notice one of the most singular of these misleading applications of arithmetic to Life. Both phrenologists and their antagonists constantly invoke the weight of the brains of different men and animals, as if they believed in an exact correspondence between so many ounces of nervous matter, and so much cerebral activity. Nevertheless they, at the same time, maintain that size is *not* the measure of power, unless "all other things are equal." Now the truth is that "all other things" never *are* equal, in two different brains.* Nervous tissue is not like so much salt or chalk, *defi-*

---

* See *Biographical Hist. of Philosophy*, 1857, p. 637.

nite in composition, presenting everywhere precisely the same quantities of water, phosphorus, sulphur, &c.; nor is it everywhere precisely similar in *development;* the proportions and directions of its fibres differing in different brains, and at different ages of the same brain. Yet it is on these two qualities, of *composition* and *development,* that the functions of the brain will greatly depend for their relative intensity; and these are not ascertainable by measurement, or by weight. To weigh the brains of two men, with a view of determining what the comparative intellectual power of the two men really was, is as chimerical as to weigh two men in the scales, with a view of ascertaining what amount of muscular energy, dexterity, and endurance each possesses. Indeed, the error never could have gained acceptance for a moment, if a true conception of biological philosophy had been prevalent, because such a conception would have repudiated the attempt to explain vital or psychological phenomena by the method effective only in Physics.

Quitting these estimates, and interrogating experience, we find the most singular and inexplicable differences in the quantities of food which individuals require, and in the quantities which they will consume if permitted.

As a general rule, more is eaten in cold climates than in hot climates; but it is by no means clear to me that the reason of this is the one advanced by Liebig when he says, "Our clothing is merely an equivalent for a certain amount of food; the more warmly we are clad, the less urgent becomes the appetite for food, because the loss of heat by cooling, and consequently the amount of heat to be supplied by food, is diminished." The relation between cold and food is more complex than that; and when Liebig refers to the gluttony of the Samoyedes, he overlooks the gluttony of the Hottentots, which is quite as remarkable. "If," he says, "we were to go naked like certain savage tribes, or if in hunting and fishing we were exposed to the same degrees of cold as the Samoyedes, we should be able with ease to consume half of a calf, and perhaps a dozen of tallow candles into the bargain, daily, as warmly-clad travellers have related with astonishment of these people. We should then also be able to take the same quantity of brandy or train-oil without bad effects, because the carbon and hydrogen of these substances would only suffice to keep up the equilibrium between the external temperature and that of our bodies."

This sounds very plausible as long as we confine our attention to Samoyedes, but it is rendered questionable by the statement, recorded by Barrow in his *Travels in Southern Africa*, that the Hottentots are the greatest gluttons on the face of the earth. Ten Hottentots ate a middling-sized ox in three days; and three Bosjesmans had a sheep given them about five in the evening, which was entirely consumed before noon of the following day. "They continued to eat all night, without sleep and without intermission, till they finished the whole animal. After this their lank bellies were distended to such a degree that they looked less like human beings than before."

The inhabitants of the Alpine regions of Lapland and of Norway are not remarkable for their voracity, nor are the Icelanders: a sufficient proof that mere temperature is not the sole cause of *excessive* eating, since such excess is sometimes observable in hot climates, and not always observable in cold climates.

Although Liebig's statement cannot be accepted, being indeed only one of the conclusions deduced from his theory of respiratory food, there is ample evidence to show that, without referring excessive gluttony to cold, we are justified in referring an increase of appetite to cold; and the increase is perfectly intelligible: more exercise must be taken in cold weather to develop the necessary amount of animal heat, more tissue must be wasted, and consequently more supply is needed for repair. "He who is well fed," says Sir John Ross, "resists cold better than the man who is stinted; while starvation from cold follows but too soon a starvation in food." The same writer thinks, that not only should voyagers to the polar regions take more food than usual, but "it would be very desirable indeed if the men could acquire the taste for Greenland food, since all experience has shown that the large use of oil and fat meats is the true secret of life in these countries, and that the natives cannot subsist without it, becoming diseased, and dying, with a more meagre diet."

The accounts which travellers give of the quantity of food which can be consumed are extraordinary. Sir John Ross estimates that an Esquimaux will eat perhaps twenty pounds of flesh and oil daily. Compare this with Valentin's six pounds, or with Cornaro's twelve ounces of solids and fourteen ounces of wine!

Captain Parry tried, as a matter of curiosity, how much an Esquimaux lad, who was scarcely full grown, would consume if left to himself. The following articles were weighed before being given. He was twenty hours getting through them, and certainly did not consider the quantity extraordinary:

	Lb.	Oz.
Sea-horse flesh hard frozen, .	4	4
„ „ boiled, . .	4	4
Bread and bread-dust, .	1	12
	10	4

To this must be added one and a quarter pint of rich gravy-soup, three wine-glasses of raw spirits, one tumbler of strong grog, and one gallon one pint of water.

Captain Cochrane, in his *Journey through Russia and Siberian Tartary,* relates that the Admiral Saritcheff was informed that one of the Yakuti ate in four-and-twenty hours the hind-quarter of a large ox, twenty pounds of fat, and a proportionate quantity of melted butter for his drink. To test the truth of this statement, the admiral gave him a thick porridge of rice boiled down with three pounds of butter, weighing together twenty-eight pounds; and although the glutton had already breakfasted, he sat down to it with great eagerness, and consumed the whole without stirring from the spot. Captain Cochrane also states that he has seen three Yakutis devour a reindeer at a meal; and a calf weighing about two hundred pounds is not too much for a meal of five of these gluttons.*

These facts are curious, but of course they throw no light on the question, how much food an individual requires to keep himself alive and active. Nor, indeed, has any method yet been devised which could elucidate that point. We can never feel confident that the quantity taken is not somewhat more, or somewhat less, than would really be advantageous. If a man is active on six pounds daily, he might be perhaps stronger on six and a half; and if six and a half should prove the precise amount which kept his weight unaltered, it would only do so under precisely similar conditions; and we know that on different days he will waste different quantities of tissue.

Some caterpillars daily eat double their weight in food; a cow eats 46 lb. daily; and a mouse eats eight times as much, in proportion to its own weight, as is eaten by a man. But when such facts are cited, we must bear in mind the enormous differences in the nature of the foods thus weighed, their relative amounts of water and indigestible material. The same caution is requisite in speaking of man's diet. It has been variously computed. Sanctorius estimated it at 8 lb., Rye at 5 lb. and 7 lb., Horne at 4 lb. 3 oz., and Valentin in his own person at 6 lb.

---

* Pereira: *On Diet*, pp. 16, 17.

Such estimates were too contradictory to afford any clue. The chemists bethought them of securing the requisite precision by taking the amount of carbonic acid expelled during the twenty-four hours as the standard of the amount of carbon necessary, and the amount of urea expelled in the same period, as the standard of nitrogen necessary. Tables were then drawn up setting forth the separate items of food requisite to supply this waste. But, apart from the profound distrust with which such chemical reasonings should be regarded, there is this separate source of distrust, that each man necessarily wastes different quantities of tissue under different conditions: if, therefore, our analysis of food correctly represented the amounts of carbon and nitrogen assimilated (which it does not), we should still have to construct a special table for each individual at each season of the year, and under varying conditions.

The question is really one of importance, when we have to apportion the rations of paupers, prisoners, soldiers, and sailors. Here we are forced to strike an average, although we know that on any average one man will necessarily have more, and another less, than is absolutely requisite; but the impossibility of arranging matters otherwise, unless food be so abundant that it may be left to the discretion of each to eat whatever amount he pleases, forces the adoption of some standard which experience rectifies on the whole. Dr Pereira has furnished several dietaries adopted for masses of men, and from these the following is taken.

The scale of diet in the Royal Navy is thus given in the Regulations:—

"There shall be allowed to every person the following quantities of provisions:—

Bread,	1 lb.
Beer,	1 gallon.
Cocoa,	1 oz.
Sugar,	1½ oz.
Fresh meat,	1 lb.
Vegetables,	½ lb.
Tea,	¼ oz.

"When fresh meat and vegetables are not issued, there shall be allowed in lieu thereof—

Salt beef, ¾ lb.	alternately	Salt pork, ¾ lb.
Flour, ¾ lb.		Pease, ½ pint.

"And weekly, whether fresh or salt meat be issued—

Oatmeal,	½ pint.
Vinegar,	½ pint."

The daily allowance to the common soldier in Great Britain is 1 lb. of bread and ¾ lb. of meat, making together 196 oz. of solid food weekly; for this he pays a fixed sum, namely, 6d. daily, whatever may be the market price. He furnishes himself with other provisions.

As to the quantity each man should eat when unrestricted, it is to be determined by himself alone. We are notoriously apt to eat too much, and consequently waste much food, even when we do not injure ourselves. Our sensations are the surest guides, yet they do not always tell us with sufficient distinctness when we have had enough: one thing is very clear, that to force the appetite—to continue eating after the stomach has once suggested "enough"—is sure to be injurious. Hospitable hosts, no less than anxious parents, should refrain from pressing food on a reluctant appetite; it is not kindness, although kindly meant.

In closing here our survey, we must confess that it has exhibited few reliable scientific data. Indeed, to some readers it may have seemed that our efforts have been mainly revolutionary, shaking foundations which promised security, and disturbing the equanimity of scientific speculation. It is a fact that Physiology is at present in too incomplete a condition to answer the chief questions raised respecting Food; and this fact it was desirable to bring into the clear light of evidence; for on all accounts it is infinitely better that we should understand our ignorance, than that we should continue believing in hypotheses which enlighten none of the obscurities gathering round the question. It is in vain that we impatiently turn our eyes away: the darkness never disappears merely because we cease to look at it.

CHAPTER III.

DIGESTION AND INDIGESTION.

Happiness and misery connected with the Digestion—In what is science distinguished from ordinary knowledge?—The mechanical and chemical processes of Digestion: the former only exists in the simplest animals—What takes place in the mouth—Chemical action of the saliva—Its mechanical action—"Bolting" the food—Mechanical action of the stomach—The gastric juice: its composition and origin—Pepsin and acid—Action of gastric juice on various substances—It arrests putrefaction—Lesson against over-eating—Influence of fats in Digestion—Influence of saliva in increasing the gastric juice—Quantity of gastric juice—Chymification equivalent to boiling—Why the gastric juice does not act upon the stomach itself—Intestinal Digestion—The bile: quantity and composition of; how affected by fats—What is meant by biliousness?—The part played by bile—It neutralises the action of gastric juice, and assists in the absorption of fat—The pancreatic juice acts on all substances—The intestinal juice completes Digestion—Nature of chyle—The various Causes of Indigestion.

"LET good Digestion wait on appetite." Much of our happiness depends on health, and health cannot continue without digestion. Riches, and honours, and the applause of crowds, are but poor compensations for the loss of that perpetual spring of pleasure which arises from the harmonious activity of all the functions; and the most prosperous of men must envy children and animals their prosperity of digestion. The misery of mankind, springing from many causes, is intensified by Indigestion, which lessens the fortitude to endure calamities, and increases the tendency to indulge in painful forebodings. Sorrow, come whence it may, is more lightly borne, and is briefer in its visitations, when the health is vigorous; and it cannot be vigorous without good Digestion. To those whose troubled secretions predispose them to gloom and fretfulness, small evils become magnified into calamities, and evils anticipated have the force of realities.

The marvels of Nature are an inexhaustible source of reverence to the reverential mind, and among these not the least marvellous is the admirable complexity of the apparatus on which Digestion depends. No sooner do we become acquainted with this complexity than our surprise is, not that many of us, imprudent and reckless as we are, should suffer from derangements of that function; but that any of us should continue to perform it with success seven days together. The more we contemplate this mechanism, the more wondrous it appears. The more we study the process, the more intricate its problems seem. Formerly the process was thought to be very simple; increase of knowledge brought increase of doubt; and now, whoever looks closely into the results of modern investigation will be surprised to find how little the enormous labours in all directions have added to our positive knowledge. The conquests of science have consisted in explaining certain details of the process, and in analysing the whole function of Digestion into separate acts.

And here we may pause to consider in what *scientific explanation* peculiarly consists, namely, in the discovery of those intermediate facts, not obvious, which link together the obvious phenomena. Thus it is matter of vulgar observation that cattle eat grass, and that this grass returns, in the shape of manure, to effect the growth of fresh crops. The farmer, in knowing this, may be said to know as much as the philosopher ; but the farmer only knows the facts which stand out, visible to all—the obvious arcs in the great orbit of life ; whereas the philosopher, not content with these, seeks to detect the *hidden* facts—the intermediate links of cause and effect, which constitute the chain. In proportion to his success in discovering these intermediate links, and their order of succession, he approaches a *scientific explanation* of the *vulgar observation*. This scientific explanation has two great advantages : it ministers to the intellectual craving for knowledge, a craving which gives man his eminence ; and it ministers to his practical aims, increasing his power over Nature by enabling him to foresee and to modify phenomena, and to adapt them to his uses. If he learns that silica* is an essential constituent of wheat, and that silica must be in the ground before it can enter the wheat, he learns two "intermediate facts" not at all obvious, which enable him to adapt circumstances to his needs: if the earth be deficient in silica, he will add a fresh supply.

---

* *Silica* is the chief ingredient of flint, sandstone, and many earthy minerals.

It is the same with Digestion. Every one knows that Food of various kinds is taken into the mouth, chewed, and swallowed; that it remains some time in the stomach, where it undergoes certain changes; that it then passes into the intestines, and separates into two portions, one of which nourishes the body, while the other is ejected as waste. These are the vulgar facts. But we cannot rest content with these. The intellect craves knowledge more precise, and practical wants require that the "intermediate facts" should be discovered.

THE FUNCTION OF DIGESTION.—The purpose of Digestion is to make Blood. To effect this there are many processes which, although closely connected with Digestion, are not really implied in it: such are Chewing the food, and Absorption. There are many animals which do not chew; there are many which have no absorbent vessels; yet all these digest. The function of Digestion is the function of the *alimentary canal*, which canal, or tube, extends from the mouth downwards through the whole trunk. Digestion prepares the Food for its entrance into the Blood. In those animals which chew their food we perceive that chewing aids in this preparation of the Food, and we shall therefore include it in our analysis of the process of Digestion, although, strictly speaking, it is not more digestive than cooking, or carving.

In Digestion two kinds of processes concur: the *mechanical*, such as trituration and maceration (or rubbing, and reducing to a pulp); and the *chemi-*

cal decompositions and transformations. My own investigations have led to the curious fact that in the simpler animals, only mechanical processes are in operation: it is not till we examine animals of a more complex organisation that we meet with chemical processes in Digestion; these being at first very feeble, and increasing in importance as the organisms become more complex.* In man, the chemical processes predominate. We shall touch on both as they present themselves.

I. WHAT TAKES PLACE IN THE MOUTH.—Before Food can enter the Blood it must be rendered *soluble*. It is very solid, for the most part, when taken into the mouth, and must be torn and ground by the teeth, the result of which is that the Food is mashed and moistened till it can be swallowed with ease. We first cook our beef; we then cut it into small pieces; and finally we tear and mash it with our teeth, in which act the saliva aids. These are mechanical preparations.

But now the question arises, whether the action of the Saliva is purely mechanical, aiding only in the maceration of the Food, or partly mechanical, and partly chemical, *i.e.* aiding also in its transformation? This has been hotly debated, and has given occasion to an immense amount of laborious investigation. I will answer it at once, as I conceive the most trustworthy researches warrant, and afterwards examine briefly some of the arguments and experiments which have been brought forward.

---

* As far as relates to the Sea-Anemones, these investigations are recorded in *Sea-side Studies*, p. 207-217.

The action of the Saliva is mainly *mechanical.* It is also *chemical,* but this in so slight a degree that it may be dispensed with altogether if the food be otherwise prepared, and introduced directly into the stomach. Its chemical action is limited to the amylaceous (starchy) substances, changing a small portion of them into dextrine and sugar.

C. H. Schultz, whose experiments at one time created a great sensation,[*] assigned the whole process of Digestion to the action of the Saliva. He denied the existence of any such fluid as the gastric juice; declaring that what was supposed to be gastric juice was only saliva and the remains of food. It is unnecessary nowadays to do more than mention this opinion, which is refuted by abundant experiments, all proving that Saliva exerts no chemical action whatever on any except starchy substances.

Claude Bernard, on the other hand—one of the greatest experimental physiologists of our day—maintains that the ancients were correct in assigning only *mechanical* influence to the saliva in facilitating mastication, taste, and swallowing; the *chemical* influence being completely insignificant, if not altogether absent.[†] In his ingenious and interesting exposition of the phenomena which take place in the mouth, he maintains that the three separate liquids which issue from the three separate glands—the *parotid*, in the cheek, the *sublingual*, under the tongue, and the *submaxillary*, in the lower jaw (see p. 230, Fig. 7, *p*, *q*, and *g*)—have distinct offices :—

---

[*] SCHULTZ: *De Alimentorum Concoctione,* p. 34. The arguments are reproduced in his curious work, *Die Verjüngung des Menschlichen Lebens,* p. 77.

[†] BERNARD: *Leçons de Physiol. Expérimentale,* ii. 168.

1. The office of the parotid secretion is to assist chewing.

2. The office of the sublingual secretion is to assist swallowing.

3. The office of the submaxillary secretion is to assist taste.

A mixture of these three secretions with that from the mucous membrane of the mouth, constitutes Saliva; and it is this Saliva which alone exerts the slight chemical action that ever does take place ; or, in other words, it is only the mucous secretion which can act chemically on starchy substances, the other three secretions separately, or mingled, being altogether without such chemical action. It is on this that Bernard founds his assertion that the Saliva has no part to play in the chemical processes of Digestion—an error in logic, I conceive ; but let us see the results of experiment.

The first person who distinctly detected the chemical action of Saliva was Leuchs.* He found that in a few hours it converted a mass of cooked starch into sugar. This was subsequently confirmed by Schwann, in a celebrated paper,† wherein he argued that inasmuch as the gastric juice was incompetent to effect any change whatever in starch, the sugar found in the stomach of animals who have had none given in their food, necessarily came from the action of the Saliva on starch.

---

* BURDACH : *Traité de Physiologie*, ix. 268.

† MÜLLER'S *Archiv*, 1836, quoted by BURDACH.

It should be remarked that these observations have not the cogency they were generally supposed to have, for nothing is better established than that many animal substances have the power of transforming starch into sugar; indeed, the *alterability* of cooked starch is so great, that almost any influence suffices to change it into *dextrine* (a substance having the same elementary composition, but different *properties*), and the dextrine into *glycose* (starch-sugar).

The researches of Mialhe* gave a new aspect to the question. He established the important fact that starch paste was changed into sugar *in less than a minute* in the mouth; as the sweet taste sufficiently indicated. It is this rapidity of action which distinguishes the Saliva from other organic substances (except the pancreatic juice), and which therefore confirms the position of Leuchs and Schwann. The same result is obtained out of the organism. Mix a little starch paste with Saliva, and it will very rapidly be converted into sugar. Mialhe conceived the idea that this action was due to a particular organic agent, similar to that known as the *diastase* of vegetables—an idea too plausible not to find ready acceptance; it is, however, now given up by the best authorities, and Bernard has shown that fibrine and gluten, in a state of decomposition, but *before* putrefaction has set in, acquire this very property of converting starch into sugar—whereupon he concludes that what is called vegetable diastase is really nothing but the spontaneous decomposition of the gluten.*

---

* MIALHE: *Mémoire sur la Digestion et l'Assimilation des Substances amylacées et sucrées*, 1846.

That a chemical action takes place in the mouth, during the ordinary process of eating, is very easily shown. Let the reader take a piece of ordinary bread, and he will perceive that when it is first introduced into the mouth no trace of sweetness is discernible for a few seconds; but as it rolls over the tongue, and the saliva mixes with it, a gradually increasing sweetness is discernible; in from 30 to 45 seconds a decided transformation into sugar has taken place. Moreover, he will find that those portions of the bread which have got fixed between his teeth, and have thus been retained longer under the influence of the Saliva, are noticeably sweeter than bread is wont to be.

The fact that Saliva does convert starchy substances into sugar is thus placed beyond dispute. The researches of Donders, Jacubowitch, Bidder, and Schmidt,† moreover, show that if the Saliva be *prevented* from entering the stomach, and starch paste be introduced into the stomach through an opening, no sugar is formed. Their conclusion is this: Although many organic substances convert starch into sugar, none do so with the rapidity of the saliva and the pancreatic juice.

---

* BERNARD: *Leçons*, ii. 161.

† DONDERS: *Physiologie*, i. 192. BIDDER and SCHMIDT: *Die Verdauungssäfte und der Stoffwechsel*, p. 15, *seq*. This latter work is the most elaborate ever published on the digestive fluids, and will be largely laid under contribution in our pages.

Starch itself is not assimilable. It must be changed into dextrine and sugar before it can pass through animal membranes, and enter the Blood. Now, inasmuch as the starchy substances form an important class of our aliments (see FOOD AND DRINK), the chemical intervention of Saliva in the digestive process is apparent.

We have thus arrived at a very different conclusion from that of Schultz, who attributes to the Saliva the whole of the chemical transformations in Digestion; and from that of Bernard, who denies that any chemical transformations are effected by it. The truth is, that *no* albuminous substance is transformed by the Saliva, and that *all* starchy substances are attacked by it.

Nevertheless we must not exaggerate this influence. It is only a small portion of the starch which is normally acted on by the saliva, either in the mouth or in the stomach; the greater portion is transformed in the intestines by the action of the pancreatic juice and intestinal juice. In consequence of this, the action of the salivary glands may altogether be dispensed with. When they are cut out, or when their secretion is prevented from flowing into the mouth, the animal seems to suffer no material inconvenience.* And Bidder and Schmidt have made the curious discovery that in young animals, during the period of suckling, the salivary glands have no secretion whatever.†

Having vindicated the *chemical* character of this secretion, we must now remark that its *mechanical* character is the most important. The amount of *transformation* is insignificant; but the amount of *maceration* which the food receives in the mouth, by the combined action of the teeth and saliva, is very important, as a preliminary towards the indispensable *solution*, which all food must undergo.

---

* BUDGE: *Memoranda der speciellen Physiologie*, p. 83.

† BIDDER and SCHMIDT, p. 22.

"It is much easier to supply the place of chewing by mechanical preparation in animal food than in vegetable. We can grind things small, but we cannot make artificial saliva. Kitchen science may prepare the former for the stomach, but not the latter. Indeed, the cooking of starchy foods for weak stomachs often makes them more indigestible, by reducing them from a solid to a fluid form, and thus enabling them to be swallowed without that manipulation in the mouth which elicits a flow of its secretions. The best form in which starch can be taken is one in which the envelope of the granule has been ruptured by heat, and the whole mass made light so as to be capable, *by* mastication, of being broken up and fermented equally by fluids, but not capable of skipping over the salivary admixture, and being swallowed *without* mastication. Such are good stale bread, mealy, well-boiled potatoes, thoroughly cooked cabbages: such are *not* new rolls, waxy potatoes, &c., which mastication merely makes into small solid tough lumps; or gruel and sago, and other slops, which annoy the dyspeptic, though so often forced upon him by his female friends." *

---

* CHAMBERS'S *Digestion and its Derangements,* p. 331.

There are animals that *bolt* their food. There are human beings who imitate these animals; but it is to be hoped that they have potent stomachs, since they thus throw upon that organ an amount of extra labour, which would better have been executed by the mouth. The food must be rubbed and macerated *somewhere.* If the mouth does not begin this process, the stomach must do it all.

II. STOMACHAL DIGESTION.—The food reaches the stomach in a state approaching that of pulp, and with a slight chemical change in its starchy elements. Formerly it was thought that the stomach did all. Digestion was thought to be the function of the stomach, and of it alone. But recent researches have profoundly modified this conception; they have circumscribed the action of the stomach to that of a still further preparatory process, the completion of which must be sought in the intestines.

Our knowledge of the stomachal process has been rendered more accurate and extensive, owing to a fortunate accident which befell the Canadian known all over Europe as Dr Beaumont's patient. This man, Alexis St Martin by name, had a large hole in his stomach caused by the discharge of a gun. He recovered perfect health, the wound healed, but the opening remained, and this opening was used by Dr Beaumont for observation and experiment. He has recorded the results in a well-known work.* Since then, another case has fallen into scientific hands. Dr Schroeder and Dr Grünewaldt, at different periods, experimented on an Esthonian woman, with what results we shall hereafter learn.* And quite recently Alexis St Martin has again become the subject of experiment in the hands of Dr Francis Smith of Philadelphia.† The value of these cases cannot be over-estimated; and the results have been compared with observations on animals, since Blondlot conceived the happy idea of establishing an artificial fistula in dogs, through which gastric juice might be obtained when wanted, and substances introduced into the stomach, or removed from it directly.

---

* BEAUMONT: *Experiments and Observations on the Gastric Juice*; 1838.

1. *The mechanical action of the stomach.*—If we examine the stomach of a fasting animal we find its lining walls are pale and flabby, lying close together, and only separated from each other by a layer of mucus and saliva.

It was this appearance which suggested the idea that the sensation of HUNGER was due to the rubbing of the coats of the stomach; an idea which we saw in the first chapter to be altogether erroneous.

The arrival of the food changes this condition. The contractions of the *œsophagus* or gullet (see p. 230, Fig. 7, *œ*), force the food into the stomach (E), which distends its walls. This distension goes on increasing and increasing as fast as fresh supplies arrive. Gradually the distension becomes more and more difficult, and this creates a sense of fulness, which warns us to desist from eating. The blood-vessels of the stomach have now become swollen; the secretions are pouring in actively; the whole condition of the stomach is changed, and its muscular activity is roused.

---

* SCHROEDER: *Succi gastrici Humani vis digestiva,* 1853. GRUNEWALDT: *Disq. de succo gastrico Humano ope fistulæ stomachalis indagatæ,* 1853. I know these works only at second hand.

† His memoir, originally published in the *Philadelphia Medical Examiner,* has been translated in BROWN SÉQUARD'S *Journal de la Physiologie,* i. 144.

This muscular activity is important, being to stomachal digestion what the action of the teeth is in the preparation which takes place in the mouth. The lining of the stomach is a mucous membrane—a continuation, indeed, of that which lines the mouth, but differing from it in the peculiar glands which characterise some regions of it. Under this mucous membrane lies a muscular coat; this muscular coat gives, by the alternate contraction and relaxation of its fibres, a *rotatory* movement to the food. According to Dr Beaumont's graphic expression, the food is *churned* in the stomach, carried round and round from right to left around the large end, and from left to right along the large bend, returning along the small bend.* In this churning movement the food is not only well "ground," but is more and more mixed with the gastric juice. The process may be compared to the rolling about of the food in the mouth by the tongue.

Blondlot maintains that this trituration and maceration constitute the whole of stomachal digestion: the food is rendered liquid without being *chemically altered.* I have shown this to be the case in the digestive process of the *Actiniæ;* but in all the higher animals there is ample evidence of a positive chemical change taking place. And this we shall now consider.

---

* This rotation has been denied by BETZ, who says the disposition of muscular fibres in the stomach is against it.—CANSTATT, *Jahresbericht*, 1853, p. 147. But it seems difficult to admit that Beaumont could have been mistaken when he says he *saw* the food thus rotate.

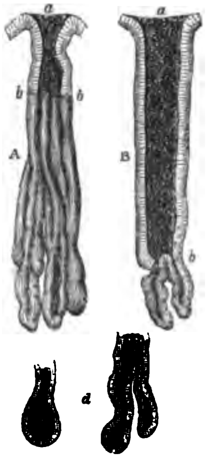
2. *The chemical action of the stomach.*—Before Réaumur instituted his experiments, it was held that the stomach merely ground the food to a pulp, by the action of *trituration* or rubbing. He tested this opinion in a very ingenious way, considering that the backward state of chemistry could furnish him with none of the means now at our disposal. He filled hollow silver balls with meat, and perforating them in several places, caused a dog to swallow them. After they had remained some time in the stomach, he withdrew them by means of a thread. If, he argued, the digestive process were merely mechanical, the meat, protected by the silver covering, would not be altered; if, however, the process were chemical, the solvent fluid would have penetrated through the holes and acted on the meat; and so indeed it proved—the meat was entirely *chymified;* that is, made into a pulp.

After this the gastric juice itself was obtained, and Réaumur and Spallanzani commenced that long series of experiments on *artificial* digestion which is not yet nearly concluded.

The stomach is lined by a mucous membrane. If we examine this membrane under the microscope, we find it crowded with minute finger-shaped tubes.

These are the *gastric follicles*, or secreting glands, from which the gastric juice is poured into the cavity of the stomach, directly the membrane is irritated by the presence of food (or indeed of any other substance).

Fig. 4.



Vertical section of Gastric Follicles, or Glands, which secrete Gastric Juice. *From the stomach of a fasting dog, magnified.*

A, a follicle from the middle of the stomach; B, one from the end near the intestine; *a a*, the openings through which the juice pours into the stomach; *b b b* mark the points where the structure becomes glandular; *d*, small pyloric tubes lined with epithelium to their ends.—After Todd and Bowman.

Thirty-one pounds daily are said to be secreted from these glands in a healthy adult! Besides this gastric juice which flows on the entrance of food into the stomach, there is also present a secretion of mucus from the membrane, and the saliva, which is constantly being swallowed.

It would lead us too far, and into questions of too technical an interest, to examine the disputes which gather round the questions—What constitutes the gastric juice? and, What is its origin? For us it is enough to know that the gastric juice is an *acid* secretion, containing in all probability hydrochloric and lactic acids, containing also a peculiar organic substance named *pepsin.** The acids and the pepsin act chemically upon meats—that is, upon all albuminous and gelatinous substances—converting them into *peptones.* These peptones have all certain properties in common, no matter how various the substances from which they are derived ; and it is owing to these properties, not possessed by the substances from which they are formed, that they are capable of being *absorbed* and *assimilated.*

---

* The student may consult the great work of Bidder and Schmidt, *Die Verdauungssäfte;* Lehmann's and Mulder's Treatises on Organic Chemistry; Donders: *Physiologie;* Chambers: *Digestion and its*

Neither the acid, nor the pepsin, exerts more than a slight chemical influence on two great classes of Alimentary Principles, the Sugars and the Fats. Cane-sugar is changed into grape-sugar, owing, Dr Harley believes, to the presence of a free acid : and fatty acids are sometimes developed from the fats. We may therefore set aside, for the moment, all starch, sugars, fats, and oils, which form so large a proportion of our food, and fix our attention exclusively on the albuminous substances. It is these which mainly undergo *chemical* change in the stomach. And we shall find that even these are not greatly changed ; so that the popular idea of the stomach being the chief organ of Digestion will have to be considerably modified.

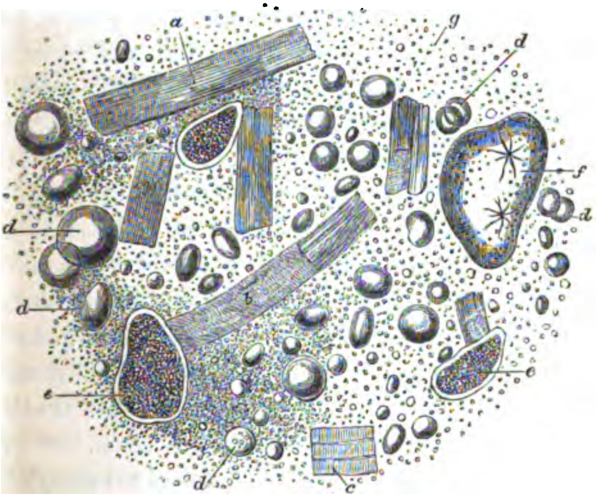
During the *churning* motion just described, the food is gradually mixed up with the fluids of the stomach. In consequence of this complete *saturation* it is reduced to a pulpy mass—the *Chyme;* and as it becomes more and more churned—more and more of semi-liquid

---

Derangements ; LONGET : *Physiologie ;* BLONDLOT'S *Traité de la Digestion,* and his Memoirs in BROWN SÉQUARD'S *Journal ;* and BERNARD'S *Leçons de Phys. Expérimentale,* ii.

consistence—it passes into the small intestines, there to undergo fresh changes. If we examine this pulpy mass of Chyme just as it quits the stomach, we shall find that, however altered to the eye, its alterations have been for the most part physical, and but slightly chemical. That is to say, the *flesh* has become macerated, the *muscle-fibres* have become dissociated and broken into fragments, and the *cellular tissue* is dissolved; but many of these *muscle-fibres* retain their primitive structure; the *fat* is liquefied, but not often otherwise altered—not even emulsified; and the *gelatine* has become liquid peptone.

Fig. 5.



CHYME TAKEN FROM THE PYLORIC END OF A DOG'S STOMACH. *a*, muscular fibre from which the transverse markings have disappeared; *b*, muscular fibre in which these markings are partly visible; *c*, in which they are perfect: *d*, globules of fat; *e* and *f*, starch grains; *g*, granules.—After Claude Bernard.

This is what the microscope reveals, and this is assuredly not a *digested* mass.

Blondlot, indeed, declares that the Chymification we have been describing is the sole digestive process ; and Chymification, according to him, is nothing but rendering the food *soluble*. Bernard takes a different view : Chymification he regards as only one part of the digestive process, and that exclusively confined to the albuminous and gelatinous substances, on which its action is precisely analogous to that of *boiling*.

If we attend closely to what goes on in the stomach, as reported by all the most trustworthy observers, we cannot, I think, come to any other conclusion than this : Chymification is mainly a physical process, but is also partly chemical ; it liquefies and minutely divides one portion of the food, and it chemically transforms the other.

Against the purely physical hypothesis there is one argument which seems to me overwhelming : Liquid albumen is very slightly absorbable, and not at all assimilable ; but when acted on in the stomach, it becomes readily absorbable and assimilable. It seems, moreover, certain, that albumen is absorbed in small quantities from the stomach. The fact that the greater part of this substance passes undigested into the intestine, is no argument against the action of the stomach. The mouth begins, but does not complete, the conversion of starchy substances ; the stomach begins, but does not complete, the conversion of albumen.

Albumen, on entering the stomach, is coagulated, and rendered insoluble in water. It must of course be again made soluble. This is effected by the gastric juice, which converts it into a *peptone*, not only soluble, but now incapable of coagulation.

If by heat you coagulate the albumen of the white of egg, and then dissolve it again, you can once more coagulate it by the application of heat. The change has been simply physical: the albumen was liquid, you made it solid; you can make it liquid again, and solid again; as you can turn water into ice, and ice into water. But you cannot do this with the peptone-albumen. *That* has undergone a chemical change, and has become a new body: it is soluble in water now, or in diluted alcohol (not in concentrated alcohol); and is incapable of coagulation, and of forming insoluble compounds *with salts*.

The gastric juice which does this, does so in virtue of its acid and its pepsin, acting together. Separately, these agents have no such power. In artificial digestion, if the acid be neutralised by an alkali—say the carbonate of soda—the action of the gastric juice is immediately suspended, to be resumed immediately on the addition of acid. Bidder and Schmidt have shown that the digestive power of the gastric juice is, up to a certain limit, in direct ratio to its amount of acid. Nevertheless it is not the acid alone which is operative. This is easily proved. Submit the gastric juice to a temperature of 212° Fahr., and it loses all its digestive power whatever; not because the acid has been destroyed, but because the pepsin has been destroyed. Pepsin is an organic substance which acts as a *ferment;* now, Chemistry teaches us that at 212° such organic substances coagulate, and when coagulated they cease to act as ferments.

Freezing the gastric juice has no such influence. Bidder and Schmidt found that when frozen it dissolved albumen as well as before—which, as they remark, must be a great comfort to the admirer of ices.

3. *Gastric Juice.*—Gastric juice is distinguished from among all other animal fluids by its *stability:* it resists decomposition longer than any other animal fluid. Nor does it lose its digestive virtue even when mouldy. One peculiarity must be insisted on, namely, its power of *arresting the putrefaction* of other organic substances. It is to this that we owe the impunity with which we eat "high" game and putrefying meat. Another curious fact is, that although the addition of organic acids increases the digestive power of this fluid, there is a limit at which this increase ceases, and beyond it, excess of acid suspends the whole digestive power. The power is also arrested by the presence of too large a quantity of organic substances, or peptones.

In this latter fact there is a lesson against over-eating. Digestion is impeded by too large a meal, first, in the arrest of the action of the gastric juice; and next, in overloading the stomach with more than it can properly move. There is another fact mentioned by Lehmann,[*] which is fertile in suggestion, namely, that the addition of water, so far from weakening, *increases* the power of the gastric juice. In this fact we see the impropriety of that advice so often given by writers on Dietetics, to abstain from drinking at dinner, "because," as they assert, "liquid dilutes and weakens the gastric juice." Plausible as this assertion is, we see that it is directly contrary to fact. Our best guide in such cases is instinct; our sensations tell us when to drink, and when to leave off drinking; and we may rest assured that until Science can reconcile its dicta with the dicta of instinct, Science has still something to seek. As Goethe says in *Hermann und Dorothea:*

---

[*] LEHMANN: *Physiol. Chemie,* ii. 47.

> "Ich tadle nicht gern was immer dem Menschen
> Für unschädliche Triebe die gute Mutter Natur gab.
> Denn was Verstand und Vernunft nicht immer vermögen, vermag oft
> Solch ein glücklicher Hang, der unwiderstehlich uns leitet." *

Curiously enough, the digestion of albuminous substances is aided by the presence of *fat.* Whether this influence is simply one of contact (*catalysis,* as the chemists say), or whether the fat itself undergoes some slight chemical change, is undetermined; and at any rate, the quantity changed must be so small as to escape our estimate. The fact having been ascertained, we are led to speculate on its application. May not the custom of our eating bacon or ham with chicken, and not with ducks and geese, have something to do with it?

---

* Of which the reader must accept the following prose version:—"I do not readily blame any harmless impulse which the kind Mother, Nature, has given to man. For often, the end which the understanding and reason are unable to attain, is arrived at by a happy instinct or inclination irresistibly leading us."

Saliva also lends its aid in the digestion of albuminous substances, although incapable of itself acting on them. Bidder and Schmidt, indeed, found that it hindered the digestion of coagulated albumen; but Donders found that cooked flesh was more rapidly digested when saliva was mingled with the gastric juice, than when the gastric juice alone was used; and even coagulated albumen was seen to be more rapidly digested when the gastric juice was made alkaline by the addition of saliva, in the case of the Esthonian peasant previously mentioned.*

The preceding paragraph suggests a serious doubt respecting the current theories on the chemistry of Digestion, and is apparently subversive of all those experiments which went to show that digestive power was intimately connected with the acid in the gastric juice. And I am the more disposed to call attention to this difficulty, because nothing is more certain than that a large quantity of alkaline saliva is normally present in the stomach, yet in spite thereof we know that digestion goes on; while it is equally certain that outside the organism, in artificial digestion, alkalies arrest the power of the gastric juice. Here are two contradictory facts: the alkaline saliva is found not to arrest Digestion in the stomach, and alkalies are found to arrest it out of the stomach. Can they be reconciled?

In a former chapter (see p. 112) attention was called to the complication of all vital problems by the necessary interference of the organism itself. We have an illustration in Digestion. *In the organism,* although the alkali of the saliva may neutralise a certain amount of the acid of the gastric juice, it stimulates the gastric follicles to an *increased secretion,* so that, over and above the quantity neutralised, there is a *larger* quantity of gastric juice produced for the purposes of Digestion; and thus *in* the organism, saliva has practically a contrary result from that which it produces *out* of the organism.

---

* DONDERS: *Physiologie*, i. 219.

Nor is this a mere hypothesis, without the warrant of analogy; for if carbonate of soda be administered with iron, the iron is more rapidly attacked by the gastric juice than when administered without the alkali; and this, says Bernard, is owing to the greater secretion of gastric juice which the carbonate of soda determines.

4. *Quantity of Gastric Juice.*—The quantity of gastric juice daily secreted has been very differently estimated, but the calculations of Bidder and Schmidt give the enormous amount of thirty-one pounds, or nearly one quarter of the whole weight of the body.* The quantity is of course variable. It depends on physical and mental conditions, and on the nature of the food eaten; some substances exciting a more active secretion than others. The *quality* is also variable. Grüne-

---

* At the Leeds Meeting of the British Association, Dr Harley stated that he had never found any animal secrete more than one-tenth of its own weight in the twenty-four hours.

waldt and Schröder found the gastric juice of the Esthonian peasant very different under different conditions. When Grünewaldt examined it, the woman was living well, at his expense, and in comfortable quarters; her gastric juice then contained 43 parts of solid matter in 100, and of these 43 no less than 36 were *pepsin.* When Schröder examined it, the woman had been living for some months on spare diet in her own hut, and her gastric juice then, instead of 43 parts in 100, contained only 5½ parts of solid; and instead of 36 parts of pepsin, only 3 appeared!

In presence of differences so great as these, we must hesitate before drawing general conclusions from particular cases. We may also understand from it how food may be digestible at one time, which at another is indigestible.

To close these details respecting gastric juice, it may be mentioned that calculations have been made respecting the quantity of albumen which definite amounts are capable of dissolving. Lehmann estimates that 100 grammes* of juice dissolve 5 grammes of coagulated albumen, on the average. Schmidt estimates it at no more than 2 or 3 grammes. But even if we accept the largest of these quantities, we are forced to admit that the whole amount of the gastric juice secreted during the day would not suffice to dissolve more than *half* the albumen necessary for nutrition—another proof that stomachal Digestion is only one part, and that not the chief part, of the digestive process.

---

* A gramme is somewhat more than 15 grains.

5. *Results of Inquiry.*—Let us now sum up in a few brief propositions the net results at which we have arrived respecting stomachal Digestion :—

1. In the stomach the food is churned, macerated, disintegrated, and much of it liquefied.

2. The Fats have been liberated from their cellular envelope, and have become Oils.

3. The Sugars have been little altered. The cane sugar has been changed into grape sugar, and probably a small proportion has been changed into lactic acid.

4. The Vegetable matters have been more or less divided and made pulpy, but not chemically altered, except some of their starch, which has been altered by the saliva.

5. The Albuminous matters have been macerated, and some portion transformed into peptones. The whole has become a pulp.

Bernard, as we before stated, compares the action of gastric juice on fatty, saccharine, and albuminous matters, to that of boiling water. Boiling liquefies fat by dissolving the cellular envelope; gastric juice does no more, but it does so at a considerably lower temperature. Starch and sugar are not changed by boiling, except that they take up a proportion of water, which makes them what the chemists call *hydrates;* this also, he maintains, is the sole change effected in them by the gastric juice. Boiling *dissolves* the gelatinous parts of the bones and skin, leaving the earthy parts, and the muscular fibre, simply dissociated, but not altered; so does the gastric juice. Finally, Bernard calls attention to the fact that the *intestinal* fluid digests meat which has been boiled, but not raw meat—which shows that boiling may *replace* the action of gastric juice.

Were then the ancients correct in the supposition that stomachal Digestion is merely a process of cooking? To a great extent, yes. The process is in many respects analogous. It differs, however, in one important point, namely, the chemical transformation of the food into *peptones*.

6. *Action of Gastric Juice on the Stomach.*—Before quitting this subject we must glance at another ancient opinion, not yet universally discarded. When one of the Alchemists announced that he had discovered an *universal solvent*, Kunckell the chemist, with quiet wit, asked, "In what kind of vessel do you keep it?" The difficulty which Kunckell felt in conceiving a vessel that could resist the action of an *universal* solvent, was felt by the physiologists, who proclaimed that gastric juice was an universal solvent of animal substances which nevertheless did *not* dissolve the stomach. What protected the stomach? The mysterious entity, named "Vital Principle," which was the *deus ex machinâ*, and universal refuge in cases of difficulty, seemed to them the sole possible cause of this immunity enjoyed by the stomach. In proof, they appealed to the fact that in the dead body the stomach is sometimes seen to be attacked by the gastric juice. Not content with attributing this immunity of the stomach to the Vital Principle, which "suspended the action of chemical laws," they urged this fact of immunity in the

living stomach as a proof of the existence of that very Vital Principle which effected it. Logicians call this arguing in a circle; but let us see if we cannot explain the facts without having recourse to any Vital Principle suspending and superseding chemical laws.*

In the first place, as a matter of fact, the Vital Principle, granting its existence, has *no* power whatever of arresting the action of gastric juice. The proof is easy. Place a little juice under the skin of a *living* animal, and you will find in a short time that the cellular tissue is dissolved, and the parts in contact with the fluid chymified. Bernard kept the hinder extremities of a living frog in contact with gastric juice, and found they were digested by it, in spite of that Vital Principle which, according to its advocates, can suspend all chemical laws.

We thus see that it is not the mere fact of the animal being alive which prevents the gastric juice from acting on its stomach, Life having no mysterious power of "suspending" such actions; but that, whether dead or alive, the gastric juice will dissolve animal tissues. Why then is the stomach protected during life? For precisely the same reason that many poisons which rapidly destroy life when entering the blood, as in wounds, are perfectly harmless when entering the stomach. The savage slays his game with poisoned arrows, and eats the poisoned flesh with impunity.

---

* In our final chapter, on LIFE AND DEATH, we shall examine this ancient hypothesis of a Vital Principle, and expound the modern substitute for it.

This is an interesting, but by no means mysterious, fact. The poison is harmless in the stomach because it cannot be absorbed from the stomach; and unless it be absorbed, and enter the blood, it cannot operate as a poison. There is no question of a Vital Principle here; it is simply a question of Absorption. Let but the epithelial lining * of the stomach be destroyed in any spot, and the poison will there enter in spite of a Vital Principle. The case is analogous with the gastric juice; the pepsin cannot be absorbed, because the epithelial lining of the stomach opposes that absorption. This epithelium, however, is very rapidly and easily destroyed; and in the dead stomach the *destruction*, not being compensated by as rapid a *renewal*, leaves the tissue unprotected against the action of the gastric juice.†

Life therefore endows the stomach with immunity, not because Life suspends the ordinary actions of matter, but because it here *renews* the protecting epithelium as fast as it is destroyed; and thus the phenomenon is explained by clear physiological principles, without invoking the interference of a Vital Principle, the very essence of which is inconceivable, and its modes of action wholly supposititious.

---

* The *epithelium*, or delicate layer of cells which lines the wall of the stomach, is another form of that layer of cells called *epidermis*, or scarf skin (see fig. 2, p. 35) which is the outermost and protecting membrane of the body.

† BERNARD: *Leçons*, ii. 406, *seq*. BASSLINGER gives a very absurd explanation—namely, that nervous influence produces a state of tension in the molecules of the tissue which prevents their being acted on.—MEISSNER, *Bericht über Physiol.*, 1857, p. 201. Since this was in type, I have learned that Dr Harley has shown by experiment that the layer of mucus covering the walls of the stomach is the chief protector against the absorption of gastric juice. This does not disturb the argument in the text.

III. INTESTINAL DIGESTION.—The food has become *Chyme*; we have now to trace its transformation into *Chyle*. It has been, so to speak, cooked; we have now to see it digested.

The pulpy mass of Chyme enters from the stomach into the intestinal canal, and there undergoes its final changes during its passage through an organ the enormous size of which (*thirty feet*, and upwards, in length) is alone sufficient to indicate its importance; and this indication is further borne out by the fact that large and important glands, such as the Liver and the Pancreas, pour their secretion into it. This thirty-feet canal is by anatomists distinguished as if it were two organs, though in reality it is one continuous tube. They speak of it as the Small Intestines—comprising the *Duodenum*, the *Jejunum*, and the *Ileum*; and the Large Intestines—comprising the *Cæcum*, the *Colon*, and the *Rectum* (see p. 230, fig. 7).

Here it is that *Vegetables* are made to yield whatever digestible material they contain; *fats* are reduced to an emulsion which permits their being absorbed; *starch* is completely converted into *glycose* (starch-sugar); and the undigested *flesh* is converted into peptones.

In the Stomach the *mechanical* processes are predominant; in the Intestine, these are subordinate to the *chemical* processes due to the influence of three fluids, the Bile, the Pancreatic juice, and the Intestinal juice. These we must now separately examine.

1. *The Bile.*—Into the first of the Small Intestines (*Duodenum*) the Liver pours its secretion of Bile at a slight distance from the stomach (see p. 230, fig. 7, B′). No less than three or four pounds of this fluid are daily poured in. The quantity is of course variable, like that of every other secretion, being dependent on individual peculiarity, and on the nature of the food eaten. Fats, and fatty foods, *diminish* the quantity in a notable degree, according to Bidder and Schmidt, who found that cats fed solely on fat secreted no more than 0.327 of a gramme per hour, whereas their secretion on ordinary diet was 0.807 grm.

Although far from denying the conclusion of these able experimenters, I cannot help pointing out a source of fallacy in this experiment, which seems to have escaped criticism. We know that the quantity of Bile secreted is always larger on a full diet than on a spare diet, larger on flesh-diet than on mixed diet; when therefore cats were fed on fat alone, they were fed on a diet which was equivalent to almost none at all, since all animals starve on fat alone. That this objection is valid may be seen in the fact that the quantity secreted by these fat-fed cats is actually the quantity which is secreted by the fasting animal. Had Bidder and Schmidt fed the cats on pure albumen, I believe they would very soon have found the secretion of Bile was no greater than that from fat diet; and for the same reason: pure albumen soon becomes equivalent to almost no food at all. The only positive conclusion I can draw from their experiment is one sufficiently curious—namely, that in spite of Bile being the most hydrocarbonaceous substance in the body, and of fat being a hydrocarbonaceous food, there seems *no* definite relation between the formation of the one and the presence of the other.

There is an important practical question directly connected with this influence of fat on the secretion of Bile. Fats are said to make people bilious. On the other hand, some medical men advise bilious people to eat bacon for breakfast. Now we must distinctly understand that even some medical men talk of "biliousness," when in truth the evil is simply Indigestion; and as fat, if decomposed in the stomach, produces Indigestion, fat may in *this* sense be said to make people bilious. There is, however, a frequent "attack of biliousness" from which some unhappy people suffer, and this arises from a deficiency in the secretion of Bile. The sluggish Liver leaves its work undone, like the over-fed servants of a great house. The best of all stimulants is continued exercise in the open air. But if it could be proved that fat has a tendency to diminish the secretion of Bile, we should therein see the perfect explanation of the common belief that "fats make people bilious."

In the preceding section it was noted that water increased, instead of diminishing, the digestive power of the gastric juice. Its value in digestion is further shown in the fact that it increases the quantity of Bile —not only its amount of liquid, but also its amount of solid matter. A dog, after eating 185 grammes of beef without water, secreted 2.283 grm. of Bile, containing 0.135 solid matter. He was subsequently allowed to take the same weight of water—185 grm.—without beef, and he secreted 5.165 grm. of Bile, containing 0.143 solid matter. On another occasion, after only 25 grm. of beef and 158 grm. of water, he secreted 4.030 of Bile. Very surprising facts, which show the immense importance of water in nutrition.

Flesh increases the secretion considerably beyond that produced by vegetable diet.

2. *The part played by the Bile.*—It would be easy to fill a small volume with the controversies which have long been raging respecting the part played by this secretion in the animal economy; but the reader will not expect anything of the kind here, and may be content with two facts which, if not absolutely beyond dispute, are at any rate fixed on so broad an inductive basis as to command confidence: These are, First, that the Bile is formed *in* the Liver, and *by* the Liver; it does not pre-exist in the blood, although of course its elements are there: Secondly, that it is at once a *secretion* and an *excretion:* the Liver separates from the blood substances which it *forms anew* into a fluid, this fluid taking its part in the digestive process, afterwards to be *reabsorbed* into the blood; and the Liver also separates from the blood, or forms anew, sub-

stances which, though constituting not more than one-eighth of the whole Bile (probably much less), are ejected from the organism as injurious.*

In the stomach *Bile at once arrests Digestion.* If from any cause it has risen into the stomach, as unhappily it sometimes does, we are seized with nausea and vomiting. The evidence for this is so plain that we cannot wonder if many writers have founded on it their disbelief in the digestive influence of the Bile, which they regard as a pure excretion. They overlook one important consideration, however, and forget that, when in the stomach, Bile is out of its right place. The intestine is its right place, and *there* its influence is digestive, although it there also counteracts the action of the gastric juice.

This leads us to a very curious example of the complication and seeming contradiction of organic processes. Bile undoubtedly arrests the influence of the Gastric Juice, inside as well as outside the body. Place a piece of meat in a glass vessel with some gastric juice; the meat will soon manifest a commencement of digestion, the fibres will be dissociated, and the cellular tissue dissolved. If now a little bile be added, the digestive process is suddenly arrested: the gastric juice preserves its acidity, but loses its digestive influence. If, in a second vessel, meat be left in contact with gastric juice only, we find at the end of a few hours that a complete chymification has been effected; whereas the meat to which bile was added remains unaltered, and will continue so for a very long period.

---

* Comp. BIDDER and SCHMIDT, p. 217.

What is the nature of this action? Bernard maintains that it is nothing else than this: the ferment *pepsin*, which is operative in gastric juice, is *precipitated*, as the chemists say (separated and thrown down), and the albuminous substances are once more rendered *insoluble*,* and cease to undergo chemical change.

The reader will remember that the albuminous substances were rendered *soluble* in the stomach by the gastric juice. On reaching the intestine, and there coming in contact with the Bile, they are once more rendered *insoluble:* a yellow precipitate is formed, which sticks to the coats of the intestine; and thus, while the starchy, fatty, sugary substances continue their progress unchanged, the albuminous substances are arrested, and all that was so laboriously achieved in the stomach seems undone again. If there were nothing to counteract this influence of the bile, the digestion of meat would be impossible; fortunately the delay is temporary, and by the action of the pancreatic and intestinal juices, the albumen is once more rendered soluble.

We may now perhaps explain some points which have seemed very puzzling and contradictory. Experiments have been made which prove that when the gall duct, through which the bile pours into the intestine, is opened, and the bile by removal is prevented from entering the intestine, the animal loses weight.

---

* BERNARD: *Leçons de Physiol. Expérimentale,* ii.

and dies in a fortnight or three weeks. This result is intelligible when we reflect that normally the bile is mostly *reabsorbed*, only one-eighth of it passing away as an excretion; so that the loss of all the bile must be an immense drain on the system.

But other experiments seemed flatly to contradict this result. The bile was removed, yet the animal lived on, apparently uninjured. Blondlot had a pointer who lived for five years with an artificial opening, or "fistula" in its gall duct; she annually suckled her pups, and went out shooting with her master, as if nothing were the matter. On the strength of this it has been argued that bile is an excretion, and totally useless in Digestion.

The contradiction has been explained thus: when the bile is removed, a much larger quantity of food becomes necessary, indeed as much again must be eaten, to preserve the standard weight. The extra supply of food compensates for the drain occasioned by the loss of the bile.

But we have still to prove that the bile has a digestive action. And that *some* material difference is produced by the bile is clear from the highly fetid odour of the excreta, and the unusual development of gas, when the bile is prevented from pouring into the intestine. Flatulent dyspepsia is the result of a languid secretion from the liver.

We have already seen that the action of bile arrests the digestion of albuminous substances; on sugar and starch its action is inappreciable; on fat its action is peculiar. There is no dispute that the bile greatly assists in the absorption of fat; but *how* it does so is disputed.[*] Wistinghausen's experiments show that whereas oil can only be made to penetrate an animal membrane when submitted to a high pressure, it penetrates without any pressure at all, if the membrane be moistened with bile. Now all absorption in the organism takes place through membranes, and not through orifices: the absorbent vessels have no open mouths to suck up fluids; the fluids pass into them through their delicate walls.[†]

3. *The part played by the Pancreatic Juice.*—Under its form of "sweetbread" every one is familiar with the Pancreas. It lies along the under-side of the stomach, and pours its secretion into the intestine through a duct (sometimes two ducts) which enters a little way below the bile-duct (see p. 230, Fig. 7, W).

If Bernard's views of its function are correct, it is the most important organ in the whole course of the alimentary canal, for it possesses the property of forming *fats* into an emulsion, of transforming *starch* into sugar, and of acting on *albuminous substances*—both those precipitated by the bile, and those also which the gastric juice has not yet dissolved. But, he says, it possesses this power over albuminous substances only *after* they have been acted on by the bile. If we take food directly from the stomach, and submit it to the action of the Pancreatic Juice, *no* such effect ensues; whereas if the food taken from the stomach be first mixed with a little bile, and then submitted to the Pancreatic Juice, the digestive effect is obtained. Thus the successive actions of Gastric Juice—Bile—and Pancreatic Juice, form a necessary series.*

---

[*] Mr Marcet has recently advanced cogent reasons for supposing that the bile assists in the absorption of the fatty acids, and that these are produced in the stomach from the fats of the food.—*Medical Times*, 28th Aug. 1858. I know only the analysis of this paper given by BROWN SÉQUARD: *Journal de la Phys.*, i. 806.

[†] See the explanation of *Endosmosis* in the chapter on FOOD AND DRINK, p. 106.

There has been great dispute respecting the functions thus attributed to the Pancreas, and although Bernard replies to his antagonists with great skill and ingenuity, the researches of Colin and Bérard† place beyond a doubt that fat is absorbed even when the Pancreas has been extirpated; and, moreover, the fact of animals digesting when their bile is removed, proves that the Pancreatic Juice *does* act on the food even without the previous action of the bile. We must, therefore, somewhat qualify Bernard's statement, and say that the action of the Pancreatic Juice alone is much less energetic than when following that of the bile; and that although the Pancreas may greatly assist the absorption of fat, it is not indispensable to it.

The united influence of the bile and Pancreatic Juice transforms such portions of the starch as have escaped the action of the saliva into *dextrine* and *glycose* (see p. 197), into which the starch must pass before it can be absorbed; it renders the fats easy of absorption; and it dissolves albuminous substances.

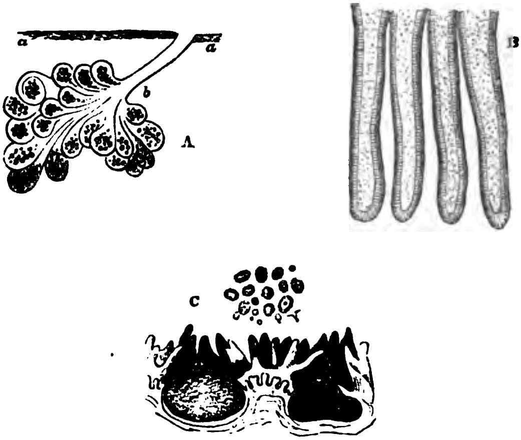
---

* BERNARD: *Leçons*, ii. 441.

† See MEISSNER: *Bericht*, 1857, p. 205.

4. *The Intestinal Juice.*—Although the processes just named are capable of digesting all alimentary substances, the process of Digestion is not really completed by them, but is continued and finished during the passage of the food along the whole length of the intestine, by means of the Intestinal Juice, a fluid secreted from the glands which are imbedded in the lining coat of the intestine. The above figure represents three of these glands, named after Brunner, Lieberkühn, and Peyer, their discoverers.

Fig. 6.



GLANDS IN THE INTESTINE WHICH SECRETE THE INTESTINAL JUICE.

A. *Brunner's gland—a a* the mucous membrane of the intestine; *b* the grape-like gland, each lobe of which pours its secretion through a common duct.

B. *Lieberkühn's glands*, being finger-like depressions of the mucous membrane, very like the glands in the stomach.

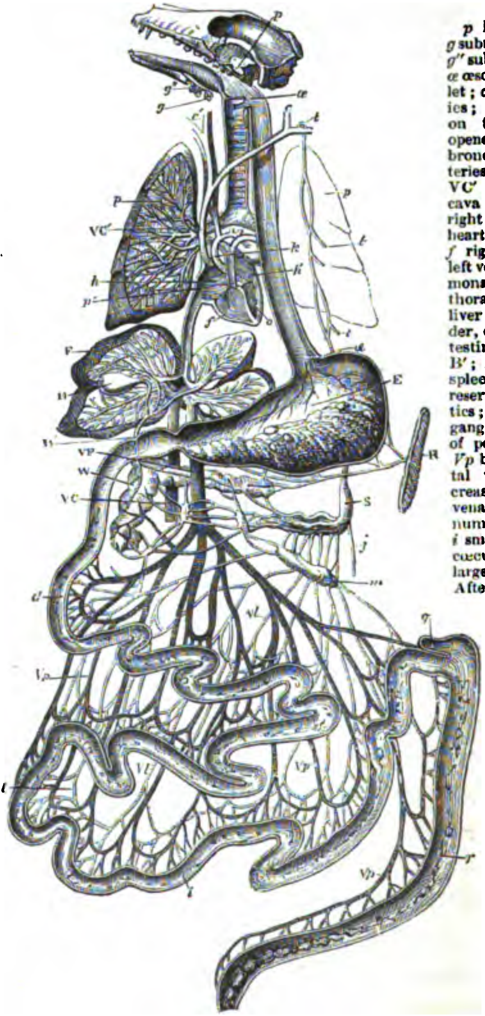
C. *Peyer's glands*—the one to the right is empty, its contents having been discharged: these are figured above. Peyer's glands project on the surface of the intestine, instead of being depressions from it.

The action of this Intestinal Juice has been demonstrated by Bidder and Schmidt in the following ingenious experiment. A coil of a dog's intestine was drawn through an opening, and a thread tied round it, so as to cut off the influx of the gastric fluids; into this intestine, small muslin bags were introduced, containing coagulated albumen and meat; the coil was then replaced. After periods of from six to fourteen hours, the dogs were killed. The bags were found in different regions of the intestine, some having scarcely moved, some having been carried down as far as the *cæcum* (Fig. 7, *q*, p. 230); but in every case the albumen and meat were more or less changed, and had lost weight, some having lost 18, and others 85 per cent of their solids.

In the Sea-Hare (*Aplysia*) I have more than once traced this action of the intestine. The food quits the stomach in a very imperfect state of digestion, and may be traced gradually along the whole length of the intestine, digestion not being finally completed till the *colon* is reached. Indeed, *a priori*, one might have drawn such a conclusion. The function of Digestion is performed by the whole alimentary canal, and if particular parts of this canal have a special office, in virtue of some special development, the whole organ is necessary for the whole result.

IV. THE NATURE OF THE CHYLE.—We have thus completed our history of the digestive process. The food has become Chyme, the Chyme has become Chyle, and the Chyle will be absorbed, and become Blood.

Fig. 7.



p Parotid gland; *g* submaxillary gland; *g''* sublingual gland; *œ* œsophagus or gullet; *cc* carotid arteries; *pp* lungs, that on the left being opened to show the bronchial tubes, arteries, and veins; VC' superior vena cava; *k* aorta; *h* right auricle of the heart; *h'* left auricle; *f* right ventricle; *o* left ventricle; *p'* pulmonary artery; *tt* thoracic duct; F liver; B gall bladder, entering the intestine by the duct B'; E stomach; R spleen; S Pecquet's reservoir; *j* lymphatics; *m* mesenteric ganglia; VP trunk of portal vein; *Vp Vp* branches of portal vein; W pancreas; VC inferior vena cava; *d* duodenum; *Vl* lacteals; *i* small intestine; *q* cæcum; *r* colon, or large intestine. — After Bernard.

THE DIGESTIVE TRACT.

The preceding diagram, copied from Bernard, may assist the amateur in following the course of the digestive tract.

In the mouth the food is acted on by the saliva issuing from the *parotid* (*p*), the *submaxillary* (*g*), and the *sublingual* glands (*g′*). It passes along the *œsophagus* (*œ*), and reaches the *stomach* (E), whence it passes into the *duodenum* (*d*), and the whole length of the *small intestine* (*i*), to the *cæcum* (*q*), and *colon* (*r*).

During its course, such portions of the food as have become soluble are absorbed—1, by the *Portal Veins* (*Vp*), which carry them to the *Liver* (F), and, 2, by the *lacteals* (*Vl*), minute absorbent vessels which carry them to the *reservoir of Pecquet* (S), from whence they are transmitted by the *thoracic duct* (*tt*) to the *subclavian vein*, where they join the blood. The other parts of this diagram will be explained hereafter. Meanwhile, if the reader considers for a moment the immense preponderance of the absorbent portal veins over the absorbent lacteals, he will see that the popular notion of the Chyle, which is found in the lacteals, being the quintessence of the food, is altogether erroneous. If we call Chyle that which is found in the intestine before absorption, we may be justified in the assertion that the object of digestion is to make Chyle. But if we suppose that the fluid circulating in the chyliferous vessels (lacteals) is all the Chyle, we shall greatly err. *This* Chyle is really little more than ordinary lymph and fat. The chief part of the digested food, chief in quality as in quantity, *never* enters the lacteal vessels, but is carried by the portal veins to the liver. Comparative anatomy, teaching as it does that birds have *no* lacteals, that reptiles have none, and that fishes have none, plainly teaches that the function of these vessels must be special, and Claude Bernard maintains that it is limited to the absorption of fat.

V. Causes of Indigestion. — In unfolding the various stages of the digestive process, we have at the same time unfolded several of the causes which may disturb that process, and afflict human beings with a slight or terrible attack of Indigestion.

It is certain that if the food be not well masticated and saturated with saliva, we must have the powerful gastric juice of a dog, or a lion, to compensate this deficiency; otherwise a larger proportion of the unchanged food will be transmitted to the intestines than they can well manage, or will lie like a load oppressing the stomach. The starch will descend in lumps, and although much of it will be dissolved by intestinal digestion, some will pass away undigested.

If the secretion of Gastric Juice be languid, or if that fluid be not sufficiently acid, chymification will be laborious and painful. If the bile rise in the stomach, digestion will cease; if the secretion of bile be too scanty, the food will lie like a burden, and produce diarrhœa or sickness; and so on to the end of the chapter. Let there be only a little less acid, or a little more alkali, each of which depends on complex conditions, and Digestion, which to the young and healthy is as easy as it is delightful, becomes the source of misery.

Ill-selected food is one source of these evils; but it has been touched on in a previous chapter, and need not detain us now. Want of fresh air and exercise is another source. The action of the liver is particularly affected by exercise; and all who suffer from biliousness should pay their fees to the livery stable and waterman, horse-exercise and rowing being incomparably the best of prescriptions. A walking excursion, especially in mountain districts, and with resolute avoidance of walking too much, will be of great service to the dyspeptic. It is important to bear in mind, moreover, that although sedentary habits are very injurious to the Digestion, they are less so than bad ventilation; those who sit long, and sit in bad air, are sure to suffer. We shall touch on this point in the chapter on RESPIRATION AND SUFFOCATION.

The influence of the Nervous System is perhaps even more prominently manifested than that of any other cause of Indigestion. It is comparatively rare to meet with Indigestion among artisans, in spite of their ill-cooked food, their exposure to all weathers, and other hardships; and it is as rare to meet with good Digestion among the artisans of the brain, no matter how careful they may be in food and general habits. Protracted thought, concentrated effort in the directions of Philosophy, Science, or Art, almost always exact a terrible price. Nerve-tissue is inordinately expensive. But it is worthy of remark, that mere intellectual activity, when unaccompanied by agitating emotion, never seems to affect the Digestion, unless the effort be of an unusual intensity. Our passions are destroying flames. Anger, Ambition, Envy, Despair, Sorrow, and even sudden Joy, immediately disturb the digestion. A letter bringing bad news, the sight of anything which painfully affects us, a burst of temper, or an anxious care, will sometimes render the strongest of us incapable of digesting a meal. If the food be swallowed, it will not be digested, or digested only at a vast expense. And herein may be learned a lesson against a very common mistake committed by very sensible people. When a friend is overwhelmed with grief, we try to force him gently to take the food he obstinately refuses. "Do try and eat a mouthful; it is necessary for your strength; you will fall ill." Perhaps our entreaties succeed; he takes a little food "as a support." Error! the food will weaken, not strengthen him. In such cases Instinct may safely be relied on. When a man is hungry, he will eat. When he will not eat, he should be left in peace until hunger prompts. If in compliance with the entreaties of friends he takes a meal, it will do him harm rather than good. There are, indeed, people who think that to eat in times of sorrow, is a proof of want of feeling, and that their appetite is a sign of disrespect; as if appetite were subject to the will. Such people must be reasoned with, and told that it is as foolish to refuse food when the appetite demands it, as to eat it when the system rebels against it.

There is another direction in which the Nervous System influences Digestion, although it can only be briefly alluded to here. When we come to treat of nervous phenomena, we shall more particularly examine the nature of Reflex Action; at present it is enough to say, that certain parts of the organic mechanism are so intimately allied in action, that they are said to *sympathise* with each other. All parts of the alimentary canal sympathise. Whenever the saliva is profusely secreted, the gastric juice "sympathises," and is also secreted; and any irritation of the mucous membrane of the stomach increases the flow of saliva. This is a fact to be borne in mind, the more so as few persons seem thoroughly aware of it, although it serves as a simple indication of an irritated state of the stomach, which they might well note. In my own person I have frequent experience of it; and the presence of an unusual flow of saliva is always a warning to me that the mucous membrane of my stomach is affected. Dr Gairdner mentions the remarkable case of a man who secreted from six to eight ounces of saliva during a meal of broth which was injected into his stomach; and the reverse has been observed—an excitation of the nerves of taste producing a flow of gastric juice and bile.*

The deduction from these facts is simple and important. All who are troubled with a deficiency of gastric juice should be careful to let their food be as full of *flavour* as possible. Tasteless food, by leaving the nerves of taste comparatively quiet, leaves the secretion of gastric juice proportionately feeble. Food which has a relish can be more easily digested. Every one knows that we can eat a variety of dishes with less labour in digestion, than a smaller quantity of one kind of food, simply because the variety of "relish" makes the digestive process more active.

---

* GAIRDNER: *Edin. Med. and Surg. Journal,* xvi. 355; BROWN SÉQUARD: "Lectures" *in Lancet,* Nov. 1858, p. 467.

It is, I conceive, from the same law of sympathetic action that smoking, after a meal, assists digestion. There has been much discussion respecting the injuriousness of smoking, ever since Tobacco was first discovered; but as Physiology was—and still is in most circles—little understood, a very considerable amount of nonsense has been, and continues to be, uttered on this question. It is a positive fact that the gastric secretion can at any time be produced by simply stimulating the salivary glands with tobacco; and, as before stated, whatever stimulates the secretion of saliva promotes that of the gastric juice. Smoking does this. A cigar *after* dinner is therefore to that extent beneficial. Not so *before* a meal.

But the action of tobacco is not confined to this—it has other influences, some beneficial, some injurious; the amount of injury depends on the nature of the organism; and therein each person must judge for himself. There is only one caution which it is right to place before the reader. When tobacco is said to be not injurious, but beneficial, it must always be understood to mean tobacco in small quantities. Excess in tobacco is very injurious; so also is excess in alcohol;

so also would be excess in mutton-chops. All excess is dangerous. All stimulants should be used sparingly. Yet the man who never thinks of exceeding his half a pint of wine, or pint of beer, daily, makes no scruple of smoking a dozen cigars. From my own experience, rendered vigilant as I am by a delicate digestion, and an easily disturbed organism, I can conscientiously say that two cigars daily, always taken after, and never before, the chief meals, have proved themselves to be decidedly beneficial in many directions; but I should no more think of increasing that quantity, than of increasing my daily quantity of coffee or beer. Other organisms could of course endure greater quantities. Each must determine the proper limit for himself, and having determined it, *abide* by it.

Among the many slight causes of impaired digestion is to be reckoned the very general disregard to eating between meals. The powerful digestion of a growing boy makes light of all such irregularities; but to see adults, and often those by no means in robust health, eating muffins, buttered toast, or bread-and-butter, a couple of hours after a heavy dinner, is a distressing spectacle to the physiologist. It takes *at least four hours to digest a dinner;* during that period the stomach should be allowed repose. A little tea, or any other liquid, is beneficial rather than otherwise; but solid food is a mere encumbrance: there is no gastric juice ready to digest it; and if any reader, having at all a delicate digestion, will attend to his sensations after eating muffin, or toast, at tea, unless his dinner has had time to digest, he will need no sentences of explanation to convince him of the serious error prevalent in English families of making tea a light meal, quickly succeeding a substantial dinner.

Regularity in the hours of eating is far from necessary; but regularity of *intervals* is of primary importance. It matters little at what hour you lunch or dine, provided you allow the proper intervals to elapse between breakfast and luncheon, and between luncheon and dinner. What are those intervals? This is a question each must settle for himself. Much depends on the amount eaten at each meal, much also on the rapidity with which each person digests. Less than four hours should never be allowed after a heavy meal of meat. Five hours is about the average for men in active work. But those who dine late—at six or seven—never need food again until breakfast next day, unless they have been at the theatre; or dancing; or exerting themselves in Parliament; in which cases a light supper is requisite.

We have thus traced Food through its metamorphoses into Chyle. As Chyle, it is absorbed by the lacteals and veins to join the Blood. It is this complex fluid which we have now to examine.

CHAPTER IV.

THE STRUCTURE AND USES OF OUR BLOOD.

Blood the river of life—Erroneous notions about medicines "purifying" the blood—How it circulates—Capillary vessels—The substances it carries: iron and soap—The blood-discs: their discovery by Malpighi and Leeuwenhoek—Hewson's researches—Size of the discs in various animals—The nucleus—The colourless corpuscles: analogy between them and the Amœbæ—What is an Amœba?—The parasite which lives in our blood—Development of blood-cells—Is the blood alive?—Coagulation and its cause—Chemical composition of the blood—Variations of, in different people—Differences between arterial and venous blood—The gases they contain—Cause of the change of colour—The marvel of the circulation—Quantity of our blood—Blood-letting—Transfusion: history of the attempts; when it may be successfully employed—Blood does *not* form the organs—Vivifying elements of blood: arterial nourishes, and venous excites the tissues—Oxygen as a condition of nutrition—How does the blood nourish?—Each organ determining the nutrition of the rest—Relation between circulation and assimilation.

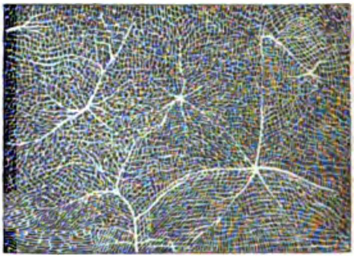
BLOOD is a mighty river of life. It is the mysterious centre of chemical and vital actions as wonderful as they are indispensable, soliciting our attention no less by the many problems offered to speculative ingenuity, than by the important practical conclusions to which our ideas respecting the Blood necessarily lead. By some the Blood is regarded as the source of all diseases, and to "purify the Blood" is the object of their treatment. Many quacks seize on this notion, and in sublime ignorance of the nature of the Blood they profess to purify, and of the means by which their drugs could possibly purify it, make fortunes out of the credulity of the public. I would warn my readers at the outset against this notion of "purifying" their Blood. Not that the Blood is always healthy and pure, free from hurtful substances, and rich in needful substances; but Nature herself takes this purification in hand, whenever it is possible—or, to speak less ambiguously, the organic processes upon which our existence depends for its continuance are themselves the means by which the Blood is kept at its proper standard, hurtful substances removed, and needful substances carried to it. The Blood is not like a river into which anything, and in any amount, may be introduced from without. It gets rid of, or destroys, all substances which do not form part and parcel of its own structure; or, failing in that, it ceases to act as living blood. And of those substances which form part and parcel of its structure, it will only take up, or retain, certain definite quantities—the surplus is rapidly got rid of. Let a man drink gallons of water, and his blood will not be more watery than before. Let him take quantities of salt, and his blood will not be more saline. In the incessant changes which take place within the circulating system, the Blood itself constantly tends towards uniformity of composition.

Nor is this all. On the supposition that we could "purify the blood" of a diseased patient, nothing would be gained by it, as respects his cure; for the malady under which he labours does not lie in the blood, but in the tissues; and when they are in a diseased condition, it is of no use to present them with purer blood; because, although they take from the blood all the materials for their nutrition, they can only take such as their condition allows. But we shall explain this more fully hereafter.

This river of Life is impetuously rushing through every part of the body, by means of an elaborate network of canals. In the course of the year, these canals carry not less than three thousand pounds' weight of *nutritive* material *to* the various tissues, and three thousand pounds' weight of *wasted* material *from* the tissues.

At every moment of our lives there is something like ten pounds of Blood rushing along in one uninterrupted throbbing stream, from the heart through the great arteries, which branch and branch like the boughs of a tree, the vessels becoming smaller and smaller as they subdivide, till they are invisible to the naked eye, and are then called *capillaries* (hair-like vessels), although they are no more to be compared in size with hairs, than hairs are to be compared with cables. These capillaries form a network much finer than the finest lace—so fine, indeed, that if we pierce the surface, at almost any part, with the point of a needle, we open one of these vessels and let out its contents. But as even this will not convey an idea of the fineness of the capillary network, we will give a figure of the vessels on the surface of a rabbit's liver, magnified eleven times.

Fig. 8.



CAPILLARY BLOOD-VESSELS ON THE LIVER.

This, which we have copied from Virchow, shows us how densely crowded the vessels are; they seem to form nearly the whole bulk of the liver.

Through the *walls* of these capillaries, the Blood suffers some of its nutritive material to ooze, receiving in exchange the worn-out material of the tissues. The Blood has now ceased to be arterial, and has become venous. The stream continues its course through the veins, which, commencing in the capillaries, reverse the arrangement we noticed in the arteries, and become less and less numerous, their twigs becoming branches, and their branches trunks, till they reach the heart.

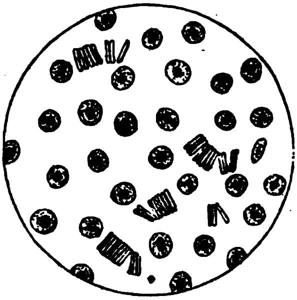
In this ceaselessly-circulating stream forty or more different substances are hurried along: it carries gases, it carries salts, it carries metals—nay, it carries what may be called soaps. The iron, which it washes onwards, can be separated; and Prof. Bérard used to exhibit a lump of it in his lecture-room—so that one ingenious Frenchman was led to suggest that coins should be struck from the metal extracted from the blood of great men! Lest this statement should mislead the reader, I will add that the quantity of iron in the blood is extremely small; but as the quantity of blood is large, and is perpetually being renewed, it affords the chemist the means of extracting a lump of iron from it.

Let us now examine the principal constituents of this organic structure—first its solid, and then its liquid parts.

1. *The Blood-discs.* — Although to the naked eye the blood appears a simple fluid, having a colour more or less scarlet, the microscope assures us that it is a fluid which carries certain solid bodies of definite shape and size—so definite, indeed, that a mere stain, no matter where, will, to the experienced eye, betray whether it be the blood of a mammal, a bird, a reptile, or a fish.

Prick your finger with a needle, place the drop on the glass-slide under your microscope, cover it with a thin glass, and look. You will be surprised, perhaps, to observe that the blood which had so deep a tint of scarlet in the mass, is of a pale reddish yellow, now that it is spread out on the slide; whereupon you conclude that the depth of tint arose from the dense aggregation of those reddish-yellow *discs,* which you observe scattered about, some of them sticking together, and presenting the appearance of piles of half-sovereigns.

Fig. 9.



Blood-Discs.

It is these "floating solids" of the blood upon which your attention must now be fixed. They are variously named *Blood-corpuscles, Blood-globules, Blood-cells,* and *Blood-discs.* It is a pity that one term is not finally adopted; and blood-discs seems on the whole the best, as being descriptive, without involving any hypothesis. Meanwhile, since physiologists use all these terms, the reader must be prepared to meet with all.

The first person who saw these blood-discs was undoubtedly Swammerdamm, in 1658; but as his observations were not published till a century afterwards, and as in Science priority can only rightfully be awarded to him who first publishes, the title of discoverer is given to Malpighi, who saw and described them in the blood of a hedgehog in 1661. He saw them, but did not understand them. They appeared to him to be only globules of fat.

The commencement of accurate knowledge dates from Leeuwenhoek, who, in 1673, detected them in human blood. "These particles," he says, "are so minute, that one hundred of them placed side by side would not equal the diameter of a common grain of sand; consequently, a grain of sand is above a million times the size of one such globule." *

We have now the exact measurement of these discs, which was not possible in his day. Extending his observations, Leeuwenhoek found that in birds and fishes, as well as in quadrupeds, the *colour* of the blood was due to these discs. He seems to have been puzzled by the fact, that in fishes the discs are not round, but oval; and he at first attributed this to the compression exercised by the vessels. It is instructive to hear him confess that he could not persuade himself "that the natural shape of the particles of blood in fishes was an oval; for inasmuch as a spherical seemed to me the more perfect form."[*] He was too good an observer, however, to permit such metaphysical conceptions long to mask the truth, and, accordingly, he described and figured the blood-discs in the fish as oval.[†]

---

* LEEUWENHOEK: *Select Works,* i. 89.

It is to Hewson that science is indebted for the most accurate and exhaustive investigation of the blood which has been made from 1770 down to our own time; and it has been even asserted by one whose word is an authority,[‡] that Hewson's works contain the germ of all the discoveries made in our own day.

There is something at once painful and instructive in the fact, that, after the publication of researches so precise and important as those of Leeuwenhoek and Hewson, the whole subject should have been suffered for many years to lapse into ignorant neglect; and instead of any progress being made, we find the most eminent physiologists at the beginning of the present century (Richerand and Majendie, for example) denying positively that the blood-discs existed, or that the microscope could tell us anything about them.* Nevertheless, there is not an amateur of the present day who is not familiar with them. Science has carefully registered the exact measurements and forms of these discs, in upwards of five hundred different species of animals.

---

[*] LEEUWENHOEK, *Select Works*, ii. 233.

[†] In the larva of the *Ephemeron* the blood-discs are as nearly as possible oat-shaped.

[‡] MILNE EDWARDS: *Leçons sur la Phys. et l'Anat. Comp.*, i. 44. The works of HEWSON have been edited, and in a very valuable manner, by Mr GULLIVER, for the "Sydenham Society."

Contempt of microscopic research seriously retarded the progress of Physiology; it has its parallel in a similar contempt inspired by the great Linnæus respecting the application of the microscope to Botany; and as the physiologists of this century have had to rediscover what was known to Leeuwenhoek and Hewson, so also have the botanists had to rediscover what was familiar to Malpighi.

2. *Influence of the Form of the Discs.*—There must assuredly be some relation between the *form and size* of these discs and their *function;* but what that relation is, no one has yet made out. In general, the larger discs are found in the less advanced organisms; that is to say, they are larger in the embryo than in the adult, larger in birds than in mammals, larger in reptiles and fishes than in birds. But they are largest of all in the Triton and Proteus, which being reptiles are exceptions to this rule. Nor can the rule be taken absolutely, even within those limits we have named, since although reptiles are less advanced in organisa-

---

* Milne Edwards notices a similar denial made by M. Giacomini at the Pisa Congress of scientific men in 1839—a denial which pretended to be based on original investigations.

tion than mammals, and have larger discs, it is not the least advanced among the mammals that have the largest discs;—for instance, the ruminants are less advanced than the quadrumana, yet among mammals the ruminants have the smallest discs; and in man they are as large as in rodents.*

3. *Structure of the Discs.*—The structure of these bodies is necessarily difficult of study. Leeuwenhoek, and others, observed that in the discs of the fish and reptile there is always a central spot, which appears dark, or clear, according as it is viewed by transmitted, or reflected, light. This appearance was interpreted as indicating a hole in the discs, which made them resemble quoits. But Hewson settled the point by proving the central spot to be a solid *nucleus*, which he saw escaping from its envelope, and floating free in the liquid—an observation subsequently confirmed.

It is worthy of remark that this nucleus is seen with difficulty when the blood is newly drawn from a vessel, although it speedily becomes distinct, especially if a little water be added. This has led Valentin, Wagner, Henle, Donders, and Moleschott to the conclusion that the nucleus is *not* present in living discs, but arises from *internal coagulation* on exposure of the discs to the air: a conclusion rejected by Mayer and Kölliker, the former averring that he has *seen* the nucleus while the blood-discs were still circulating in the capillaries of a young frog's foot. I have not been able to see this in the large discs of the Triton, and know not if Mayer's observation has been confirmed by any other microscopist.

---

* In man their diameter varies between $\frac{1}{4000}$ and $\frac{1}{2800}$ of an inch; and their average thickness is $\frac{1}{12\cdot400}$ of an inch. VIERORDT estimates that in about $\frac{1}{160}$ of a cubic inch there are as many as 5,055,000 of these discs.

The most perfect opportunity I ever enjoyed of seeing the nucleus, under the circumstances mentioned by Mayer, was with Professor von Siebold, when examining the gills of the embryo of *Salamandra atra.* This salamander brings forth its young alive; but it lives high up in the mountains, out of the reach of water, and its young would perish were they to come into the world like the young of other salamanders; namely, as tadpoles. Instead, therefore, of going through its metamorphoses in the water, the young *Salamandra atra* goes through them in its mother's body, and is born with perfect lungs, and without gills. Professor von Siebold removed two or three of these young ones before they had completed their metamorphoses, while therefore they were still in the tadpole state; and the elegant appearance under the microscope of their long delicate gills, like gelatinous fern-leaves, was exquisite. The excessive transparency of these gills rendered our observation of the circulation peculiarly favourable; but although I gave special attention to it, I could not detect the nucleus.

But there are other grounds on which I should be disposed to accept the fact of the nucleus being normally present, and not simply the result of coagulation: the chief of these is, that in the embryo of every mammal we discover nuclei in the discs, whereas in the adult animal no nuclei are discoverable, even after long exposure to the air; and the philosophic zoologist well knows in how many minute particulars the *embryonic* state of the higher animals represents the *permanent* state of the lower. In the discs of all adult mammalia the nucleus is absent; what has sometimes been mistaken for it is simply a central depression of the disc, which gives it the form of a bi-concave lens. Nevertheless, although the nucleus is absent in the adult, it is present in the embryo.

Robin says that *almost* all the discs have a nucleus when first the blood appears, but even then some are without it. "By the time the embryo has attained a length of thirty *millimètres*, at least one-half of the discs are without a nucleus, and the number goes on increasing, although even as late as the fourth month a few rare examples may be found." From that period, he says, they are no longer visible in the *human* subject;* but I have seen on two occasions what may have been the nucleus in a few discs of a newborn kitten, and *one* in a drop of blood of an adult mole.†

4. *The Colourless Corpuscles.*—There are other bodies in the blood beside these, and they are known

---

* C. ROBIN: *Sur quelques points de l'Anat. et de la Phys. des globules rouges*, in BROWN-SÉQUARD'S *Journal de la Physiologie*, i. 288.

† Mr WHARTON JONES, one of our best investigators, says that the blood of the elephant and the horse contains a few of these nucleated discs. NASSE has seen them in the blood of pregnant women, and Mr BUSK found one in that of a man. KÖLLIKER disputes the accuracy of these observations, and thinks that in each case the nucleus was pro-

as the *colourless corpuscles*, which consist of two, if not three, different kinds. The true colourless corpuscle (and it will be convenient to confine the term disc, or cell, to the *red* corpuscle) is much larger than the disc, and seems to be a round vesicle containing a number of excessively minute particles imbedded in a gelatinous substance. This corpuscle has the property of *spontaneous expansion and contraction*, which forcibly reminds the observer of the contractions and expansions of that singular microscopic animalcule, the *Amœba*, probably the very simplest of all organic beings.

Some of my readers are probably not acquainted with even the name, much less the nature, of this microscopic animal, which is nevertheless so interesting to naturalists that a goodly array of authorities may be cited when it is mentioned. A few words of explanation will suffice here.

The Amœba is a single cell: it has no "organs" whatever, but crawls along the surface by extemporising an arm or a leg out of its elastic body; this arm or leg is speedily drawn in again, and fresh arms are thrown out; thus, as you watch it, you perceive it assuming an endless succession of forms, justifying the name of Proteus originally bestowed on it.

So like the Amœba is the colourless blood-corpuscle,

---

duced by some alteration of the contents; and VIRCHOW makes it very probable how the optical illusion could be produced.—*Cellularpathologie*, 1858, pp. 10 and 126. At any rate, the presence of nucleated discs is the indication of physiological inferiority, and we may perhaps find them in certain cases of disease.

that many observers have not hesitated to adopt the opinion that these corpuscles are actually animalcules, and that our blood is a select vivarium; an opinion which is not very tenable, and is far from necessary for the purposes of explanation. We may admit, and the point is of philosophic interest, that the blood-corpuscles are *analogous* to the Amœbæ, without admitting them to be parasites.

Considering the wondrous uniformity in the organic creation, considering how Life seems everywhere to manifest itself under forms which through endless varieties preserve an uniformity not less marvellous—so few and simple seem to be the laws of organic combination — there is nothing at all improbable in the idea that as the Amœba is a starting-point of the animal series, an analogous form may also be developed in the blood. In many of the lower animals the blood abounds in these Amœba-like cells.* Moreover, the very substance of the fresh-water Polype *sometimes* breaks up into several distinct masses, which can in no respect be distinguished from Amœbæ.†

Although it is wrong to consider these Amœba-like corpuscles to be parasites, I may mention, in passing, that the blood *has* its peculiar parasite, and a very singular animal it is. Bilharz describes it, under the name of *Distoma hæmatobium*, as a double animal, the sexes being perfectly separate, the male lodging the female in a sort of tube extending along its stomach. With two heads and two tails, it seems only to have one body. This specimen of "two single parasites rolled into one," is only found in the blood of man, and is as limited in its choice of a home as the parasite which finds a home in the brain of the sheep (producing there the "staggers," well known to farmers), or the parasite which gives pigs the "measles." *

---

* They have been seen in mollusca, crustacea, and insects. I have seen them in the beautiful transparent *Corethra* larva.

† Sometimes, but often not; so that the phenomenon probably depends on the state of the animal. ECKER describes a "contractile substance" in the Hydra, which he likens to the Amœba, but his figures do not at all resemble the contractile masses which I saw, and which indeed were so like Amœbæ as to make me believe at first that the Polype had swallowed them.

The colourless corpuscles are not numerous in healthy human blood, and play but a secondary part, unless we assume, with many physiologists, that they are the early stage of the red-discs. Professor Draper speaks unhesitatingly to this effect. He says there are three periods in the history of our blood-cells. Those of the first period originate simultaneously with, or even previously to, the heart—these are the embryonal cells which are colourless and nucleated. These becoming liquid internally, are developed into the cells of the second period, which are red, nucleated, and oval, like the normal cells of reptiles. The cells of the third period replace these, "the transition being clearly connected with the production of lymph and chyle corpuscles." This change takes place at the close of the second month of fœtal life; and from henceforwards no change is observable; the cells continue to be red, bi-concave, non-nucleated, and circular. "The cell of the first period is therefore spherical, white, and nucleated; that of the second, red, disc-shaped, and nucleated; that of the third, red, disc-shaped, bi-concave, and non-nucleated. The primordial cell advances to development in different orders of living beings. The blood of the invertebrated animals contains coarse granule-cells, which pass forward to the condition of fine granule-cells, and reach the utmost perfection they are there to attain in the colourless nucleated cell of the first period of man. In oviparous vertebrated animals, the development is carried a step further, the red nucleated cell arising, and in them it stops at this, the second period. In mammals the third stage is reached in the red non-nucleated disc, which is therefore the most perfect form." *

---

* BILHARZ in SIEBOLD U. KÖLLIKER'S *Zeitschrift*, 1849, p. 207.

The resemblance here indicated between the *transitory* forms of the blood in the higher animals, and the *permanent* forms of the blood in the lower animals, points at a hidden law of organic combination which will perhaps one day be detected, and which will effect for Biology as much as the law of definite proportions has effected for Chemistry. No one can have studied the development of animals, without being profoundly impressed with the conviction that there is something deeper than coincidence in the recurrence of those forms, however transitory, which characterise the permanent condition of some animals simpler in organisation.

The colourless corpuscles are found by Moleschott to be far more numerous in children than in adults.

---

* DRAPER: *Human Physiology*, p. 115.

The difference between the blood of youth, manhood, and old age, is but trifling; yet there is a continual decrease with age. Women, when in their ordinary condition, have fewer corpuscles than men; but during pregnancy, and other periods, the quantity increases, without, however, reaching that in the blood of children. Albuminous food increases the quantity.*

5. *Is the Blood alive?*—After making ourselves acquainted with these blood-cells and their history, which even the amateur may do with pleasure and profit, we shall have to meet the question—*Is the blood alive?*—a question often debated, and not without its interest to the speculative mind. Harvey † held the blood to be the "primigenial and principal part, because that in and from it the fountain of motion and pulsation is derived; also because the animal heat or vital spirit is first radicated and implanted, and the soule takes up her mansion in it." We see here the influence of the ancient philosophy. Harvey further declares, "Life consists in the blood (as we read in Holy Scripture), because in it the Life and Soule do first dawn and last set. . . . The blood is the genital part, the fountain of Life, *primum vivens, ultimum moriens.*"

Harvey's views were taken up, with modifications, and argued earnestly by Hunter, in his celebrated work *On the Blood.* The constant objection urged against

---

* *Wiener Med. Wochenschrift*, 1854. No. 8.

† HARVEY: *Anatomical Exercitations concerning the Generation of Living Creatures*, 1653. Exc. 51, p. 276.

Hunter by his contemporaries and successors, was their inability to conceive a *living liquid;* but Milne Edwards meets this by saying that it is not the *liquid* which is alive, but the *cells* floating in that liquid, and these he regards as organisms.

The reader must feel that the discussion of such a question cannot be brought to an issue, unless preceded by an accurate definition of the terms employed. What is meant by the blood being alive? If it be meant that an organic structure, having a specific composition, and passing through a definite cycle of changes—such as Birth, Growth, Development, and Death, can truly be said to *live*, then blood, which manifests these eminent phenomena of life, must be pronounced to be alive.

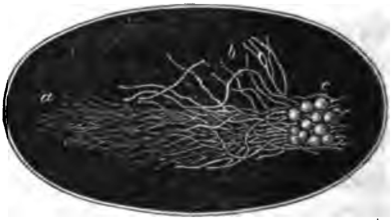
This, however, no one would think of denying. But if it be meant that blood has an *independent* vitality, unlike the vitality of any other tissue, a vitality which can be manifested apart from the organism, the opinion seems wholly untenable. Blood is vital, and has vital properties; but so has every tissue of the body, and in no sense can we attribute to it independent life.

6. *The liquid Plasma of the Blood.*—Having touched upon the floating solids, we must now direct attention to the liquid in which they float. This is called the plasma, or liquor of the blood—*liquor sanguinis.*

Although this plasma is perfectly liquid when in the vessels, yet no sooner does it pour from them, than there separates from it a sort of jelly, leaving behind a yellow fluid. This jelly-like mass is *Fibrine;* the yellow fluid is the *serum*. As the Fibrine separates, and becomes solid, it entraps some of the red-discs in its meshes, and the two are then named the *clot*. The process of becoming solid is called *coagulation*.

The following figure, copied from Virchow, represents the aspect of this coagulated Fibrine, magnified 280 times. At *c* the discs may be observed entrapped in the meshes. The Fibrine itself takes the form of fine and broad fibres, as seen at *a* and *b*.

Fig. 10.



FIBRINE.

The fact of coagulation has always been known; indeed, it could not have escaped observation, being the almost constant result of exposure of blood to the air. The blood of all vertebrate, and many invertebrate animals, has this property; but having observed in several of the Mollusca that no coagulation takes place, I propose, when my researches are finished, to draw up a list of all the animals which are thus excepted from the general law, hoping that these data will furnish some interesting result.

Although the fact was known to the ancients, it was only in the 17th century that this coagulation was discovered to be dependent on the solidification of a peculiar substance. Malpighi* washed the clot free from its discs, and found that the remaining substance was a mass of white fibres. Borelli, at the same epoch, declared this substance to be liquid in the blood, and that it coagulated spontaneously when the blood was drawn from the veins. Ruysch discovered that by whipping the blood, as it poured out, the whipping twigs were covered with a mass of white elastic filaments, exactly similar to that which was got in washing the clot free of its discs.

---

* MALPIGHI: *Opera Omnia*, 1666, p. 123; cited by MILNE EDWARDS.

This substance was named Fibrine by Fourcroy, and it was universally believed to be identical with the substance of muscular fibre. The idea was too plausible not to be at once accepted. The formation of muscle-fibres from fibrine seemed the easiest thing in the world; although really, when the idea comes to be closely examined, it is as wide a departure from all that we know of the formation of muscle, as the notion of eating brains to get more brains, or drinking milk to make milk. We now know that blood-fibrine is *not* the same substance as muscle-fibrine (which is called *musculine*, or *syntonin*), and we know that muscles are not formed by this spontaneous coagulation.

What fibrine is, whence it comes, and what purpose it subserves, are questions upon which science is at present so far from settled that I shall say nothing here about them.*

---

* The student will do well to consult SIMON: *Lectures on Pathology*, p. 49 *et seq.* BROWN-SÉQUARD'S *Journal*, i. p. 296. BRÜCKE: *Ueber die Ursache der Gerinnung des Blutes* in the *Archiv für path. Anat.* xii. 81. VIRCHOW: *Cellularpathologie*, 148. BERNARD: *Liquides de l'Organisme*, i. 455, 466-8; and the works already cited of LEHMANN, MULDER, and MILNE EDWARDS.

There is one point, however, which must be noticed. It has been universally held that the coagulation was simply the solidification of the liquid Fibrine. Nor can there be any doubt but that the chief part is played by the Fibrine. Nevertheless, some observations of Claude Bernard tend to show that the blood entirely deprived of its fibrine can and will coagulate. He whipped out all the fibrine from some blood, and found that, even when it was impossible to get any more, the blood subsequently coagulated. He found, moreover, that the blood of the veins from the kidneys contains no fibrine, yet it coagulates. And he reminds us that pancreatic juice sometimes shows a kind of coagulation which cannot be attributed to the fibrine.[*]

While it thus seems as if blood could coagulate without fibrine, it is known that blood will sometimes not coagulate even when its fibrine may be assumed to be present. Thus Dr Richardson, to whose valuable researches this question is greatly indebted, informs us that "Morgagni had described the blood as quite fluid after death in only four cases: all these were cases in which death ensued from slow arrest of the respiration. Drs Peters, Goldsmith, and Moses, three American physicians, have published a report on the appearance of the blood in 20 cases of death resulting from the excessive use of ardent spirits. In every case the blood was fluid and dark, was of a cherry-juice appearance, and showed no tendency to coagulate. In deaths from narcotic poisons, from delirium tremens, typhoid fever, and yellow fever, the blood is generally described as thin and uncoagulable. Dr John Davy found the blood fluid and uncoagulable on exposure in cases of drowning, hanging, suffocation from the fumes of burning charcoal, and effusion of blood into the pulmonary air-cells." *

---

[*] BERNARD: *Liquides de l'Organisme*, i. 418, 456.

I said that we might assume the fibrine of the blood to be present in these cases, although it is certainly allowable to assume that it may not; but in the following curious example there can be no doubt whatever that the fibrine is present. Dr Richardson has observed, that not only is the blood drawn by a leech entirely uncoagulable, but that the bite of the leech seems to affect even the blood remaining in the bitten vessels; since that blood continues to flow much longer from the wound, than from a wound made by the lancet; and this, he thinks, can only be because the wound is not stopped up by the coagulation.

7. *Cause of the Coagulation.*—Why does the blood coagulate out of the vessels, and not in them? The question has frequently been put, and answered in very contradictory terms. In the form in which it is often put, it seems not less idle than to ask why roses have thorns, why the cohesion of iron is greater than that of clay, or why stupid querists are not entertaining companions? Fibrine coagulates because it is the property of fibrine to coagulate, and would always do so spontaneously, were there not some obstacle present. We may study the conditions which assist,

---

* RICHARDSON: *The Cause of the Coagulation of the Blood,* 1858, p. 34.

and the conditions which arrest this tendency, but it is hopeless to inquire into the cause of the tendency.

It is said that the blood would remain fluid were there no fibrine present; but this fibrine has a spontaneous tendency to coagulate, which can only be prevented by the presence of some solvent. What is that solvent? The researches of Dr Richardson satisfactorily establish some points which go very far towards a demonstration of the true cause, namely, the presence of ammonia in the blood. He shows, in the first place, that ammonia *does* preserve the fluidity of the blood, if it be present in quantities amounting to 1 in 8000 parts of blood containing 2.2 per thousand of fibrine. He shows, in the second place, that the blood does normally contain this volatile alkali, which is rapidly given off during coagulation. And he shows, moreover, that the causes which *retard* coagulation are causes which *obstruct* the giving out of ammonia, whereas the causes which *favour* the giving out of ammonia *hasten* the process of coagulation. Finally, he shows that if the vapour arising from blood be caught in a vessel, and then passed through another mass of blood, the coagulation of this second mass is suspended. The numerous and ingenious experiments by which Dr Richardson has established these important propositions must be sought in his work, which gained the Astley Cooper prize.

There still remain some difficulties, however, which are not cleared up by this hypothesis: I refer especially to the cases in which the blood does not coagulate in spite of exposure to the air, and to several of those mentioned by Brücke in the memoir previously cited.

Brücke's hypothesis is, that contact with the living blood-vessels is the necessary condition which arrests the spontaneous tendency to coagulation. The blood of a carp, he says, will remain uncoagulated for twenty-five hours in contact with the walls of the blood-vessel; but if a glass tube be inserted, so as to preserve the blood from contact with the walls, all the blood inside the tube coagulates just as it would in the open air. This does not explain why the blood is incoagulable after death from suffocation, typhus fever, &c. In fact, we have as yet no demonstration of the cause sought, and must await the result of further researches.

8. *Chemical Composition of the Blood.*—Before concluding our description of the blood, we must glance at its chemical composition; for if the microscope reveals it to be far from a simple fluid, chemical analysis further assures us that it contains water, salts, sugars, fats, and albuminates. In spite, however, of numberless analyses made with the greatest care, our present knowledge is only approximative—a rough estimate, and that is all; the excessive difficulty of making an unexceptionable analysis being acknowledged by all who have attempted it. We know tolerably well what the *elementary* composition is—that is to say, how many atoms of carbon, hydrogen, &c. are included in every 1000 parts; but what the *immediate* composition is—that is to say, in what forms these atoms exist in the blood—we do not know so

well. The elementary composition of ox blood, when all its water is removed, is as follows:—

Carbon,	519.50
Hydrogen,	71.70
Nitrogen,	150.70
Oxygen,	213.90
Ashes,	44.20
	1,000.00

The following may be taken as the nearest approach to a table of the substances which form the *immediate* composition of every 1000 parts of human blood:—

Water,		784.00
Albumen,		70.00
Fibrine,		2.20
Cells,	Globulin,	123.50
	Hæmatin,	7.50
Fats,	Cholesterine,	0.08
	Cerebrine,	0.40
	Seroline,	0.02
	Oleic and margaric acid, Volatile and odorous fatty acid, Fat containing phosphorus,	0.80
Salts,	Chloride of sodium,	3.60
	Chloride of potassium,	0.36
	Tribasic phosphate of soda,	0.20
	Carbonate of soda,	0.84
	Sulphate of soda,	0.28
	Phosphates of lime and magnesia,	0.25
	Oxide and phosphate of iron,	0.50

Extract, salivary matter, urea, colouring matter of bile, accidental substances,	5.47
	1,000.00

In this table sugar is omitted, yet we know that sugar, in varying quantities, always exists in the blood quitting the liver, where it is formed from albuminous matters, and is also generally found in blood at other parts of the organism ; but, because this sugar rapidly undergoes transformation into other substances, its amount cannot be estimated.

All analyses of the Blood hitherto have been only rough estimates. In fact, the fluid itself is constantly changing. The following table, drawn up by Lehmann,* gives the results of analyses of the solid red-discs, and the fluid or *liquor sanguinis*, separately examined :—

1000 *parts Red Discs are composed of*—

Water,	688.00
Solid remains,	312.00
Specific weight,	1.0885
Hæmatin,	16.75
Globulin and cell membrane,	282.22
Fat,	2.31
Extractive matters,	2.60
Mineral substances, excluding iron,†	8.12

---

* LEHMANN, ii. 131.

† The iron is reckoned with the hæmatin.

Chlorine,	1.686
Sulphuric acid,	0.066
Phosphoric acid,	1.134
Potassium,	3.328
Sodium,	1.052
Oxygen,	0.667
Phosphate of lime,	0.114
Phosphate of magnesia,	0.073

1000 *parts Liquor Sanguinis are composed of*—

Water,	902.90
Solid remains,	97.10
Specific weight,	1.028
Fibrin,	4.05
Albumen,	78.84
Fat,	1.72
Extractive matters,	3.94
Mineral substances,	8.55
Chlorine,	3.644
Sulphuric acid,	0.115
Phosphoric acid,	0.191
Potassium,	0.323
Sodium,	3.341
Oxygen,	0.403
Phosphate of lime,	0.311
Phosphate of magnesia,	0.222

But, granting that Chemistry had succeeded in making a perfect analysis, we should still have to bear in mind that all the constituents vary in different individuals, and in different states of the same individual. The blood of no two men is precisely similar; the blood of the same man is not precisely similar in disease to what it was in health, or at different epochs of life.

The iron which circulates in the veins of the embryo, is more abundant than the iron in the veins of the mother; and this quantity declines after birth, to augment again at puberty.

The fats vary, in different individuals, from 1.4 to 3.3 in 1000.

The blood-cells vary with the varying health.

The albumen fluctuates from 60 to 70 parts in 1000, the proportion being greater during digestion.

Tho fibrine, usually amounting to about 3 in a 1000, may rise as high as 7½, or fall as low as 1.

9. *Arterial and Venous Blood.*—Such are the chief points ascertained respecting the blood in general. We must now call attention to the different *kinds* of blood in the different parts of the circulation; for although we speak of "the blood" as if it were always one and the same thing, it is, in truth, a system of various fluids, a confluence of streams, each more or less differing from the other.

The first grand division is familiar to all men—namely, that of venous and arterial blood; the former being dark purple—"black blood," as it is called—the latter bright scarlet.

To many it will seem that this is but a distinction of colour—a distinction so easily effaced, that no sooner does the dark blood come in contact with the atmosphere than it brightens into scarlet. The distinction of colour is, however, the sign of an important difference. Between the two fluids a profound difference exists; and yet the venous blood has only to pass through the lungs in an atmosphere not overcharged with carbonic acid, and at once it becomes transformed into a life-giving fluid. Wherefore? Analysis of the two detects but trifling variations in their solids, the most notable being the larger amount of red-discs and the smaller amount of fibrine in venous blood. But in their gases an important difference is detected. In both there are nitrogen, oxygen, carbonic acid, and ammonia, either free, or combined so feebly that they are easily disengaged. The quantity of nitrogen is much the same in both; that of ammonia probably does not vary, but the oxygen and carbonic acid vary considerably. Indeed, there is a notion current in popular works that venous blood contains carbonic acid, and arterial blood oxygen—*that* being the difference between the two fluids. But the physiologist knows that both fluids contain large amounts of both gases, the difference being only in the relative amounts contained in each. The experiments of Magnus were for a long while held to be conclusive of the opinion that arterial blood contained absolutely *more* carbonic acid than venous blood, although in relation to the amount of oxygen the amount was less; that, in short, it contained more of both gases, but the larger proportion of oxygen gave it its distinction. Recent investigations have considerably shaken this conclusion, but they leave unaltered one result—namely, that arterial blood contains a large amount of carbonic acid, and a still larger amount of oxygen.[*]

Where does the oxygen come from? The atmosphere. Where does the carbonic acid come from? We do not know. The most generally accepted hypothesis is, that the carbonic acid comes from the oxidation of tissue. On this point, however, the evidence seems to me extremely defective, but as I dare not venture here on the discussion, I will content myself with stating the received opinion, referring the student to Bernard's latest work for some grounds of doubt.[†] This is the hypothesis: The blood which flows *to* the tissues is scarlet, but in the capillaries it parts with some of its oxygen; and as it flows *from* the tissues it is dark, and will become scarlet again on its passage through the lungs. When we know that arterial blood contains carbonic acid as well as oxygen, the idea suggests itself, that on parting with some of this oxygen it might assume the dark colour, owing simply to the carbonic acid retained; but this idea is set aside by the fact that unless an *exchange* take place, no oxygen will be liberated. The carbonic acid is said to be the product of the vital activity of the tissues, and as such is taken up by the blood in exchange for its oxygen; for if the nerves which supply a limb be cut, and vital activity be thus arrested, the current of blood will not be darkened; precisely as it will not be brightened in its passage through the lungs, if there be a surplus of carbonic acid in the air. The experiments of Bruch* are very instructive on this point. He found that blood saturated with oxygen became darker *in vacuo*, while blood saturated with carbonic acid did not change colour.

---

[*] On the quantity of gases in the two bloods, see BERNARD, i. 367.

[†] BERNARD: *Liquides de l'Organisme*, i. 336-346; and compare ROBIN et VERDEIL: *Traité de Chimie Anatomique*, ii. 86 *et seq.*

What causes the change of colour when venous blood is submitted to oxygen? Formerly it was held to be due to the iron in the discs; but the iron may be removed without this removal affecting the phenomenon; so that the opinion now held is that the change of colour is due solely to the difference in the *form* of the discs, which become *brighter* as they become more *concave*, and *darker* as they become more *convex*. Oxygen renders them concave, carbonic acid renders them convex.

Arterial blood is everywhere the same: it is one stream perpetually flowing off into smaller streams, but always the same fluid in its minutest rills as in its larger currents.

Not so venous blood. *That* is a confluence of many currents, each one bringing with it something from the soil in which it arises; the streams issuing out of the muscles bring substances unlike those issuing out of the nervous centres; the blood which hurries out of the intestine contains substances unlike those which hurry out of the liver.

---

* SIEBOLD U. KÖLLIKER: *Zeitschrift für Wissenschaftliche Zoologie*, iv. 273.

Not only is venous blood different in different parts of the body, but it has even differences of colour: it is not always black, it is sometimes scarlet, without, however, ceasing to be venous. The discovery of this curious fact is due to Claude Bernard,[*] who found that, in the renal veins, during the *activity* of the kidney, the blood is always scarlet, and black during the *repose* of that organ. He has extended his observation to the submaxillary and parotid glands; and the conclusion he draws is this: while the venous blood from the active muscles is always black, the blood from the secreting glands, when active, is always scarlet; and *vice versâ*, the venous blood is scarlet in the muscles when they are perfectly quiet, and black in the glands when they are quiet. This leads Bernard to establish a distinction between the functional and mechanical activity of the glands: when the chemical process of forming the secretion is going on, he considers the gland to be in a state of functional activity, and then its venous blood is black; when the mechanical process of pouring out that secretion is going on, he considers the gland to be in a state of mechanical activity, and then the blood is scarlet. Although his interesting experiments open a new field of speculation, they are far from having satisfactorily established the cause of this change of colour.

---

[*] *Mémoire lu à l'Académie des Sciences*, 25th January 1858. See his *Leçons sur les Liquides de l'Organisme*, 1859; and BROWN-SÉQUARD'S *Journal*, 1858.

Disregarding these minor variations, we may still keep to the broad distinction of scarlet and black, as characteristic of arterial and venous blood. But these variations help us to conceive how, while arterial blood is everywhere the same, venous blood is everywhere fluctuating in its composition, according to the organs from which it comes.

Wondrously does the complex machine work its many purposes: the roaring loom of Life is never for a moment still, weaving and weaving,

> "Geburt und Grab,
> Ein ewiges Meer,
> Ein wechselnd Weben,
> Ein glühend Leben." *

It is difficult for us to realise to ourselves the fact of this incessant torrent of confluent streams coursing through every part of our bodies, carrying fresh fuel to feed the mighty flame of life, and removing all the ashes which the flame has left. Sudden agitation, setting the heart into more impetuous movement, may make us aware that it is throbbing ceaselessly; or we may feel it beating when the hand is accidentally resting on it during the calm hours of repose; but even then, when the fact of the heart's beating obtrudes itself on consciousness, we do not mentally pursue the current as it quits the heart to distribute itself to the remotest part of the body, and thence to return once more—we do not follow its devious paths, and think

---

* "Birth and the grave, an eternal ocean, a changing motion, a glowing life."—*Faust.*

of all the mysterious actions which attend its course. If for a moment we could with the bodily eye see into the frame of man, as with the microscope we see into the transparent frames of some simpler animals, what a spectacle would be unveiled! Through one complex system of vessels we should see a leaping torrent of blood, carried into the depths and over the surfaces of all the organs, with amazing rapidity, and carried from the depths and surfaces through another system of vessels, back again to the heart: yet in spite of the countless channels and the crowded complexity of the tissues, nowhere should we detect any confusion, nowhere any failure. Such a spectacle as this is unveiled to the mental eye alone, and we cannot contemplate it, even in thought, without a thrill.

10. *The Quantity of Blood.*—It is a natural question, and often asked, but difficult to answer, What *quantity* of blood circulates every minute in our bodies?

Anything like an accurate answer to this question is rendered impossible by the fact overlooked by most physiologists, or unknown to them, that the blood necessarily *varies* in quantity every hour. I was surprised to find striking differences in the amount of blood which flowed from two decapitated Tritons of, as nearly as possible, the same size; but on proceeding to open them, I found the one animal was in a state of active digestion, whereas the other had not eaten for a long while. Since this note was made, Claude Bernard has published his experiments on rabbits and dogs, upon which he founds the startling opinion that the blood of the same animal may, during digestion, amount to double the quantity during fasting;* for he finds that from a rabbit, during digestion, he can remove 30 grammes of blood without killing it.; whereas the same rabbit, fasting, is killed by the removal of 15 grammes. The knowledge of this fact will be very important to surgeons about to perform any operation which entails considerable loss of blood; and it points also to the eminent advantage there would be in giving soldiers a hearty meal before battle.

We thus perceive that no estimate can pretend to be very accurate; nor need we notice those which earlier writers have given: only those of Lehmann, Weber, and Bischoff now command general attention. Lehmann says that his friend Weber aided him in determining the quantity of blood in two decapitated criminals. The quantity which escaped was thus estimated: Water was injected into the vessels of the trunk and head, until the fluid, escaping from the veins, had only a pale red or yellow colour. The quantity of blood remaining in the body was then calculated by instituting a comparison between the solid residue of this pale red aqueous fluid and that of the blood which first escaped. The living body of one of the criminals weighed 60,140 grammes,† after decapitation 54,600 grammes, consequently 5,540 grammes of blood had escaped; 28.560 grammes of this blood yielded 5.36 of solid residue; 60.5 grammes of sanguineous water, collected after the injection, yielded 3.724 of solid substances. There were collected 6050 grammes of the sanguineous water that returned from the veins, and these contained 372.4 of solid residue, which corresponds to 1,980 grammes of blood. The estimate, therefore, turns out as follows: 5,540 grammes escaped after decapitation, and 1,980 remained in the body, thus making 7,520 grammes; in other words, the weight of the whole blood was to that of the body nearly in the ratio of 1 to 8.

---

* BERNARD: *Liquides*, i. 419.

† A *Gramme* is somewhat more than 15 *grains*.

It is obvious from the account of the experiment that only an approximation could be arrived at. And Bischoff's more recent investigations on the body of a criminal, carefully weighed before and after decapitation, lead to the conclusion that the blood amounted to 9¼ lb., or *exactly one-fourteenth of the whole body.** This nearly corresponds with his former investigations, which gave the weight as one-thirteenth of the whole body.

If we say ten pounds for an adult healthy man, we shall probably be as near the mark as possible. The quantity, however, necessarily varies in different persons, as well as in different conditions, and seems from some calculations to be greater in women than in men. In the Seal its quantity is enormous, surpassing that of all other animals, man included.

---

* See his Memoir in SIEBOLD u. KÖLLIKER: *Zeitschrift,* ix. 72. HEIDENHAIN: *Disqu. criticæ et experimentales de sanguini quantitate,* also arrives at a somewhat similar result.

11. *Blood-letting.*—In former days, blood-letting was one of the "heroic arms" of medical practice; and it is sometimes almost appalling to read of the exploits of practitioners. Haller mentions the case of a hysterical woman who was bled one thousand and twenty times in the space of nineteen years; and a girl at Pisa is said to have been bled once a-day, or once every other day, during several years. A third case he mentions of a young man who lost seventy-five pounds of blood in ten days; so that if we reckon ten pounds as the utmost which the body contains at any given period, it is clear that this young man's loss must have been repaired almost immediately. In truth, the blood is incessantly being abstracted and replaced during the ordinary processes of life. Were it not continually renewed, it would soon vanish altogether, like water disappearing in sand. The hungry tissues momently snatch at its materials, as it hurries through them, and the active absorbents momently pour fresh materials into it.

12. *Transfusion.*—In contemplating the loss of blood from wounds or hemorrhage, and in noting how the vital powers ebb as the blood flows out, we are naturally led to ask whether the peril may not be avoided by pouring in fresh blood.

The idea of *transfusion* is indeed very ancient. But the ancients, in spite of their facile credulity as to the effect of any physiological experiments, were in no condition to make the experiment. They were too unacquainted with physiology, and with the art of experiment, to know how to set about transfusion.

Not until the middle of the seventeenth century had a preparation been made for such a trial. The experiments of Boyle, Graaf, and Fracassati, on the injection of various substances into the veins of animals, were crowned by those of Lower, who, in 1665, injected blood into the veins of a dog.

Two years later a bolder attempt was made on man. A French mathematician, Denis, assisted by a surgeon, having repeated with success the experiments of Lower, resolved to extend the new idea. It was difficult to get a human patient on whom the plan could be tried; but one evening a madman arrived in Paris, quite naked, and he was daringly seized by Denis as the fitting subject for the new experiment. Eight ounces of calf's blood were transfused into his veins. That night he slept well. The experiment was repeated on the succeeding day; he slept quietly, and awoke sane!

Great was the sensation produced by this success. Lower and King were emboldened to repeat it in London. They found a healthy man willing to have some blood drawn from him, and replaced by that of a sheep. He felt the warm stream pouring in, and declared it was so pleasant that they might repeat the experiment.

The tidings flew over Europe. In Italy and Germany the plan was repeated, and it now seemed as if transfusion would become one more of the "heroic arms" of medicine. These hopes were soon dashed. The patient on whom Denis had operated again went mad, was again treated with transfusion, and died during the operation. The son of the Swedish minister, who had been benefited by one transfusion, perished after a second. A third death was assigned to a similar cause; and in April 1668 the Parliament of Paris made it criminal to attempt transfusion, except with the consent of the Faculty of Paris. Thus the whole thing fell into discredit, to be revived again in our own day, and to be placed at last on a scientific basis.

It occurred to Majendie that the ill-success of the experiments arose from the supposition that the blood of all quadrupeds was the same, and that it was indifferent whether a man received the blood of another man, or of a sheep or calf. This supposition he thought altogether erroneous. His opinion was, that only the blood of animals of the same species can be transfused in large quantity without fatal results. The blood of a horse is poison in the veins of a dog; the blood of a sheep is poison in the veins of a cat; but the blood of a horse will revive the fainting ass. From this it followed, he thought, that when transfusion is practised on human beings, human blood must be employed; and so employed, the practice is in some urgent cases not only safe, but forms the sole remedy. But the recent investigations of Brown-Séquard have proved that transfusion is successful with the blood of different species. Blundell has the glory of having revived and vindicated the practice of transfusion,* and he has seen his idea amply confirmed. Bérard cites fifteen distinct cases of hemorrhage in which transfusion has saved life.*

---

* BLUNDELL: "Experiments on the Transfusion of Blood," *Medico-Chirur. Trans.* 1818, p. 56.

So startling and so important is this success of Transfusion, that it is very necessary we should distinctly understand in what cases it may with advantage be employed. And these cases are very simple, since it is only when there has been a dangerous loss of blood that any benefit can accrue from transfusion. In all cases of disease it is useless, or worse. The ancients, indeed, thought that by infusing new blood into an old and failing organism, new life would be infused; and wild dreams of a sort of temporal immortality were entertained. Completely as these notions are banished, the initial error of supposing that the blood *forms the organs*—and that if blood be *purified* the organs will be restored to health and vigour—this error is still general, even among men of science. It rests on a misconception of the laws of Nutrition.† Because the organs are nourished by materials drawn from the blood, and because, unless blood be duly supplied, the organs will decay, it has been supposed that the point of departure of Nutrition was in the blood itself, and that the blood formed the organs. It is not so. The organs are, many of them at least, in existence before blood appears;‡ and even afterwards the process of Nutrition always consists in the assimilation of certain materials from the blood by the organs; not in the organisation of this blood itself. In vain will you carry generous food to a sick stomach—it cannot digest the food; in vain will you carry young blood to old organs—they cannot draw their youth from it. The blood is always young, for it is always being renewed. The organs get daily older, and different. Between the blood of an infant, and the blood of a patriarch, no appreciable difference can be found; but how great is the difference between their organs! That which is true of old age, is likewise true of disease. The tissue which is in an unhealthy condition cannot be made healthy by bringing to it a "purer" blood (were such obtainable); it can only be brought back to its healthy condition by the cessation of those causes which keep up the morbid action, and these are not in the blood.*

---

* BÉRARD: *Cours de Physiologie*, iii. 220. It is from this work, and the *Leçons* of MILNE EDWARDS and BERNARD, that all the details on this subject in the text have been taken.

† These laws will be expounded in our final Chapter on LIFE and DEATH.

‡ The young fish *Clypeus* has been observed by Filippi to quit the

13. *Which are the Vivifying Elements?*—Seeing that blood has a power of reanimating the failing body, it is natural we should inquire to which element of the blood this is due—to the cells, or the plasma?

---

egg and swim vivaciously before there is any trace of circulation—before, indeed, the blood-discs are formed.—*Annales des Sciences*, 1847, vii. 67. If the student desires an easy mode of studying this point, let him carefully watch the development of tadpoles; and he will see, in their transparent tails, how the vessels gradually appear, and how the blood-discs, which at first are in all respects the same as the cells of the general substance, gradually lose their large fat-globules, and assume more and more the character of blood-discs.

* On this extremely important subject the student should weigh carefully the whole of VIRCHOW'S *Cellularpathologie*.

We know that it is only necessary to withdraw blood from a part, or prevent its access by a ligature round the arteries, and the part gradually loses all its vital properties; but even after the rigour of the muscles announces death, we have only to readmit the blood by removing the ligature, and the vitality will be restored.

Unless the circulation be maintained in the nerves, their power almost instantaneously disappears; which proves that in the nerves the chemical changes must be very rapid. Swammerdamm, and others since, have shown this by the experiment of tying the aorta; immediately the circulation is interrupted, the power of voluntary movement disappears. This is only true, however, of warm-blooded animals. Frogs may have the aorta tied, and still possess the power of hopping about for some hours. They will even do this for an hour, when the heart and all the viscera are taken away, according to Stilling.* Schiff thinks that they do this in virtue of the remnant of stored-up force retained in their nerves, and the remnant of blood still retained in the parts.† The difference between frogs and warm-blooded animals in this respect, seems to me solely referable to the difference in the normal rapidity of their nutritive changes. The warm-blooded animal "lives fast," and unless fresh blood be constantly present to supply the waste, the vital changes rapidly cease. The frog can live for months without food, which shows that its nutritive changes are comparatively slow. It will therefore require a much longer time before the interruption of the circulation in a frog will interrupt all manifestation of vitality, than in the case of a warm-blooded animal.

---

* STILLING: *Untersuchungen über die Functionen des Rückenmarks*, 1842, p. 38.

† SCHIFF: *Lehrbuch der Physiologie*, 1858. p. 103.

Every one knows how loss of blood causes man to faint; and how checking the action of his heart instantly produces unconsciousness. Our nervous centres demand incessant supply.

Blood, as we said at starting, is the river of life, and where it ceases to flow, all organic action soon vanishes. But blood is composed of two things—the plasma and the cells, or discs. And we return to the question: Which of these is the vivifying element? It has been ascertained that the plasma of the blood, deprived of its cells and fibrine, has no reanimating power when injected, being in fact not more effective than so much warm water. It has also been ascertained that blood, deprived of its fibrine only, produces the same effect as pure blood; whereby it appears that as neither the plasma nor the fibrine possesses the vivifying power, that power must belong to the cells. This is a great step gained, but the restless spirit of inquiry cannot content itself with such a gain, and it asks, What gives to the blood-cells this specific power? Let us see the answer that can be made to such a question.

We know that the cells (discs and corpuscles) carry the oxygen, either in slight combinations, or free, as in vesicles. We know this, because we find that the blood-plasma is unable to absorb much more than one per cent of its volume of oxygen, whereas the blood containing cells absorbs from ten to thirteen times that amount. The change of colour exhibited by the discs as they take up or give out oxygen, and the fact that, if they are placed in a vessel containing air, they absorb oxygen from that air, whereas the plasma does nothing of the kind, are proofs of the cells being the transporters of oxygen.

But this is not all. The experiments of M. Brown-Séquard seem to establish the important fact that it is to the oxygen carried by these cells that we must attribute their *nutritive* agency, and to the carbonic acid carried by them that we must attribute their *stimulating* agency.*

Blood has two offices: it furnishes the tissues with their food, and it stimulates them into activity. Unless the tissues be endowed with certain vital properties, they cannot be stimulated into activity; and when stimulated, this activity brings about a destruction, which must be repaired. If stimulus be applied without equivalent nutrition, the force is soon exhausted. This double office the blood performs, according to M. Brown-Séquard, chiefly through the oxygen, as the *agent of nutrition*, and of carbonic acid, as the *agent of excitation*.

Without accepting his conclusions in all their absoluteness, we may accept thus much of them, for we see him operating on dead animals, or dead parts of animals, by means of *venous* blood charged with oxygen, and producing therewith precisely the same effects as with *arterial* blood; and we see him showing that arterial blood, charged with carbonic acid, acts precisely as venous blood. The conclusion, therefore, is obvious, that the vivifying difference between the two fluids is simply owing to the difference in their amounts of oxygen.

---

* BROWN-SÉQUARD: *Journal de la Physiologie*, 1858, i. 91.

He takes the blood from a dog's vein, and the blood from its artery, whips both till the fibrine be extracted, and till both have become equally scarlet from the absorption of oxygen. He then injects one of these fluids into the right thigh artery of a dead rabbit, in which the rigidity of death has set in for ten minutes, and the other fluid into the left thigh artery. The result is precisely similar in both limbs, namely, in about five minutes both recover their muscular irritability, which they both retain for twenty minutes. Repeating this experiment with blood drawn from vein and artery, but charged with carbonic acid instead of oxygen, he finds a similar result as to the *exciting* power.

Having thus made clear to himself that, as respects nutrition and excitation, there is no other difference between arterial and venous blood than is assignable to their differences in the amount of oxygen and carbonic acid contained in each; that venous blood, charged with oxygen, acts precisely as arterial blood; and that arterial blood, charged with carbonic acid, acts precisely as venous blood, M. Brown-Séquard proceeds with his demonstration, that unless the blood be highly oxygenated it has *no* power of nourishing the tissues; and unless it be highly carbonised, it has *no* power of stimulating them.

We cannot here afford sufficient space to give any account of the experiments by which these conclusions are reached, and must refer the curious reader to the memoir itself.* But as the idea of the stimulating power of the blood residing chiefly in the carbonic acid, will be novel and startling to most physiological readers, it may be useful to mention one of the experiments. A rabbit was suffocated; and, as usual in such cases, the intestine exhibited very powerful disorderly movements. Into a coil of this agitated intestine he injected some *venous* blood highly *oxygenated*. Immediately the movement ceased. He then injected *arterial* blood highly *carbonised*, and the movements were at once resumed. Again he injected oxygenated blood, and again the movements ceased, to appear on the second injection of carbonised blood. "It is possible," he says, "to produce two conditions of the organism essentially different, one of which consists in the presence of a greater amount of oxygen than usual, both in the venous and in the arterial blood, the other of which consists in the presence of an excess of carbonic acid in both fluids. In the first of these conditions, life ceases in spite of the extreme energy of the vital properties, simply because the stimulating power of the blood is insufficient. In the other of these conditions, the stimulating power, being excessive, causes an activity which is soon spent, because it cannot be reproduced."

---

* *Journal de la Physiologie*, i. 95.

Even should we accept to the full the ingenious hypothesis just propounded, we must guard against an exaggeration of its application. Oxygen may be the one chief *condition* for that exchange between the blood and the tissues which constitutes Nutrition, and without a due supply of oxygen Nutrition may be brought to a stand-still; but we shall greatly err if we suppose that oxidation is itself the process of Nutrition, or that the cells are the sole agents. The albumen, the fats, and the salts which the tissues draw from the blood, are not drawn from the cells, but from the plasma. It is, therefore, quite possible, indeed M. Séquard's experiments render it eminently probable, that the blood-cells, by their oxygen, furnish the indispensable *condition* of Nutrition, the material being furnished by the blood-plasma. It is also probable that the cells, by their carbonic acid, furnish the condition of muscular excitement; so that arterial blood, when containing more than its usual amount of carbonic acid, causes an excess of the stimulating over the repairing processes. This may account for the greater cerebral excitement succeeded by languór, consequent on exposure to the vitiated atmosphere of a theatre, a ball-room, or a lecture-room.

14. *Relation of Blood to Nutrition.*—Such is the wondrous fluid we name Blood, and such its proper-

ties, as far as Science hitherto has learned them. Before quitting our survey, it will be desirable to say a few words respecting the relation which blood bears to Nutrition, since that relation is not generally understood.

Every one knows that all the tissues are nourished by the blood. But in what way is this effected? Blood, in itself, is perfectly incapable of nourishing the tissues—so incapable that, if it be poured on them from the rupture of a vessel, it hinders nutrition, and acts like a foreign substance. Accordingly, we see it rigorously excluded from them, shut up in a system of closed vessels; but as it rushes along these vessels, *certain of its elements ooze through the delicate walls* of the vessels, and furnish *a plasma from which the tissues are elaborated.* In exchange, certain products of waste are taken up by the blood, and carried to the organs of excretion.

An image may render the process memorable. The body is like a city intersected by a vast network of canals, such as Venice or Amsterdam; these canals are laden with barges which carry to each house the meat, vegetables, and groceries needed for daily use; and while the food is thus presented at each door, the canal receives all the sewage of the houses. One house will take one kind of meat, and another house another kind, while a third will let the meat pass, and take only vegetables. But as the original stock of food was limited, it is obvious that the demands of each house necessarily affect the supplies of the others. This is what occurs in Nutrition: the muscles demand one set of principles, the nerves a second, the bones a third, and each will draw from the blood those which it needs, allowing the others for which it has no need to pass on.

This leads us to notice a luminous conception, attributed by Mr Paget to Treviranus, but really due to Caspar Friedrich Wolff, whose doctrine of epigenesis reposes on it;* namely, that "each single part of the body, in respect of its nutrition, stands to the whole body in the relation of an excreting organ." Mr Paget has illustrated this idea with his accustomed felicity.† Every part of the body taking from the blood those substances which it needs, acts as an excretory organ, inasmuch as it removes that which, if retained, would be injurious to the nutrition of the rest of the body. Thus the polypes excrete large quantities of calcareous and silicious earths: in the polypes, which have no stony skeleton, these earths are absolutely and utterly excreted; but in those which have a skeleton, they are, though retained within the body, yet as truly excreted from the nutritive fluid and the other parts as if they had been thrown out and washed away. In the same manner, our bones excrete the phosphates from our blood. The hair in its constant growth not only serves its purposes as hair, but also as a source of removal from the blood of the various constituents which form hair. "And this excretion office appears in some instances to be the only one by which the hair serves the purpose of the individuals ; as, for example, in the fœtus. Thus in the fœtus of the seals, that take the water as soon as they are born, and, I believe, in those of many other mammals, though removed from all those conditions against which hair protects, yet a perfect coat of hair is formed within the uterus, and before, or very shortly after birth, this is shed, and is replaced by another coat of wholly different colour, the growth of which began within the uterus. Surely in these cases it is only as an excretion, or chiefly as such, that this first growth of hair serves to the advantage of the individual."

---

* WOLFF: *Theorie von der Generation.* 1764.

† PAGET: *Lectures on Surgical Pathology,* i. 24 *et seq.*

Mr Paget also applies this principle to the explanation of the rudimental hair which exists all over our bodies, and to that of many other rudimental organs, which subserve no function whatever. He also, without apparently being aware of Wolff's ideas on this point, applies it to the explanation of the embryonic phases. "For if it be influential when all the organs are fully formed," he says, "and are only growing or maintaining themselves, much more will it be so when the several organs are successively forming. At this time, as each nascent organ takes from the nutritive material its appropriate constituents, it will co-operate with the gradual self-development of the blood, to induce in it that condition which is essential, or most favourable, to the formation of the organs next in order to be developed."

This principle further enables us to understand how the existence of certain materials in the blood may determine the formation of structures in which these materials are to be incorporated; and it enables us to understand the "constitutional disturbance," or general state of ill health, which arises from some local disturbance, such as a cold in the head; for, "if each part in its normal nutrition is an excreting organ to the rest, then cessation or perversion of nutrition in one, must, through definite changes in the blood, affect the nutrition of the rest."

How evidently the special condition of the organism determines the growth or decrease of certain organs, may best be seen in the sudden development of the beard and the voice at the period of puberty. Birds in the pairing season acquire their most brilliant plumage, and express the tumult of their emotions in perpetual song. Stags at the same epoch develop their antlers, and make the forest ring with their hoarse barking. Mr Paget justly says—"Where two or more organs are thus manifestly connected in nutrition, and not connected in the exercise of any external office, their connection is because each of them is partly formed of materials left in the blood on the formation of the other." *

Does not this throw a new light upon the blood? and do you not therein catch a glimpse of many processes before entirely obscure? It assures us that the blood is not "flowing flesh"—*la chair coulante*—as Bordeu called it, to the great delight of his successors;

---

* PAGET, p. 32.

nor is it even liquid food. It is an organic structure, incessantly passing through changes, which changes are the *conditions* of all development and activity. Food and Drink become subjected to a complicated series of digestive processes. The liquid product of Digestion is carried into the blood-stream, undergoing various changes in its route. It is now blood; but other changes supervene before this blood is fitted for the nourishment of the tissues; and then certain elements pass from it, through the walls of the capillaries, to be finally assimilated by the tissues. In the simpler animals, the liquid product of digestion is itself the immediate agent of Nutrition, and does not pass through the intermediate stage of blood. It escapes from the digestive canal into the general substance of the body, which it permeates and nourishes much in the way that the blood-plasma nourishes the substance of the more complex animals. But in the simplest animals there is not even this approach to blood. There is no liquid product of digestion, for there is no digestion at all, the water in which these animals live carrying organic matter in solution; *this* permeates the substance, and is assimilated: thus does the water play the part of blood, carrying the food, and carrying away the waste.

Let the speculative mind traverse the marvellous scale of created beings upwards, from the simplest to the most complex, and it will observe that Assimilation first takes place by means of the direct relation of the organism to the surrounding medium; next arrives the interposition of agencies which *prepare* the food for the higher effects it has to produce; and instead of relying on organic substances in solution, the organism is seen extracting nutriment from other organisms; finally is seen the operation of still more complicated agencies, which impress on the digested food still higher characters, converting it into blood. This blood is retained in a system of vessels everywhere closed. Yet, in spite of the absence of orifices or pores, it is distributed impartially to the most distant parts of the organism, and it is distributed according to the momentary requirements of each part, so that when an organ is called upon to put forth increased energy, there is always an increase of food sent to supply that energy. If the stomach has been quiet for hours while the brain has been active, the regulating power of the circulation has adapted the supply of blood to each organ; and no sooner will the stomach be called upon to exert itself, than an abundant supply of blood will instantly be directed to it. This simple and beautiful fact in the animal economy should warn men against the vicious habit of studying at, or shortly after, meals, or of tasking the brain when the stomach is also tasked.

CHAPTER V.

CIRCULATION OF THE BLOOD: ITS HISTORY, COURSE, AND CAUSES.

Was Harvey the discoverer of the circulation?—Course taken by the blood—History of seventeen centuries—Three errors removed by Galen, Vesalius, and Colombo—What then remained for Harvey?—Discovery of the valves—Exaggeration of their importance: veins without valves—Discovery of the fact of the circulation—Reception of the discovery by contemporaries—Deficiencies in Harvey's doctrine—Discovery of the capillary vessels: their structure and uses—Discovery of the lymphatics—Harvey's opposition to it—Statement of Harvey's real claims—The cause of the circulation—Influence of the heart; not the sole cause—Rapidity of circulation—Circulation in absence of the heart—Motions of the heart; cause thereof; nerves and ganglia of the heart—The heart continuing to beat after death—Action of the Arteries—Circulation in the capillaries—Draper's hypothesis—Spallanzani's observations—Circulation and Respiration.

DID Harvey discover the circulation of the blood? To many, the question will sound like an impertinence. To those who have critically examined the historical evidence, the question wears another aspect, and their answer will run somewhat thus: Harvey did, and he did *not*, make the discovery; he made a very great discovery, which has given an imperishable glory to his name, but it was not precisely that which is popularly attributed to him. In endeavouring to mark clearly out that which he discovered, and that which he did not discover, no attempt will be made here to diminish the fame England is justly proud of, by ransacking the archives of science to detect stray passages of meaningless vagueness, wherein older authors may have indicated something like the truths which Harvey established on the firm basis of experiment and reasoning. Erudite prejudice has done its worst in this direction, and its worst has only set Harvey's merits in a clearer light.

Harvey discovered the *fact* of the circulation; but he did not discover the *course* of the circulation, nor the *causes* of the circulation. He knew that the blood was carried from the heart through the arteries to the tissues, and from the tissues, through the veins and lungs, back again to the place whence it started. But he knew not *how* the blood passed from arteries to veins; he knew not *why* the blood thus moved. In our day Science is in possession of the exact *course* of the circulation, but the exact *causes* are still under question. We know that the circulating system consists of *heart, arteries, veins, capillaries,* and *lymphatics.* Harvey knew not the capillaries and lymphatics; so that his knowledge of the course taken by the blood was necessarily incomplete.

To put the reader in possession of what is now known on this subject, and to enable an estimate to be formed of what Harvey discovered, we will first take a rapid view of the circulation.

The following diagram will convey a general conception of the course of the circulation:—

Fig. 11.

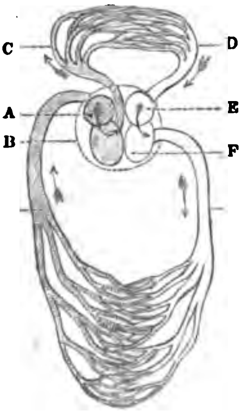


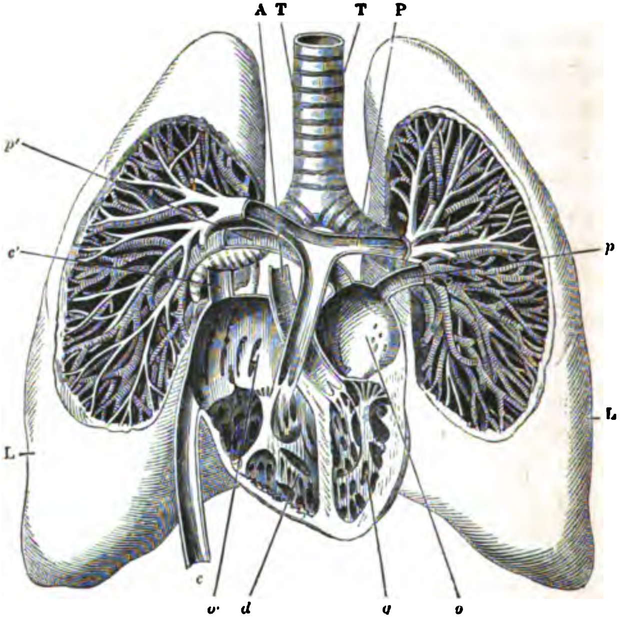
Diagram of Circulation.

The reader will understand that this figure is a *diagram*, not a *picture:* it represents, in an ideal manner, the relation of the various parts of the circulating system to each other, and shows how a circle is formed by the blood starting from the *right auricle*, A, of the heart, and passing thence into the *right ventricle*, B; from this again it is driven by a strong contraction of the *pulmonary artery*, C, into the lungs; from the lungs it is carried by the *pulmonary veins*, D, back to the *left auricle*, E, of the heart, and from that to the *left ventricle*, F, whence it is poured into the arteries, and passes from these through the *capillaries* (described in our last chapter, p. 241, and represented by dotted lines in fig. 11) into the veins, and thus is once more carried back to the heart.

Such is the diagram, or *theoretic* view, of the circulation. But lest the reader should be misled by this, which is intended to simplify the conception, I will now give a representation of a part of the actual disposition of the circulating apparatus, which he can complete by the aid of the theoretic diagram.

The heart is composed of four cavities, two *auricles* or antechambers, and two *ventricles* or chambers. Into the right auricle (*o'*) the blood enters by means of the two great trunk-veins, the superior and inferior *venæ cavæ* (*c'* and *c*). After passing thence into the right ventricle (*d*) it is driven by the *pulmonary artery* (P) into the lungs. This vessel is called an artery although it contains venous blood; for vessels do not receive their names from the nature of the blood they carry, but from the nature of their office: those which carry

Fig. 12.



HEART AND LUNGS.

blood to the heart are called veins, those which carry it from the heart are called arteries.

In the lungs (L L) the blood comes in contact with the oxygen of the atmospheric air, admitted to the air-cells of the lungs through the windpipe, or *trachea* (T) which branches off, and ramifies through the lungs as *bronchial tubes.* The action of the oxygen changes the blood from venous into arterial.

The blood now returns from the lungs by the *pulmonary veins* (*p p'*) and enters the left auricle (*o*), whence it passes into the left ventricle (*g*). It is then driven into the arteries, through the great arterial trunk, the *aorta* (A), and is by them distributed to the various tissues.

The arterial blood having served its purpose, and having yielded some of its materials to the tissues through which it flowed, has now to return once more to the heart, to be reinvigorated by passing through the lungs. In the diagram we saw this return effected means of the veins. But there is another system of vessels, different from veins and capillaries, of which no notice is taken in the diagram. This is the Lymphatic system.

The Lymphatics, like the roots of plants absorbing nutriment from the earth, absorb lymph from the various organs in which they are distributed, as we shall learn more particularly hereafter. These lymphatics pour their contents into the venous current, which thus becomes a confluence of streams, all flowing towards the heart.

This is the circuit taken by the blood, from the right auricle of the heart back again to that starting-point.

I°. Story of the Discovery.—The story of this discovery is one of the most interesting and instructive in the whole range of science, and it has recently been re-written by M. Flourens in a very agreeable style.* He declares that before him no one had accurately narrated it. In some sense this is true; but there are important omissions in his own account; and while availing myself of his labours, I shall endeavour to complete them. It is a story whose episodes extend over not less than seventeen centuries; and the two centuries that have elapsed since the discovery, have not sufficed entirely to complete it. Seventeen centuries is a vast span of time for the elaboration of the discovery of a fact which, now we know it, seems so obvious that the marvel is why it was ever unknown; and the moral of the story lies precisely there, teaching, as it does, the remarkable servility of the mind in the presence of established opinions, and the difficulty which is felt, even by eminent men, in seeing plain facts, when their eyes are hoodwinked by preconceived notions. To those who are unfamiliar with the practical investigation of science, it seems singular that errors, so baseless that they vanish immediately they are challenged, should continue to find believers: and to men who have never trained themselves in the difficult and delicate art of Observation, it seems singular that facts, extremely simple when observed, should continue to be overlooked. But the truth is, observers are at all times rare, because *new* observation requires singular independence of mind; and, unhappily, those who never made an observation themselves, are always ready to dispute the accuracy of new observations made by others.

---

* Flourens: *Histoire de la Découverte de la Circulation du Sang.* 1854.

1. *Three errors which masked the truth.*—In the case now before us there were three capital errors, which for seventeen centuries masked the fact of circulation; and the reader will probably learn with surprise what those errors were.

The first error was, the belief that the arteries did not contain blood.

The second error was, the belief that the two chambers of the heart communicated with each other by means of holes in the partition dividing them.

The third error was the belief that the veins carried the blood *to* the various parts of the body.

How was it possible that errors so flagrant as these could have maintained their ground a single day after men began seriously to examine the subject? It was obvious that air entered the body, and entered it by the trachea, or windpipe. The conclusion was natural, in the early days of science, that the air, thus entering by the trachea and bronchial vessels, should continue its course through other vessels. When the arteries of a dead body were examined, they were found empty, and consequently the arteries were chosen as the veritable air-channels: hence their name ("air-containers," from *ἀηρ* and *τηρεω*).

What, then, becomes of the air inspired? Galen said it did not enter the parts of the body, but was thrown out again after performing its office, that office being to *cool* the blood. If you open an artery, said Galen, blood will issue, but not air: whence the conclusion seems inevitable, that the arteries do not contain air, and do contain blood. Modern science has proved that atmospheric air is not contained in the arteries, but only its oxygen with a slight amount of nitrogen, and a certain amount of carbonic acid gas. And as the composition of the atmosphere was not suspected in the days of Galen, the presence of blood, and the absence of air, were facts so firmly established by him, that, in spite of all antagonists, they finally assumed the place of incontestable truths.

Here, then, we see one error removed. The others still remained. Galen, and all his successors, maintained that the two chambers of the heart communicated directly by means of holes in their partition; an opinion perfectly intelligible when we learn that it rested on a theoretical assumption. Theory wanted the fact, and men saw the fact they wanted. Theory distinguished between venous blood, and *spirituous* or arterial blood: The venous blood nourishing all the coarser organs, such as the liver; the spirituous blood nourishing all the delicate organs, such as the lungs.

In our day we should demand some proof, before accepting such a theory; but the ancients had a very vague idea of the necessity of proof; and were equally vague respecting the nature of proof: so long as an opinion was logical and plausible, it was held to be irresistible.

The spirituous element was supposed to be formed in the left ventricle of the heart; but inasmuch as even venous blood requires some of this spirit for the purposes of nutrition, it was necessary that the two bloods should mingle; and, to meet this necessity, holes were assumed in the partition dividing the two ventricles. So deeply impressed were anatomists with reverence for what Galen had said, and what theory required, that they one and all *saw* the holes—which do not exist. Berenger de Carpi had, indeed, an uneasy doubt on the subject, which he naïvely expressed in the admission that the holes were only to be seen with great difficulty—*cum maximâ difficultate videntur;* but, by straining the eyes sufficiently, he doubtless saw what Galen required him to see—as thousands daily see what they believe they ought to see. The first man who had sufficient strength of mind to use his eyes, and say what he saw, was Vesalius,* the father of modern anatomy: for whom Titian did *not* draw the figures which illustrate his work.†

---

* VESALIUS: *Opera Omnia,* edit. 1725, i. 519. The first edition of the Anatomy was published in 1543.

† The real artist was Calcar, a pupil of Titian's, called by Vasari, Giovanni Fiammingo. I do not know how the tradition arose which attributes the figures to Titian—(CUVIER, *Hist. des Sc. Nat.,* ii. 21). Vesalius himself names *Johannes Calcarensis* in his preface, and makes no mention of Titian.

Thus was the second error overthrown in 1543. The third error—namely, that of the veins carrying the blood *to* the tissues—was somewhat more complex. If the venous and arterial bloods do not mingle in the heart, where do they mingle? *We* know it is in the lungs that the one passes into the other; but it was an immense discovery to make; and there is something piquant in the fact that it was first divined by a restless and daring theologian, whom Calvin burned, with affectionate zeal, for speculations of another kind.

Michael Servetus was the first to announce the existence of the circulation in the lungs; and he announced this in the *Christianismi Restitutio,* a work which was burned by the theologians. Two copies of this work exist: one, still reddened and partly consumed by the flames, is in the Royal Library of Paris; and copious extracts from it are printed by M. Flourens in his "History." Servetus there describes the passage of the blood from the heart to the lungs, "where it is agitated, prepared, changes its colour, and is poured from the pulmonary artery to the pulmonary vein." *

This idea was as novel as it was true; but the critical reader will probably not agree with M. Flourens in regarding this as a *discovery,* in the strict sense of the term; for although Servetus had some notion of the anatomical evidence furnished by the large size of the pulmonary artery, which enables it to carry a far greater quantity of blood than could be needed for the nutrition of the lungs, yet we have only to read the passages in which he describes this pulmonary circulation to perceive that he had no accurate idea of it: he speaks confidently of the nerves being continuations of the arteries, and describes with great precision how the air passes from the nose into the chambers of the brain; and how the devil takes the same route to lay siege to the soul. All we can say is, that Servetus made one lucky guess among his numerous guesses by no means of the lucky kind. I think it right to insist on this point, because, owing to the rarity of the book in which the guess was promulgated, very few persons have been able to form a correct idea of what Servetus really meant. Thus Cuvier, among others, emphatically asserts that "the physiological phenomenon of the pulmonary circulation is explained in a very clear manner,"* and that Servetus says, "in a positive manner, that the whole mass of the blood passes through the lungs." The copious extracts given by M. Flourens now enable every one to see what Servetus really did assert. He announced the fact of the pulmonary circulation, and may receive from History the whole credit of such priority; he also guessed rightly that in the lungs, and not in the liver, the blood received its elaboration, passing from venous into arterial.

---

* "Fit autem communicatio hæc, non per parietem cordis medium, ut vulgo creditur. Sed magno artificio a dextro cordis ventriculo longo per pulmones ductu, agitatur sanguinis subtilis: a pulmonibus præparatur, flavus efficitur, et a vena arteriosa in arteriam venosam, transfunditur."

But whatever merit may be assigned to Servetus, no influence can be attributed to his discovery, since both he and his treatise were roasted by Calvin; and no one heard of the pulmonary circulation. Six years afterwards, Realdo Colombo re-discovered the pulmonary circulation;* and that the discovery was ready to be made on all sides, is seen in the fact of its being also made by Cæsalpinus, the great botanist, who does not seem to have been aware of what Colombo had written, since he makes no mention of him, and, as M. Flourens observes, *le grand mérite est toujours probe.* Cæsalpinus, moreover, was the first to pronounce the phrase "circulation of the blood."†

---

* Cuvier: *Hist. des Sciences Nat.*, ii. 15.

2. *Discovery of the general Circulation.*—Here the reader may ask, What further remained for Harvey to discover? and it may surprise him to hear the answer: Everything! Such is pretty nearly the fact. The pulmonary circulation takes place only through a small arc of the great circle traversed by the blood. Besides this arc, there is the other greater arc, through which the *systemic*, or general circulation, takes its course; and of this no one, except Cæsalpinus, had even a suspicion. Every one supposed that the veins carried the blood *to* the tissues, and nourished them; no one suspected that this function was reserved for the arteries, and that the veins carried the blood only to the heart. It was thought that the arteries had their origin in the heart, and the veins in the liver; from the liver the veins carried the blood to every part.

---

* COLOMBO: *De re Anatomicâ*, edit. 1572, p. 325.

† CÆSALPINUS: *Quæst. Peripatet.*, edit. 1593, lib. v. p. 125. Flourens gives the passage, as well as that from Colombo.

A single fact, familiar to every surgeon, and to every barber who ever opened a vein, ought to have revealed the error; since every time a ligature was applied, the operator must have seen that the vein swelled *below* the ligature, and not *above* it; from which the deduction seems obvious that the blood in the vein flowed *to* the heart, and not *from* it.

But here, as in so many other cases, familiar facts were not *observed;* they were seen, but not interpreted. Cæsalpinus was the first who observed it; but he has only the merit of having suspected the cause to be the current setting towards the heart.[*] His suspicion was not a demonstration; and we are surprised to find De Blainville saying, "that the circulation was known to Cæsalpinus, although he had not demonstrated it." In Science, the difference between a guess and a demonstration is as great as that between the fame which an unwritten poem may achieve, and the fame which a great poem *has* achieved. If guesses counted as achievements, the temple of fame would be thronged with the statues of heroes.

De Blainville says that the reason why Haller and others have denied the claim of Cæsalpinus, is because they did not read what he had said in his work *On Plants.*[†] Now, if we turn to the passage in question, we shall see how far the writer really was from the truth, and yet how near his guess went to the truth. "In animals, we see the food carried by the veins to the heart, as to a centre of innate heat; and there, having acquired its final perfection, it is distributed over the whole body through the arteries, by the agency of the spirit, which is engendered in the heart by this same food."[*]

---

[*] "Quia tument venæ ultrà vinculum, non citrà. Debuisset autem opposito modo contingere, si motus sanguinis et spiritus a visceribus fit in totum corpus."—*Quæstionum Medicarum,* lib. ii. p. 234.

[†] DE BLAINVILLE: *Hist. des Sciences de l'Organisation,* ii. 227—a statement repeated by ISIDORE GEOFFROY ST HILAIRE: *Histoire Générale des Règnes Organiques,* i. 44.

Easy is at is for us to *read into* this passage almost all that we understand by the circulation, a close historical criticism detects in it nothing but a guess; and as Bérard remarks, we ought not to confound two such vague statements as these, themselves requiring demonstration, and by Cæsalpinus himself subsequently contradicted, with the clear ideas, and imposing proofs, on which Harvey established his discovery.[†]

The convincing evidence of Harvey's originality is, that not only was the guess advanced by Cæsalpinus without any influence on the theories of that day, in spite of his deserved authority; but that when Harvey promulgated his theory, he found, all over Europe, the greatest difficulty in getting it accepted. Bérard maintains, that so far from any one before Harvey having had a clear idea of the true theory, no one had even accurately conceived the true theory of *pulmonary* circulation; for although Servetus, Colombo, and Cæsalpinus knew that the blood passed through the lungs, they fancied only so much passed as was necessary for the reception of the "vital spirits,"—a quantity which their predecessors fancied took its course through the holes in the partition of the heart. But they had no conception of the entire mass of blood traversing the lungs; and even had they known so much, they would have been wholly at a loss to say whence it came, and whither it went. It was necessary to understand the whole circulation before any part of it could accurately be understood.

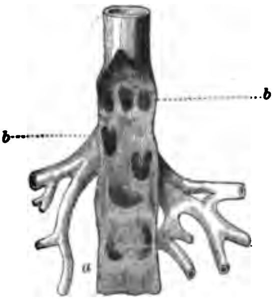
---

[*] CÆSALPINUS: *De Plantis*, i., c. 2, p. 3.—"In animalibus videmus alimentum per venas duci ad cor tanquam ad officinam caloris insiti, et adeptâ inhibi ultimâ perfectione, per arterias in universum corpus distribui agente spiritu, qui ex eodem alimento in corde gignitur."

[†] BERARD: *Cours de Physiol.*, iii. 581. Compare MILNE EDWARDS: *Leçons sur la Phys. et l'Anat. comparée*, iii. 20.

3. *Discovery of the Valves.*—The discovery that the veins had valves, opening and closing like doors, brought the discovery of the circulation within compass. It was made in 1574 by Fabrice d'Acquapendente, under whom Harvey studied at Padua. In the following figure the valves *b b b* are represented in the course of the venous trunk, and at the entrance of the venous branches. These valves, preventing any flow from the heart, but admitting the flow to the heart, might have suggested to their discoverer the true interpretation of their use. But five-and-forty years elapsed before any one arose, who had the sagacity to perceive the real value of this anato-

Fig. 13.



Valves of the Veins.

mical structure in respect to the blood-currents; and during these five-and-forty years, everything that had been discovered or surmised respecting the circulation, was familiar to every anatomist of the great Paduan school in which Harvey studied: nevertheless, when Harvey promulgated his theory, it was vehemently opposed.

In 1619 Harvey first publicly taught what he had discovered; and in 1628 he published, for the benefit of Europe, his celebrated treatise, *Exercitatio Anatomica de Motu Cordis et Sanguinis,* which may justly be called the basis of modern physiology. That the theory was new, and would be opposed as a heresy, no one more clearly divined than he did.* The greatness of the discovery, and the force of genius required to make it, can only be appreciated by those who, familiar with the state of opinion in those days, read the evidence and arguments by which Harvey established his doctrine. It is true that he appeared at a particular epoch, when the confluence of various discoveries rendered his discovery possible; but that a man of genius was necessary to interpret and co-ordinate those discoveries, is evident in the fact, that no one except Harvey had, for nearly half a century, seen the significance of the facts.

Here, however, a caution must be interposed. The importance of the valves has been greatly exaggerated, and their real bearing on Harvey's discovery misconceived. They are thought to have rendered the discovery facile, because, inasmuch as they prevent the blood from flowing backwards, while permitting it to flow outwards, the idea of the circulation, it is said, must necessarily have emerged from the contemplation of these valves.

---

* "Adeo iis nova erunt et inaudita, ut non solum ex invidia quorundam metuam malum mihi, sed vereor ne habeam inimicas omnes homines, tantum consuetudo aut semel inhibita doctrina, altisque defixa radicibus, quasi altera natura apud omnes valet, et antiquitatis veneranda opinio cogit."—*Exercit.* p. 88.

Against this supposition there is one decisive fact: no one *did* deduce the conclusion which is said to have been so necessary.

Moreover, in many cases, circulation takes place entirely without their aid. There are *no* valves in the veins of the Invertebrata,* *none* in the veins of fishes and reptiles, and *very few* in birds; yet the circulation is as complete in these animals as in man.

Nay, even in man the *chief* veins are destitute of valves, although writers on Natural Theology, and even better-informed physiologists, are in the habit of speaking of them as if they were universal and indispensable: it may, therefore, be useful to mention, that there are no valves whatever in the great venous trunks, the venæ cavæ, and portal veins—none in the hepatic, renal, and uterine veins—none in the brachio-cephalic, spinal, and iliac veins; and they are rarely present in the azygos and intercostal veins.

4. *Harvey's Originality.*—M. Flourens says, that when Harvey appeared everything had been suspected

---

* In the aorta of the *Eolis* there is a semilunar valve.

or indicated, but nothing established. This seems to me even less than the truth, for I cannot ascertain that any one had the slightest conception of the real process.

Acquapendente could make nothing of the valves he had detected. He thought their office was simply to prevent a too great accumulation of blood in the lower parts of the body, and a diminution from the upper parts.

Colombo thought with his contemporaries, that the veins had their origin in the liver, and carried blood to the tissues.

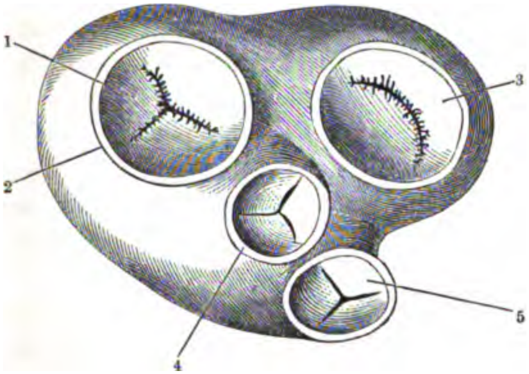
Cæsalpinus, in spite of his recognition of pulmonary circulation, thought the blood also passed from the right chamber of the heart to the left.

But Harvey not only conceived a clear idea of the process, he described it minutely and accurately. He noticed the successive contractions which forced the blood into the ventricle when the auricle contracted, and forced it from the ventricle into the lungs when the ventricle contracted; a process repeated on the left side with the blood which had come in contact with air in the lungs. And at each passage of the blood from one cavity to another, there were the valves, or "little doors" (*ostiola*), opening to let the current pass, and closing to prevent its reflux.

These valves are represented in the following figure, which shows the upper surface of the heart, when the *auricles* are removed :—

Harvey described the course of the blood along the arteries, which he attributed to the pulsations of the heart; and in this, instead of in Galen's "pulsific virtue," he recognised the cause of the blood's movement.

Fig. 14.



VALVES OF THE HEART AND ARTERIES.

Upper surface of the heart, the auricles having been removed.

1. Valve between right auricle and ventricle; 2. Fibrous ring; 3. Valve between left auricle and ventricle; 4. Valve of the aorta; 5. Valve of pulmonary artery.

5. *Reception of the new Doctrine.*—By Harvey the overthrow of ancient authority was completed. Men dared no longer swear by Galen—they swore by Harvey, who had discovered the greatest fact in the animal economy—a fact totally unknown and unsuspected by Galen, or any other ancient. The new era had commenced.

It was not in the nature of things for the old system quietly to accept the new; accordingly, the opposition was loud and vehement. Like many other parts of this history, and like most oppositions to new doctrines, it has been immensely exaggerated by historians, and by writers who have chosen it as a theme for rhetoric. It is true that the Faculty rejected the new doctrine; but it is no less true that eminent men accepted it with enthusiasm. If Guy Patin was caustic in opposition, Molière laughed at Guy Patin's prejudice; and Boileau ridiculed the Faculty. Some anatomists accepted the doctrine, and the great Descartes warmly espoused it.* Swammerdamm and Malpighi, two of the great names of the century, speak of Harvey with reverence; and soon no one spoke of him in any other tone.

6. *Gaps in the Doctrine of Harvey.*—It is impossible to read Harvey's work without the highest admiration for the scientific genius it displays, and the conviction that here the circulation was not only demonstrated, but for the first time conceived. The experiments and arguments by which he establishes the fact are still worthy of study, as models of investigation.

But there were necessary gaps in his doctrine. The

---

* The carelessness with which history is often written, is exhibited in the criticism of M. ISIDORE ST HILAIRE: *Hist. des Règnes Organiques*, i. 49; on the passage in CUVIER, *Hist. des Sciences Nat.*, ii. 53, where Harvey is said to have had the rare happiness of seeing his discovery accepted by Descartes. M. St Hilaire remarks that this is an error, because Harvey died in 1657, and Descartes did not publish his *Traité de l'Homme* until 1662. This remark is doubly unfortunate, Descartes having expressed his adherence to the doctrine in his very first work, the *Discours de la Méthode;* and Harvey having, in his Second Reply to Riolan, expressed his gratification at this flattering approval of Descartes.

course of the circulation was not known to him, could not indeed have been discovered by any instruments at his disposal. He supposed the blood passed from arteries to veins by two paths, either through anastomosis (that is to say, the arteries opening directly into veins), or through the porosities of the parts—*aut porositates carnis et partium solidarum pervias sanguini.* He thought it necessary that so much of the blood as was required by the tissues for their nutrition, should *remain behind* in the tissues, and the rest be carried onwards to the heart. The error is considerable; and its bearing on the theory of the circulation will be appreciated by any one who reflects on the fact that venous and arterial blood being so obviously distinguished, it is necessary that the passage of the one into the other should be demonstrated—not surmised—before the theory of the circulation could be accepted as complete; for any one might reasonably assume that the blood in the veins is altogether another fluid from that in the arteries, and not merely another state of that fluid.

This, indeed, *was* assumed by the adversaries of Harvey, and has found supporters even in our own day. Burdach cites two German physiologists—Willbrand and Runge—whom he thinks worthy of refutation, and who maintained that arterial blood was transformed in a mass into the tissues, and that venous blood was the re-transformed tissues. Unless the passage of the blood into the veins be clearly traced, there can be no reason against supposing that the veins simply absorb from the tissues, in the same way as the lymphatics and lacteals absorb their fluid. To prove that the blood makes a circuit, that circuit must be traced; and Harvey plainly declares that, with all his diligence, he could not succeed in tracing any connection between arteries and veins; in only three places did he find them presenting anything like an anastomosis; in every other place he imagined porosities.

7. *Discovery of the Capillaries.*—Nor, with the means at his disposal, *could* Harvey have traced the complete course of the blood. The Microscope was needed; and the first to employ the microscope in such researches was Malpighi, who, four years after Harvey's death, in 1661, detected those Capillaries which form the channel of communication between arteries and veins. (See fig. 11, p. 293.) He says that, at first, he thought the blood poured out from the minute arteries in streams, without his detecting any vessels for these streams; but afterwards he detected the distinct walls of these vessels; and he describes the modes of examining them in the lung of the frog. Their network arrangement on the pulmonary cells is well described by him.* Nevertheless, in 1668, Leeuwenhoek describes them as if previously they had been quite unknown. "I used every means I could devise," he says, "to see the complete circulation of the blood—namely, that one

---

* MALPIGHI: *Epist. II. de Pulmonibus*, in *Opera Omnia*, ii. 327 of the 4to edition. From the *Opera Posthuma*, p. 9, it appears that the date of this discovery was 1661. I am indebted to Professor SHARPEY for this reference.

of the smallest of those vessels which we call veins, arose from another which is called an artery, and afterwards conveyed its contents to a larger vein; but I found this to be impossible, for when I followed the course of the artery until it became so small as only to admit of one or two globules to pass through it at a time, I then lost sight of it." This was in the wing of a bat; but subsequently he was more fortunate with the tail of a tadpole: "a sight presented itself more delightful than any that my eyes had ever beheld; for here I discovered more than fifty circulations of the blood in different places. I saw that not only the blood in many places was conveyed through exceedingly minute vessels, from the middle of the tail towards the edges, but that each of these vessels had a curve or turning, and carried the blood back towards the middle of the tail, in order to be conveyed to the heart. Hereby it appeared plainly to me that the blood-vessels I now saw in this animal, and which bear the names of arteries and veins, *are, in fact, one and the same*—that is to say, that they are properly termed arteries so long as they convey the blood to the farthest extremities of its vessels, and veins when they bring it back towards the heart." *

Thus, then, was the demonstration of the *course* of the blood completed; and we must confess that it is with surprise we find all historians overlooking the great gap in the doctrine which had been left by Harvey, a gap only filled up by Malpighi and Leeuwenhoek in their discovery that these capillaries formed the true passage of arterial into venous blood.

---

* LEEUWENHOEK: *Select Works*, i. 92.

It is necessary to bear in mind that the capillaries are a distinct set of vessels, differing from the arteries and veins which they connect, in their anatomical structure, and in their arrangement as a network (Fig. 8, p. 242). Bichat was the first who systematically conceived them as a distinct system ; but their structure was not known until investigated by Henle, in 1841, and by subsequent histologists. The existence of these vessels is not only important to the theory of the circulation, but is even more important to the theory of nutrition ; since it assures us that not only does the blood truly circulate, but circulates in a system of *closed* vessels, so that only by oozing through the walls of those vessels can it reach the tissues, and nourish them. Indeed, those who imagined that the blood was poured on to the tissues, were not aware that blood under such circumstances would act like a foreign substance : instead of nourishing, it would destroy.

If the reader feels any difficulty in understanding how the blood can ooze through the walls of the vessels in sufficient quantities for the purposes of nutrition, he is referred to the explanation previously given of Endosmosis (see p. 105). Having there learned with what facility the passage takes place when the membrane separates two fluids of unequal density, he will be prepared to understand how it may take place even in the Slug, which has a continuous layer of *chalk* imbedded in the walls of its blood-vessels—a fact which considerably surprised me when I first observed it. In other animals the walls of the blood-vessels are more delicate.

8. *Discovery of the Lymphatics.*—Harvey did not live to hear of the Capillaries; but there is another system of vessels, of which indeed he heard, although he failed to appreciate their significance. We allude to the Lymphatics, or Absorbents. They are minute vessels, abounding in all the viscera, rare in the muscles, and not yet detected in the nervous centres; when they rise from the alimentary canal, they are called *Lacteals*, or chyliferous vessels, and as such were, until quite recently, supposed to be the chief channels for the conveyance of the chyle to the blood. But in the chapter on DIGESTION we learned that this chyle is not the quintessence of the digested food, and that the lacteals only differ from all other lymphatics in carrying a larger proportion of fat, which gives their fluid its milky aspect during digestion. Lymph is blood without its cells. Chyle is lymph with the addition of fat, and perhaps also the addition of some other products of digestion. Lymph contains albumen, fibrine,* fat, salts, and extractive matters, like the blood: and, like the blood, it coagulates at certain temperatures. The chief differences between lymph and blood are the absence of the cells, and the presence of a greater

---

* VIRCHOW considers the fibrine of the Lymph to be different from that of the Blood. It is certainly less easily coagulated. We may, perhaps, assume it to be an early form of blood-fibrine.

proportion of water. By absorbing its water, lymph leaves the plasma bathing the tissues in a more concentrated state. Lymph is one of the streams which set towards the heart, and join the venous current.

The discovery of the Lymphatics is due to Aselli, Pecquet, Rudbeck, and Bartholin. Anatomists taught that there were three kinds of vessels in the body: the veins, which carried blood; the arteries, which carried spirituous blood; and the nerves, which carried "animal spirits." To the surprise of all, the news came that an Italian anatomist, Aselli, had discovered a fourth kind, which carried the chyle. This discovery was announced in 1622—three years after Harvey first announced *his* discovery, but six years before his book was published.

Aselli was dissecting a dog, and, to his surprise, on opening the abdomen he saw a network of delicate white vessels. What could they be? Did they contain the chyle? He pricked one, and, in a transport of delight, exclaimed *Eureka!* as he saw a milky fluid flowing out. But on opening another dog, he was greatly discouraged, for there was not a vessel of the kind to be seen. Had he been deceived? Was his joy premature? In this perplexity it occurred to him that the first dog had been fed a little while before it was opened; whereas the second dog was fasting. With the insight of genius, Aselli detected here the clue which might lead him to the truth. He fed another dog; four hours afterwards he opened it, and had the intense satisfaction of once more seeing the milky vessels.

But although Aselli made Europe aware of a new system of vessels, which he named Lacteals, he failed to trace their issue. He thought they conducted the chyle to the liver.

In 1648, a French anatomist, Pecquet, distrusting the conclusions of that "mute and frigid science," as he calls the dissection of dead bodies, determined to seek the truth in the living organism, and began a series of vivisections. He was rewarded by the discovery of the course taken by the chyle in the lacteals, as it passes into the Reservoir, still named after him, and along the Thoracic Duct, to be poured into the subclavian vein, and thence, mingled with the blood, into the heart.

In 1650, Rudbeck, a young Swede, discovered the lymphatics in the liver, and their connection with the reservoir of the chyle.

In 1652, Bartholin, another Frenchman, completed this discovery by finding the lymphatics in the viscera and limbs, and by tracing them into a common trunk.

These discoveries, following in such quick succession, greatly disturbed the equanimity of the Faculty, whose members swore by Galen, and could not tolerate the idea of the ancients being supposed to have overlooked anything. "Un chacun invente à present!—Everybody must needs be a discoverer nowadays," was the indignant sarcasm of Riolan, the most renowned teacher of that day—the only adversary whom Harvey condescended to answer, and of whom he stood in such awe, that, even when answering his attacks, he declares the book which contains those attacks "will live for ever, and when marble shall have mouldered, will proclaim to posterity the glory which belongs to your name."*

Harvey knew indeed of the existence of the lacteals, and says that he had observed them before Aselli published his book. But he denied that they contained the chyle, and he is twitted by Riolan for his disbelief. In consequence of this, a tradition has come down, that Harvey showed the same spirit of opposition towards the novelties of others, as was shown by others towards the novelties he advocated; but this is a mistake. If the reader will turn to Harvey's letter to Dr Morison of Paris, he will see a very circumstantial and temperate exposition of the objections which Harvey felt against the notion of the lacteals conveying the chyle: many of these objections are of great force, and still remain unshaken. His chief error is in supposing that the fluid in the lacteals is milk. But he insists very properly that it should be demonstrated that this fluid is really chyle brought from the intestines, and that it supplies nourishment to the whole body; "for unless we are agreed upon this point, all discussion is vain." Subsequent investigations have cleared up what was obscure, and have shown that the lacteals convey lymph mingled with fat, and that the lymphatics convey whatever they can absorb from the tissues, in which they are imbedded like the roots of a tree in the earth. The lacteal and lymphatic streams are confluent with the great venous streams, and thus form parts of the circulation.

---

* *Exercit. de Circul. ad Riolanum*, 1649; last paragraph.

It may now be convenient to state in a few brief sentences the results of all these discoveries, and the course taken by the circulation. For this purpose, let us cast a glance at the disposition of the organs represented in figure 15, on the following page.

The Food having become chyle in the intestines, is from them carried to the heart by these two courses :

1°. The system of *portal veins* (*Vp*) which conduct the chief mass to the liver ; from the liver, the chyle, now become Blood, passes along by the inferior vena cava to the right auricle of the heart (*h*).

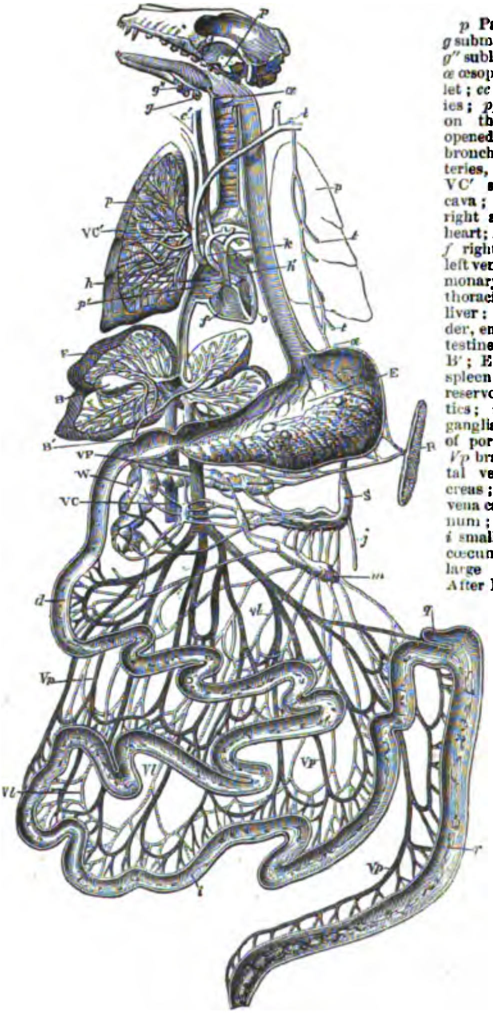
2°. The system of *Lacteals* (*Vl*), which conduct the fluid through the mass of mesenteric ganglia, *m* (not to be confounded with nervous ganglia), to the Reservoir of Pecquet (*S*) along the thoracic duct (*t*, *t*), whence it is poured into the subclavian vein, and there mingling with the venous blood, it is afterwards carried to the heart and through the lungs to be arterialised.

Thus the Food reaches the heart as Blood. From the heart it takes the course already described.

The arteries carry the blood to all parts of the body by a system of subdividing branches, the twigs of which are Capillaries. Through the walls of these capillaries some of the elements of the blood pass to nourish the tissues.

The Capillaries are continuations of the arteries, and the veins are continuations of the capillaries (or more correctly speaking, the capillaries are intermediate

Fig. 15.



p Parotid gland; *g* submaxillarygland; *g''* sublingual gland; *œ* œsophagus or gullet; *cc* carotid arteries; *pp* lungs, that on the left being opened to show the bronchial tubes, arteries, and veins; VC' superior vena cava; *k* aorta; *h* right auricle of the heart; *h'* left auricle; *f* right ventricle; *o* left ventricle; *p'* pulmonary artery; *tt* thoracic duct; F liver; B gall bladder, entering the intestine by the duct B'; E stomach; R spleen; S Pecquet's reservoir; *j* lymphatics; *m* mesenteric ganglia; VP trunk of portal vein; *Vp Vp* branches of portal vein; W pancreas; VC inferior vena cava; *d* duodenum; *Vl* lacteals; *i* small intestine; *q* cœcum; *r* colon, or large intestine. — After Bernard.

THE DIGESTIVE TRACT.

vessels uniting these two). Into the veins that portion of the blood enters which has lost its arterial character in the passage through the capillaries. Into the veins also the Lymphatics pour their fluid, which is partly the unused plasma of the blood absorbed from the tissues, and partly the products of waste in the tissues. When the Lymphatics arise from the intestines they are called *Lacteals*, or chyliferous vessels, and carry some portions of the digested food, as well as lymph. This compound of altered blood, unused plasma, wasted tissue, and food, forms venous blood, and is carried by the innumerable veins, ramifying all over the body, to two great venous trunks, the *venæ cavæ*, inferior and superior (*Vc* and *Vc'*), which pour the whole mass into the right auricle of the heart.

9. *Harvey's Claims.*—From the preceding exposition of the march of discoveries, it is clear that Harvey did not discover all that *we* mean by the circulation of the blood; but he discovered the great fact, that the blood does circulate, propelled from the heart along the arteries, and back to the heart and lungs through the veins.

Having thus shown that he did not discover the whole *course* of the circulation, we have now to show that he did not discover its whole *cause*. Before doing so, it will be interesting to remark, that in one important respect he was behind Servetus, for he thought that the blood returned to the heart, as to a fountain, there to receive the additions necessary to its perfection — *tanquam ad fontem sive ad lares corporis,* *perfectionis recuperandæ causa reverti.* Servetus correctly stated that it was in the lungs, and not in the heart, that this perfection was attained. Instead of perceiving that arterial blood acquired its bright colour by its exposure to the air, Harvey maintained that this colour was owing to the lighter part being "strained" from the heavier in its passage through minute openings. "In blood-letting, when the blood forcibly escapes to a distance in a full stream, it is thicker and darker; but when flowing from a small orifice drop by drop, it is brighter because it is then strained, and the thinner portion alone escapes." We know that the difference is owing to a more perfect exposure of the whole mass to the air when it issues slowly in drops, and *that* is why the blood is then of a brighter hue. Harvey, having no suspicion of this atmospheric influence, is driven to ingenious devices to explain the change of colour. "In the lungs," he says, "it is more florid than in the arteries, because it is strained through the pulmonary tissue." His notion of the office of the lungs was, that they served to *cool* the blood, "and prevent it from boiling up."

II°. CAUSE OF THE CIRCULATION.—What is it which causes the blood to circulate? "The heart," answers an unhesitating reader. That the heart pumps blood incessantly into the arteries, and that this pumping must drive the stream onwards with great force, there is no doubt; but although the most powerful agent in the circulation, the heart is not the sole agent; and the more we study this difficult question, the more our doubts gather round the explanation.

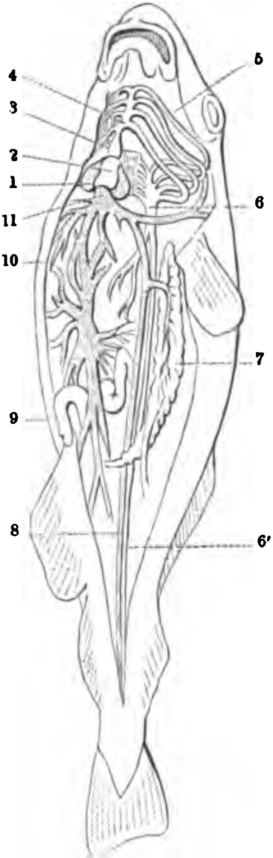
1. *Circulation in the absence of a Heart.*—Let a few of the difficulties be stated. There have been cases of men and animals born without a heart; these "acardiac monsters" did not live, indeed could not live; but they had grown and developed in the womb, and consequently their blood must have circulated. In most of these cases there has been a twin embryo, which was perfect; and the circulation in both was formerly attributed to the heart of the one; but it has been fully established that this is not the case. Further, Dr Carpenter reminds us that "it has occasionally been noticed that a degeneration in the structure of the heart has taken place, during life, to such an extent that scarcely any muscular tissue could at last be detected in it, but without any such interruption to the circulation as must have been anticipated if this organ furnishes the sole impelling force."* On the other hand, an influence acting on the capillaries will give a complete check to the action of the heart although that organ is itself perfectly healthy and vigorous.

Professor Draper conceives that circulation in the liver is peculiar, since a complete circuit is there performed by the blood, yet no heart, or pulsating organ, is found in that circle; the small vessels which belong to the intestine converge into a large trunk, and this portal vein on entering the liver ramifies over it in the manner of an artery—that is to say, it branches and branches into the minutest subdivisions, and terminates in capillaries; these once more converge into venous trunks, and carry the altered blood from the liver. Here is a complete circle, analogous to that larger circle which traverses the whole body. But this circulation is dependent on the primary impulsion given by the heart; and Prof. Draper would have to show that without the heart the circuit would still be performed, before he could claim the liver as an illustration of his views. He also refers to the circulation in the fish.

---

* *Human Physiology,* p. 249.

Fig. 16.



CIRCULATION IN THE FISH.

1. Auricle; 2. Ventricle; 3. Arterial bulb; 4. Artery of the gills; 5. Vessels of the gills; 6 6′. Aorta or dorsal artery; 7. Kidney; 8. Vena cava; 9. Intestine; 10. Portal veins; 11. Large venous sinus.

The fish has a heart, but that heart only drives the blood into the gills; it does not suffice for the rest of the body—it is a pulmonary, not a systemic heart. This will be understood by reference to the following figure, which represents the circulation in a fish.

The blood, propelled from the auricle into the ventricle, is aerated in the capillaries of the gills, and descends through them into the aorta, *not* into the heart; and from the aorta it passes to the rest of the body, from whence it returns through veins to the heart.

Thus we perceive that in the fish, although the blood performs a complete circuit, it only once enters the heart; whereas in the mammal it twice enters the heart, going to and returning from the lungs. The following *theoretic* diagram (copied from Milne-Edwards like the preceding) may assist the reader in forming a distinct conception of the fish circulation.

This diagram should be compared with that at p. 293, in order to detect the important differences between the circulation in the fish and the mammal.

Fig. 17.

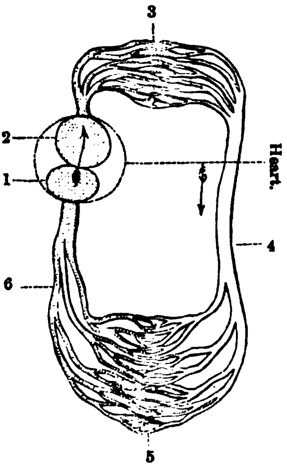


DIAGRAM OF CIRCULATION IN THE FISH.

1 Auricle; 2. Ventricle; 3. Vessels of the gills passing into arteries; 4. Arterial system passing at 5 and 6 into the venous.

It is possible to meet these difficulties, but the objections to the Harveyan doctrine do not end here. The heart may be removed in cold-blooded animals, and the capillary circulation will continue for some time, in spite of that removal. This has been observed more than once; and although I had myself observed it some time ago, yet, in preparing these pages for publication, I again investigated the point; and for this purpose removed the heart of a Triton, with as much care as possible, and found the circulation going on in the tail for some minutes afterwards; nor did it entirely cease on separating the tail from the body.*

While the fact was thus indubitable, I had many doubts as to the cause. But the fact is enough for our present purpose; and that it is also true of warm-blooded animals may be inferred, since after death various processes of secretion, and some even of *growth* (as of hair, beard, &c.), are known to take place; and this seems to imply capillary circulation. "After most kinds of death," says Dr Carpenter, "the arterial system is found, subsequently to the lapse of a few hours, almost or completely emptied of blood; this is partly, no doubt, the effect of the tonic contraction of the tubes themselves; but the emptying is commonly more complete than could thus be accounted for, and must therefore be partly due to the continuance of the capillary circulation. It has been observed by Dr Bennet Dowler, that in the bodies of individuals who have died from yellow fever, the external veins frequently become so distended with blood, *within a few minutes* after the cessation of the heart's action, that when they are opened the blood flows in a good stream, being sometimes projected to the distance of a foot or more. It is not conceivable that the slowly-acting tonicity of the arteries could have produced such a result as this; which can scarcely therefore be attributed to anything else than the sustenance of the capillary circulation by forces generated within itself."

---

* A curious fact connected with this investigation may here find a place. Having some doubts as to the cause of the continued circulation, I proceeded to repeat the experiment with another Triton; but no sooner was the thorax opened than I was seized with a sudden giddiness and faintness, which caused me instantly to throw open the window, and breathe energetically for some minutes; and, of course, to give over the experiment. I should have fainted away, had not the remembrance of a passage in Mr BUCKLAND'S *Curiosities of Natural History* warned me of the danger. He describes a similar effect as having arisen during his dissection of a Triton recently dead. Another note-worthy circumstance is, that the effect is by no means constant. I have dissected scores of Tritons, but this was the only occasion on which any effect was perceptible.

2. *Rapidity of the Circulation.*—Those who have never watched with attention the circulation in the capillaries, may perhaps imagine that the mere force of the heart which propels the blood into the gills, will suffice to propel it also through the general circuit. They will see the heart of the fish beating vigorously, and will imagine these pulsations suffice. But this will no longer seem so plausible, if we place the tail of a tadpole, or the foot of a frog, under the microscope, and delight ourselves with the wondrous spectacle. We shall immediately perceive that the blood flows with far greater rapidity in the arteries than in the capillaries; we shall note that, although the heart continues its vigorous pulsations, and the blood in the arteries advances in regular leaps, the currents in the capillaries are very irregular, sometimes momently arrested, and even reversed. Instead of the *leaping rush,* we perceive an *unsteady flow,*

which is never at any time equal in rapidity to the flow in the arteries.

This slackening of the capillary current is held to be an important point in the present discussion. Although precise estimates are excessively difficult in such cases, we need accept the estimates yet made as only approaching the truth, and the argument is equally fortified by them, for the difference in the rate is enormous: in the main arteries the blood rushes at the rate of a *foot* per *second;* in the capillaries only an *inch* per *minute.*[*]

What does that indicate? It indicates that, when the heart has driven the blood along the arteries at a rapid rate, there is a considerable *retardation* occurring in the capillaries, which must call for some new force to *restore* the rate.[†]

Not further to multiply examples, we may take our stand on these, and pronounce the Harveyan doctrine to be incomplete, since it fails altogether to account for many important phenomena. Whatever influence the action of the heart may exert, it is not the sole cause of the circulation, but only *one* of the causes. But before attempting to assign the other causes, let us see the part actually assignable to the heart.

---

[*] This is Valentin and Weber's estimate. Draper makes the rate in the capillaries an inch in three minutes, which would imply that the arterial current was 2160 times more rapid than the capillary current.

[†] The force of this argument is destroyed by the remark of a friend of mine, that the slackening of the capillary current may be explained on the principle of Hydraulics, that "if in any part of a closed current the sectional area of the current is increased, the velocity is diminished." Where a river widens, its velocity slackens, and is recovered again where it narrows. The total sectional area of the capillaries being much greater than that of the arteries supplying them, there is no need of any other cause to explain the retardation and recovery of the velocity. This objection destroys the force of the argument derived from the retardation in the capillaries, but it does not touch the other arguments.

3. *The Motions of the Heart.*—The motions of the heart consist in the alternate contractions and relaxations of its muscular walls. The process is this: The two antechambers (*auricles*) suddenly contract; immediately afterwards, *but while the auricles are still contracted*, the two chambers (*ventricles*) also contract, having been powerfully expanded by the rush of blood from the auricles. This contraction is named the *systole* of the heart. It continues for a moment, and is followed by a relaxation of the two auricles, which is immediately succeeded by the relaxation of the two ventricles. This relaxation is named the *diastole.*[*]

During each beat, two sounds may be heard; one dull, which may be imitated by pronouncing the word *lubb;* the other, quickly succeeding it, has a sharper sound, like *dup.* The former sound is supposed to be due to the contraction of the muscular fibres of the ventricles, and the thump of the heart against the chest; aided, no doubt, by the rush of blood and the closing of the valves. The latter sound is caused by the shutting of the semilunar valves of the aorta and pulmonary artery.

---

[*] Although Harvey describes the process correctly, Haller, and, until quite recently, all succeeding writers, described the contraction of the auricles as coincident with the relaxation of the ventricles. The movement is really wave-like, and the contraction passes on from one cavity to another before the relaxation supervenes.

4. *The Pulse.*—The number of pulsations varies greatly, not only between different sexes and different individuals, but at different ages and conditions of the same person. The following table shows the average at several epochs:—

	Beats per Minute.
In the fœtus in utero, . .	150 to 150
Newly-born infant, . .	130 „ 140
During the 1st year, . .	115 „ 130
During the 2d year, . .	100 „ 115
During the 3d year, . .	95 „ 105
From the 7th to 14th year, .	80 „ 90
From the 14th to 21st year, .	75 „ 85
From 21st to 60th year, .	70 „ 75
Old age, ...	75 „ 80

These figures open a wide field for speculation, especially when coupled with the differences noticed between the sexes, the female having greatly the superiority over the male in respect of frequency, her pulse beating from ten to fourteen times a minute beyond that of man. It must be remembered, however, that a quick pulse and a strong pulse are very different things. The rate of the pulse varies at different periods of the day, gradually diminishing from morning to night, and notably declining during sleep. It is quickened during exercise and digestion. It is slower when we lie down than when we sit, slower when we sit than when we stand.*

---

* A valuable collection of details respecting the frequency of the pulse will be found in MILNE-EDWARDS: *Leçons,* iv. 56 *et seq.;* and BERNARD: *Liquides de l'Organisme,* i. 221.

5. *Cause of the Pulse.*—What is it causes the beating of the heart? Haller and his school attributed it to the irritability of the muscular walls, which are stimulated by the presence of the blood. There is this fact in favour of such an hypothesis, namely, that *after* the heart has ceased to beat, and its irritability is extinct, a little arterial blood injected into it will cause it instantly to resume its pulsations. This, however, is met by another fact, that the heart continues to beat long after it is empty of all blood.

Nor is the common statement correct, that the heart *retains* its irritability longer than any other muscle; I have found the tail and lower extremities of a Triton preserving their irritability, and, indeed, almost all their vital properties, several hours after the heart had ceased to beat: and Budge found the amputated leg of a Frog retain its irritability as long as the heart.*

Not, therefore, by the possession of any greater irritability is the heart distinguished from other organs, but by the possession of a power of spontaneous contraction, such as they do not manifest. Other muscles will contract if some stimulus be applied, but they remain quiet so long as they are undisturbed. The heart does not remain quiet. Remove it from the body, and you will see its rhythmic pulsations continuing almost as if it were within a living breast. Cut it lengthwise into two halves, and each half will continue beating.* Cut it across, through both auricles and ventricles, and both sections will beat as before.

---

* DONDERS: *Physiologie der Menschen*, i. 49.

This is one of the spectacles that assail the mind of the anatomist with somewhat of a tremulous awe. The beating of the heart, which from his childhood he has learned to associate in some mysterious manner with life and emotion, he here sees occurring under circumstances removed from all possible suggestions of emotion or life. What mean those throbbings? They are not the equable movements of Life; they are not the agitations of terror; they are not the impulses of instinct. Dead and destroyed is the wondrous mechanism of which this heart but lately formed the mainspring; and yet, beside the inert body lies this beating organ, as if in the expiring agonies of struggle.

Why is this? For many years no explanation could be given. We now know that in the substance of the heart there is a complete little nervous system, consisting of ganglia and nerves.† This system is not made up of the nerve-filaments which come from the *pneumogastric* nerve. It is a system belonging to the heart. Ganglia are to be found at the base of the auricles and ventricles. The *auricular septum* of the frog's heart is the best place in which to study these ganglia, because its transparence permits microscopic investigation, without any preparation, which might alter the natural disposition of the parts.[*] In mammals the ganglia may be found on the surface of the auricles; and according to Remak, who may be considered the discoverer of this ganglionic system,[†] they are also on the surface of the ventricles. Todd and Bowman,[‡] however, declare they could not find them in the ventricle of the calf. I was equally unable to find them in that of a pig, a mole, a mouse, a cat, and a kitten; and Eckhard[§] saw fibres, but no ganglia, in the septum of the tortoise. It is probable, therefore, that there are considerable variations in the distribution of these ganglia; but the fact of their existence is enough for us.

---

* Harvey was acquainted with this fact.

† In Chapter VIII. the reader will learn what is meant by ganglia; for the present it is enough to say they are the nerve-*centres*.

From these ganglia nerves are distributed through the muscular substance. That it is to this nervous apparatus we are to ascribe the spontaneous activity of the heart is easily proved; for if any part be severed from all connection with the ganglia, the pulsations cease at once in that part; but if any part be severed which still retains a ganglion, the pulsations will continue. The movements during life or death are thus seen to be due to the ganglia.

---

[*] Few, I think, who examine a ganglion thus, can resist the conclusion that the fibres pass beside and round the cells, but are *not* all in direct connection with them. I never saw any but "apolar cells" in these ganglia; and three very eminent anatomists have assured me that they also have failed to detect a single cell in direct connection with a fibre. If these negatives do not affect the positive statement of those who say they have seen such cells in connection with fibres, at least they establish the fact that this connection is by no means *necessary*. For my own part, attentive study of the nervous tissue in every class of the animal kingdom, has convinced me that the theoretical conception of cells being necessarily in direct connection with fibres is untenable.

[†] REMAK in *Müller's Archiv*, 1844.—Compare also LEE: *On the Ganglia and Nerves of the Heart*, Philos. Trans., 1849.

[‡] TODD and BOWMAN: *Physiological Anatomy*, ii. 342 (where the two previous references are given).

[§] ECKHARD: *Beiträge zur Anat. u. Physiol.*, p. 151.

But why these ganglia retain their power after the circulation has been destroyed, and why a similar power is not observable in other ganglia, still remains a problem. It seems certain that the power is only retained during the continuance of those molecular changes which we vaguely name vital; for if the heart be subjected to the influence of foreign gases, or be dipped in oil, its pulsations suddenly cease: on the contrary, if arterial blood be injected long after the cessation of all movement (provided decomposition has not commenced), the contractions are resumed.* It has been observed to beat in an exhausted air-pump; which excludes the idea of the atmosphere being the stimulus that sets it going.

The heart pulsates in the embryo long before it contains blood, and long before any nerves have been developed in it—when, indeed, it is nothing but a mass of cells. Nor have we any evidence of the existence of nerves in those pulsatile sacs which constitute the hearts of the simpler animals: not only is there no evidence of such a structure, but all the evidence is decidedly against our supposing that these pulsatile sacs derive their contractions from nervous influence.

---

* Harvey says that one day, after the heart of a pigeon had ceased to beat, he placed his finger on it wetted with saliva, and in a short time, under the influence of this "fermentation," as he calls it, the heart recovered its vigour, and both auricles and ventricles pulsated.

Here the reader probably thinks he sees a flagrant contradiction in our statements. We first do our utmost to show that the heart contracts only under nervous agency, and we then quietly assert that the embryonic heart, no less than the heart of various animals, is under no such agency whatever, yet it pulsates with a vigour not to be gainsaid.

But the contradiction is only apparent. The student of Physiology must expect to meet with such at every stage of inquiry. Extending his investigations into the vast field of animal life, he will gradually learn that Contractility is one of the vital properties of tissue, which may be excited by *various* stimuli. We happen to know with tolerable certainty, that, in the heart of the complex animal, the stimulus acts through the agency of a nervous system; in the embryonic heart of that animal, or in the permanent heart of simpler animals, we do not know the agency by which the stimulus is conveyed, nor do we know what the stimulus is.

Before quitting this beating heart, we may remark, that while on the one hand the pulsations are not in themselves evidence of life, on the other hand their cessation is no evidence of death, but only one among the many signs of death.

When death follows on a long or painful illness, the irritability of the heart vanishes almost with the vanishing breath; but if the decease be sudden, the heart will continue beating for some time afterwards. Harless observed it beating in the body of a decapitated murderer one hour after the execution. Margo found the right auricle beating two hours and a half after the execution, although not a trace of irritability could be detected in the other parts of the heart. Dietrich, Gerlach, and Herz, found that both ventricles contracted, if one were irritated, forty minutes after death.* Remak observed the rhythmic contractions in the hearts of birds and mammals two days after death; and Em. Rousseau mentions that a woman's heart had these rhythmic movements seven-and-twenty hours after she had been guillotined.

It is not always, indeed, that the pulsations cease even when the death has been gradual. Vesalius had a terrible experience of it. That great anatomist, who had nobly braved so much odium because he would not, as his predecessors had done, content himself with the dissection of animals, but suffered his scalpel to traverse the complexities of the human frame, one day opened the body of a young nobleman, whose medical attendant he had been, to ascertain, if possible, the cause of his death. Imagine the horror which ran through all present at the sight of the heart still equably beating! Vesalius was accused of having dissected a live man; nor was the accusation unreasonable in those days. He had to appear before the Inquisition, and narrowly he escaped with his life. A pilgrimage to the Holy Land was his punishment; but he never outlived the scandal created by this unfortunate occurrence.

---

* DONDERS: *Physiologie*, i. 49.

6. *Action of the Arteries.*—Having made ourselves acquainted with the action of the heart, let us now inquire into its influence on the circulation. Every time the blood is pumped into the arteries a pressure is exerted, the force of which is estimated at *thirteen pounds.* This pressure, being on a column of liquid, it will, by the known laws of hydrostatics, not only drive that liquid onwards, but will also cause a great *lateral* pressure, and thus distend the arterial tubes. These tubes are eminently elastic, owing to the elastic tissue of their outer walls. They are also eminently contractile, owing to the muscular tissue of their inner walls. The elasticity is a physical property, and continues after death. The contractility is a vital property, and vanishes with the disappearance of the molecular changes of Nutrition.

Although the arteries are elastic and contractile throughout their whole length, they are so in varying degrees: the elasticity decreases, and the contractility increases, as the vessels become smaller in calibre. What follows? Why, that when fresh blood is impetuously poured into them from the heart, it dilates them; and no sooner is the pressure taken off by the reopening of the ventricles, than muscular contraction once more restores the arteries to their former size, and in so doing forces the column of blood onwards.

The heart's influence is thus decomposed into two portions: one, which is of momentary duration, lasting no longer than the contraction of the heart; another, which is occupied in expanding the artery. This second action is not lost, because the contraction of the artery gives it back to the blood. At each injection of blood there is a pulsation. The distension does not occur at the same instant in all the arteries; those nearest the heart yield first, and those more distant a little later. There is, consequently, a *wave of distension* passing along the whole length of the vessel, and another *wave of motion* in the blood itself. The interval of wave-motion from the heart to the wrist is only one-seventh of a second.

7. *Action of the Capillaries.*—The sudden push and continuous pressure which the column of blood thus receives, suffice to carry it with great velocity to the network of capillaries, which, be it observed, are elastic, but *not* contractile. In them a very noticeable change occurs. Instead of the *rushing, leaping movement,* which characterises the flow in the arteries, we observe *an equable current* of much less velocity: it no longer jets like a spring, it wanders like a canal. The absence of contractility in the capillaries prevents their assisting in driving the blood onwards; and when we reflect that the *capillary area* traversed by the blood is *five hundred times greater than the arterial area,* and further reflect that, after traversing the capillaries, the blood has to pass through the veins to the heart, we shall understand why it has been held

that some new force originates in the capillary area capable of effecting such a movement.*

What is that new force which comes into play when the force of the heart is nearly spent? A perfectly satisfactory answer to this question cannot, perhaps, be given in the present state of science; but we will give the hypothesis recently revived by Professor Draper.† He grounds it on a well-known physical law, namely, that if two fluids communicate in a capillary tube, which have *different* degrees of affinity for the walls of that tube, the fluid having the highest affinity for the tube will drive the other fluid before it. The two fluids in the blood-vessels are arterial and venous, and the greater affinity of arterial blood for the tissues causes it to drive the venous blood onwards.

Professor Draper commences by applying this principle to the circulation of the sap in the cells of plants.

"The motions of the sap in plants are clearly dependent on this principle. Leaving out of consideration the minor movements which take place for special purposes, or at specific epochs in the development, it may be truly said that the *nutritive changes occurring in the leaf are the primary cause of the motion;* for as the ascending sap presents itself on the sky-face of the leaf, it receives carbon under the influence of the sunlight from the air, and becomes converted into a gummy glutinous liquid. And just as in the pores of a bladder, or in those of any pervious mineral, pure water will drive out gum water, and occupy the pore, so will the ascending sap expel the gummy solution from the capillary tubes or intercellular spaces of the leaf. As fast as this takes place the active liquid becomes inactive, by itself changing into a gummy solution, and the movement is perpetuated. And this ensues not only in the leaf, but in every part of the plant; *the liquid to be changed presses upon that which has changed, and forces it onwards.*"

---

* Unless we adopt the suggestion offered in the note to p. 328.

† DRAPER: *Human Physiology,* 1856, p. 142 *et seq.* The idea was casually thrown out by MÜLLER, and elaborately argued by ALISON: *Outlines of Phys.,* 3d ed. p. 62 *et seq.* It is restated by DRAPER with great ingenuity.

The motion of the blood depends on the same principle. The arterial blood, charged with oxygen, passes to all parts of the body in search of organic particles, for which it has affinity. No sooner is this affinity satisfied, than the blood becomes venous, and is pressed onwards by the eager column behind. "In my view of this subject, it is therefore the arterialisation of the blood in the lungs which is the cause of the circulation. I consider the circulation as the consequence of respiration; and though, in one sense, the minor causes are numerous, each portion of nervous material, each muscular fibre, every secreting cell, working its own way—these subordinate actions are all referable to one primordial act, and that is, the exposure of the blood to the air." Professor Draper then refers to the fact that whatever interferes with respiration, interferes with circulation. If an unbreathable gas is thrown into the cells of the lungs, the passage of the blood is instantly arrested, and suffocation occurs. If the access of air be prevented, as in drowning, in vain will the heart throb convulsively—the blood is not driven forward.

Professor Draper's hypothesis, then, is briefly this: the arterial blood has an affinity for the tissues, which causes it to press forwards in the capillaries; and no sooner is that affinity satisfied, than the blood becomes venous, and is pressed forward by the advancing column. In the lungs, venous blood presses forward to satisfy its affinity for the oxygen which is in the air. Having satisfied this, and become arterial, it is pressed on by the advancing column.

If the reader will station himself at the door of a theatre, and watch the column of eager playgoers struggling to get to the money-takers, he will see an image of the forces of the circulation. Each visitor is anxious to put down his half-crown in exchange for a ticket. No sooner has he satisfied that "affinity," than he finds himself pressed forward by the man behind him, still in a state of unsatisfied affinity; and so the rush continues. An image is not an explanation, but it may render an hypothesis more intelligible; and having attempted to make Professor Draper's hypothesis intelligible, we will add, by way of criticism, that it has one serious defect. It rests on the notion of chemical affinity, yet chemical affinity acts only at *insensible* distances, and here the distances are *sensible*. That liquids should circulate in a tube, when one of them has a greater affinity than the other for the walls of that tube, is not evidence that the liquids will circulate in a tube in virtue of an affinity supposed to exist between one of them and the tissues *outside* the tube, because these tissues, being at sensible distances, cannot exert their affinity. If instead of "affinity" we substitute "leakage"—if we remember that the action of endosmose is necessarily set up between the blood and the tissue-plasma, the hypothesis may be more acceptable.

Spallanzani, in his celebrated *Mémoires sur la Respiration*, relates that, when snails were confined in vessels, and had absorbed all the oxygen from the contained air, the movement of their lungs ceased, and with it ceased all movement of the heart—the circulation was arrested. He had only to remove the top of their shells, which could be effected without injury, and the phenomenon was easily watched. By keeping a snail thus confined, at a temperature gradually diminishing, the gradual diminution of the respiration and circulation became very evident. When the temperature fell to zero, the heart ceased to beat altogether, and the blood was stagnant in the veins. In this state of suspended animation the animal was kept for several hours; but on raising the temperature, the lungs began once more to inflate, the heart to beat, the blood to circulate; and, as in the palace of the Sleeping Beauty, all was vivid activity where a minute before all was the image of death.

> "The hedge broke in, the banner blew,
> The butler drank, the steward scrawl'd,
> The fire shot up, the martin flew,
> The parrot scream'd, the peacock squall'd,

The maid and page renew'd their strife,
The palace bang'd, and buzz'd, and clackt,
And all the long-pent stream of life
Dash'd downward in a cataract."[*]

The same effect of torpor was produced by the absence of oxygen and the absence of heat. Spallanzani placed a snail in a vessel containing mephitic gas. In eleven minutes the heart was still, and remained so during five hours. On reintroducing atmospheric air the lungs once more began to move, and life returned. To prove that it was the oxygen of the air, and nothing else, which caused this reanimation, Spallanzani repeated the experiment, substituting nitrogen gas for atmospheric air, as the replacer of the mephitic gas; but no movement was visible. Thus it appeared that the animal ceased to breathe because it had ceased to absorb oxygen. It ceased to absorb oxygen under two conditions—when there was none present in the air, and when the temperature was too low—the absorption of oxygen being always in a direct ratio to the temperature; and under both conditions the cessation of the absorption of oxygen was followed by the arrest of the circulation.

Viewing the Circulation in connection with Respiration, we see many arguments favourable to Professor Draper's hypothesis; but that there are some difficulties not easily reconcilable with that hypothesis, cannot be denied. For the present, however, it is enough to have mooted the question, and to have touched on some of the difficulties in the way of our accepting the heart as the sole agent in propelling the blood. "The relation between the interspaces of the capillaries, and the blood thus introduced to them, continues the current. The oxidising arterial blood has a high affinity for those portions that have become wasted; it effects their disintegration, and then its affinity is lost. The various tissues require repair; they have an affinity for one or other of the constituents of the blood; they take the material they need, and their affinity is satisfied; or secreting cells originate a drain upon the blood, and the moment they have removed from it the substance to be secreted, they have no longer any relation with it. So processes of oxidation, of nutrition and secretion, all conspire to draw the current onwards from the arteries, and push it towards the veins."*

---

[*] TENNYSON.

We have now brought to a close our survey of the course and cause of the Circulation, and assigned to each labourer in this difficult field of research his share in the work. As an episode in the History of Science, the discovery of the circulation will always command the interest of readers; and if the foregoing sketch has had the good fortune to secure the attention of any medical readers, we hope it may have the further effect of inducing them to go carefully through the immortal works of WILLIAM HARVEY.†

---

* DRAPER, p. 145.

† HARVEY'S works have been ably translated by Dr R. WILLIS, and published in one volume by the Sydenham Society.

Several times in the course of our exposition we have been forced to allude to the passage of the blood through the lungs, and the changes there impressed on it. Indeed, without such changes blood could serve no purpose in the organism. Respiration is the mainspring of animal existence. In the next chapter we shall attentively examine it.

CHAPTER VI.

RESPIRATION AND SUFFOCATION.

Two suicides—Suffocation of seventy-two persons on board the "Londonderry"—History of our knowledge of respiration—The air we breathe—Distinction between respiration as an Animal Function and as a Property of Tissue—Oxygen the life-giver—Varnished eggs will not develop; tail of a tadpole developing after separation from the body—The breathing-mechanism in various animals—The process of breathing—Tight-lacing—Alterations of the air in respiration—Necessity of ventilation—German taverns—How the organism accustoms itself to bad air—Effect of bad air in depressing the vital functions—Respiration a vital, not a physical, problem—Suffocation: various forms of—Carbonic acid not a poison—Oxide of carbon a poison: its effects on the blood—Deaths by suffocation: escape of gas: drowning—Respiration not a process of oxidation—The balance of nature—Trees not beneficial in streets—Varieties of animal respiration—Respiration during sleep—Effect of temperature on respiration—Why do we breathe?

A FEW years ago a young Frenchman, named Déal, finding his hopes of making a figure in the world were daily becoming more chimerical, resolved to die; and that he might not quit the world without producing some "sensation," he left this written account of his dying moments:—"I have thought it useful, in the interest of science," he wrote, "to make known the effects of charcoal upon man. I place a lamp, a candle, and a watch on my table, and commence the ceremony. It is a quarter past 10; I have just lighted the stove; the charcoal burns feebly.

"Twenty minutes past 10: the pulse is calm, and beats at its usual rate.

"Thirty minutes past 10: a thick vapour gradually fills the room; the candle is nearly extinguished; I begin to feel a violent headache; my eyes fill with tears; I feel a general sense of discomfort; the pulse is agitated.

"Forty minutes past 10: my candle has gone out; the lamp still burns; the veins at my temples throb as if they would burst; I feel very sleepy; I suffer horribly in the stomach; my pulse is at 80°.

"Fifty minutes past 10: I am almost stifled; strange ideas assail me. . . I can scarcely breathe. . . I shall not go far. . . There are symptoms of madness. . .

"Sixty minutes past 10: I can scarcely write. . . My sight is troubled. . . My lamp is going out. . . I did not think it would be such agony to die. . . 10. . ." Here followed some quite illegible characters. Life had ebbed. On the following morning he was found on the floor, a corpse.

A few hours later, she whom he loved, and who loves him, hears of this rash act, which annihilates even hope. In her despair she flings herself into the dark and sullen Seine. The next morning a corpse is exposed at the dreadful Morgue. The casual spectator gazes on it with undefinable awe, as he thinks of the stillness of that wondrous organism, which but a few hours before was so buoyant with life. Where is all that mystery now? The body is there, the form is there, the wondrous structure is there, but where is its activity? Gone are the graceful movements of those limbs, and the tender sweetness of those eyes; gone the rosy glow of youth; gone the music of the voice, and the gaiety of the heart. The mystery of Life has given place to the mystery of Death.

What has thus suddenly arrested the wondrous mechanism, and, in the place of two palpitating, vigorous beings, left two silent corpses? The cause seems so trifling that we can only marvel at its importance, when revealed in the effect; it was the same in both cases, in spite of the difference of the means: that which killed the one, killed the other; the fumes from the charcoal pan, and the rushing waters of the Seine, interrupted the exchange of a small quantity of gases, and by preventing the blood from getting rid of its carbonic acid, in exchange for an equivalent of oxygen, the fervid wheels of life were suddenly arrested.

To get rid of its carbonic acid for an equivalent of oxygen—that is to say, to make a slight exchange of gases—seems a very trifling process; and only the impressive lessons of tragic experience can persuade men that this process is extremely important. Every child knows that we must have air to breathe; every one knows how unpleasant it is not to have *fresh* air to breathe; but the mass of mankind have no conception that air which is *not* fresh, is as bad as poison.

A very painful illustration of this ignorance is afforded by the calamity which occurred on board the "Londonderry," a steamer plying between Liverpool and Sligo. On Friday, 2d December 1848, she left for Liverpool, with two hundred passengers on board, mostly emigrants. Stormy weather came on, and the captain ordered every one to go below. The cabin for the steerage passengers was only 18 feet long, 11 feet wide, and 7 feet high. Into this small space the passengers were crowded; they would only have suffered inconvenience, if the hatches had been left open; but the captain ordered these to be closed, and—for some reason not explained—he ordered a tarpaulin to be thrown over the entrance to the cabin, and fastened down. The wretched passengers were now condemned to breathe over and over again the same air. This soon became intolerable. Then occurred a horrible scene of frenzy and violence, amid the groans of the expiring and the curses of the more robust: this was stopped only by one of the men contriving to force his way on deck, and to alarm the mate, who was called to a fearful spectacle: seventy-two were already dead, and many were dying; their bodies were convulsed, the blood starting from their eyes, nostrils, and ears.

The cause of this tragedy was owing to the ignorance of the captain and his mate. They had never learned the vital importance of fresh air. They had never been taught, that air which has once been breathed, cannot be breathed over again without injury; never been taught the fact, that air which has once passed to and fro in the lungs is vitiated, and that vitiated air is as bad as poison.

It is the same cause, acting with milder force, which makes the faces pale of those who issue from a crowded church, and gives a languor to those who have sat for some hours in a theatre, concert-room, or any other ill-ventilated apartment, in which human beings have been exhaling carbonic acid from their lungs. A breath of fresh air quickly restores them, and after breathing this fresh air, during a walk home, they scarcely feel any evil results of the late partial suffocation. Had the young man's door been burst open, and fresh air admitted to his room, or had the girl been rescued from the river, and made to breathe within a few minutes after her plunge, or had the hatches been broken open in time, all these victims would have been finally restored, as our concert-goers are restored; and the concert-goers, if kept much longer in that ill-ventilated room, would have perished, as the others perished.

Among the earliest experiences of mankind must have been the necessity of fresh air for the continuance of life; but the complete explanation of the fact, in all its details, is a scientific problem, the solution of which only began to be possible when Priestley discovered the gases of which the air is composed, and the relation these bear to the organism; nor is the problem even now entirely solved, in spite of the labours of so many illustrious men. We have learned much, and learned it accurately; but the difficulties which still baffle us are many and considerable. The ancients really knew nothing of this subject; nor did the men of the sixteenth and seventeenth centuries lay any solid foundation-stone. *That* was laid by Priestley, when he discovered the oxygen contained in atmospheric air to possess the property of converting venous into arterial blood. Lavoisier carried out this discovery, and founded the chemical theory of Respiration. Goodwyn (1788) applied the new views to Asphyxia (Suffocation), showing, by a series of experiments, that when air was excluded, venous blood remained unchanged; and when it remained unchanged, death inevitably followed. Bichat concluded from a number of striking experiments, that an intimate relation existed between Respiration, Circulation, and Nervous Action; he showed how the access of venous blood to the brain stopped its action, and subsequently stopped the action of the heart.* Legallois extended these observations to the spinal chord. But by far the most brilliant investigations on the subject of Respiration are those of Spallanzani, whose *Mémoires* still deserve a careful study, both as models of scientific research, and as storehouses of valuable facts. He was succeeded by W. F. Edwards, whose *Influence des Agens Physiques sur la Vie* (which may be found on the old book-stalls for a couple of shillings) still remains one of the best books the science can boast of. During the present century, hundreds of physiologists have devoted labour to the elucidation of the various difficulties which darken this subject, and a vast accession of valuable facts has been the result. The chief points which have been cleared up we may now endeavour to exhibit.*

---

* Claude Bernard has recently shown that venous blood is *not* poisonous, although incapable of sustaining life; and that Bichat's experiments consequently must be otherwise interpreted.—BERNARD, *Liquides de l'Organisme*, i. 507-18.

1°. *The Air we breathe.*—In Respiration there are two objects to be considered, namely, the Air which is breathed, and the Breathing-mechanism. This air, which no man can *see*, is nevertheless a very material substance. We stuff cushions with it; and the chemists analyse it into well-known substances. It forms an atmospheric ocean forty-five miles in depth, surrounding our planet, and whirling with it, as it whirls around the sun. It is an ocean subject to incessant fluctuations, or tides, as we may call them. Like the other ocean it carries a variety of substances in its restless currents, but preserves a constant composition of its own, not affected by these substances. It is chiefly composed of two gases—Oxygen, which forms about one-fifth, and Nitrogen, which forms nearly the remaining four-fifths; that is to say, there are twenty-one parts of Oxygen to seventy-nine parts of Nitrogen. But besides these, there is always a *fraction* of carbonic acid gas even in the purest atmospheric air: at ordinary elevations there are about two parts of carbonic acid in 5000 parts of air; so that we may reckon this gas as forming the $\frac{1}{2500}$ of the atmosphere. There are also traces of ammonia, but they are very slight.

---

* In the following works will be found most of the facts cited or alluded to in our exposition:—SPALLANZANI: *Mémoires sur la Respiration;* EDWARDS: *De l'Influence des Agens physiques sur la Vie;* CLAUDE BERNARD: *Leçons sur les Effets des Substances toxiques;* MILNE EDWARDS: *Leçons sur la Phys. et l'Anat. Comp.;* LEHMANN: *Physiologische Chemie;* DR JOHN REID's article *Respiration* in the *Cyclopædia of Anat. and Phys.;* and the Treatises on Physiology of BÉRARD, MÜLLER, VALENTIN, LONGET, FUNKE, and DRAPER, already cited.

The air of inhabited rooms, or of caves and dungeons, although constantly *tending* towards the standard of composition, is, of course, subject to great variations. The respiration of animals and plants, and the decay of organic matters, incessantly alter the composition of the air in these places, and unless these alterations be quickly counteracted by free access of fresh air, the result is an atmosphere *injurious* or *unbreathable*. The air is vitiated by its proportion of oxygen being lessened, and its carbonic acid increased. It may be vitiated by other causes, such as the presence of injurious gases and effluvia, but the chief cause is the one just named.

When we breathe, and burn candles, or lamps, in a room, the breathing and the burning have a similar bad effect on the air, robbing it of oxygen, and loading it with carbonic acid; there is neither breathing, nor burning of candles, without these effects. When Dalton analysed the air of a room in which, during two hours, fifty candles had been burning, and five hundred persons breathing, he found that instead of the proportion of carbonic acid being only two gallons in five thousand of air (which would have been the proportion in the air of the street), it was not less than one gallon in every hundred! This air would soon have become unbreathable. Leblanc analysed the atmosphere of three hospitals in Paris, and found that it contained five, ten, and twelve times as much carbonic acid as the air of the streets.

2°. *The Breathing-mechanism.*—Before describing the organs by which we breathe, it will be necessary first to define what is meant by breathing. It will perhaps strike some readers as an idle question, to ask What is Respiration? And yet until a distinct answer to that question is given, there can be no philosophical comprehension of the present subject; and as the purpose of these pages is to furnish philosophical students with material for reflection, no less than to furnish explanations of vital processes, we must endeavour to answer this question.

Reduced to its simplest terms, Respiration appears to be nothing more than the interchange of carbonic acid and oxygen, which takes place between the blood and the atmosphere. Our analysis comes down to this; and yet we shall be led into error if we accept this as the true answer to our question: for this is the *physical fact* implied in the process, it is not the *vital function* itself. This physical fact is not only manifested by the breathing apparatus, but by every tissue in the body, even when it is separated from the body. Take a fragment of muscle, rid it of its blood, and leave it exposed to the atmosphere; it will absorb oxygen, and give out carbonic acid. But no one would say that the muscle breathed; because breathing is an animal function, not a mere interchange of gases: a function dependent upon this interchange, but dependent *also* on the organic apparatus which performs it. We are thus led to distinguish between Respiration as the *Function of an apparatus of organs*, and that interchange of gases which is merely a Property of the tissues.

In the higher animals we see this Function performed by two different organs—gills and lungs. In both organs we find that a large quantity of blood is exposed to the air by means of a network of vessels spread over the surface. The blood arrives there black, and passes away scarlet. It has exchanged some of its carbonic acid for some of the oxygen of the air; it has become changed from venous into arterial blood. This *oxygenation of the blood* is therefore the special office of Respiration; and although all animals exhale carbonic acid and absorb oxygen—although every tissue does so—yet we must rigorously limit the idea of Respiration, as an animal Function, to that which takes place in the gills or lungs. True it is, that the simpler animals effect such exhalation and absorption by their *general surface*, and not by any *special modification of it*—such as gills or lungs; true it is, that even fish and reptiles, furnished with gills, also respire by their skin; and that, when the lungs of a frog are removed, the necessary oxygenation of the blood may be effected through the skin, if the temperature be low; nay, it is also true, that even man himself, in a slight degree, *respires by the skin;* so that the student tracing upwards the gradual complication of the organic apparatus, and finding, *first,* the whole of the general surface effecting the aëration needed; *secondly,* a part of the surface formed into a gill, in which aëration is far more active; and, *finally,* finding this gill replaced by a lung, may be tempted to say, "If the aëration of the blood is the office of Respiration, and if this is effected in some animals by the skin alone, in others by the skin and the gills, and in others principally by the lungs, but still in a slight degree also by the skin, how can you pretend to establish a distinction, other than a simple distinction of degree; how can you expect me to lay much stress on a verbal difference such as that between Function, and general Property of tissue?"

In reply to this plausible objection, we must observe that in science verbal distinctions are often extremely important; they keep attention alive to real, though subtle, distinctions. It is difficult to keep to such distinctions, for, as Bacon says, "words are generally framed and applied according to the conception of the vulgar, and draw lines of separation according to such differences as the vulgar can follow: and when a more acute intellect, or a more diligent observation, tries to introduce a better distinction, words rebel." In strict physiological language, no animal without blood ought to be said to *breathe;* for Respiration in such animals is not effected by a special apparatus of breathing organs; and in physiology *the idea of Function is inseparably connected with that of Organ,* as the Act is with its Agent. Professor Bérard says that, penetrated with the idea of a special organ being necessary for Respiration, he experienced a singular disappointment in reading the experiments of Spallanzani, which proved that every tissue of the body absorbed oxygen, and gave out carbonic acid; and he "only recovered his contentment on perceiving that the essence of Respiration consisted in this interchange of gases, so that, wherever a nutritive fluid was in contact with the atmosphere, Respiration must take place." Here the professor seems to us to have made an oversight, confounding the general with the particular, as completely as if a savage visiting England, and observing the transport of men and goods by railways, and "penetrated with the idea of a special method of transit being necessary," were afterwards to observe that vans, carts, and wheelbarrows also conveyed goods, from which he would conclude that the essence of transport being the removal of goods from one place to another, every means of transport must be a railway. The interchange of gases, like the transport of goods, may be effected by various means, but we only call the one Respiration, when it is effected by gills or lungs; and the other Railway transit, when it is effected by Railways. Professor Bérard was right in conceiving that a special organ was necessary for Respiration; and his error arose from confounding the *action of the organ* with the *result of that action.* Respiration effects the interchange of gases, and the aëration of the blood, by means of a peculiar organic apparatus, without which the due aëration would not take place in the higher animals. In the simpler animals this apparatus is not needed, because the nutritive fluid, being easily accessible, requires no apparatus to bring it into contact with the air; but no sooner does the organism become so complex that a direct aëration of the nutritive fluid ceases to be possible, than an apparatus is constructed, the function of which is to effect this aëration. In the gills and lungs we see such an apparatus.

Unless distinctions like these are established, Respiration ceases altogether to be a vital process; and every interchange of carbonic acid and oxygen, no matter where effected, will have an equal claim to be designated as a respiratory act. Therefore, as it is of the first importance in all physiological inquiries to keep constantly in view the part played by the organism in modifying physical laws, the philosophic reader will see at once that any verbal distinction which aids us in this must be of advantage.

If now we ask, What is Respiration? the answer will be this: *The function of the lungs or gills, by means of which the blood absorbs oxygen, and parts with carbonic acid and some other noxious elements.* Oxygen is the great inciter of vital changes; its presence is the indispensable condition of life. It is at once fuel and flame: it feeds, and it destroys: constantly withdrawn from the blood by the ceaseless activities of vital change, it is as constantly drawn into the blood by the process of Respiration. If the blood rushing through our lungs does not meet there with a supply of oxygen, the torrent carries to the tissues venous in lieu of arterial blood, and the consequence is an arrest of all the vital changes. If in passing through the lungs the blood only meets with a small supply of oxygen, an imperfectly arterialised fluid is carried to the tissues, and a partial arrest takes place, which is seen in the diminished vigour of the organism: all the functions are depressed; and if this depression continue, death arrives.

That oxygen does play this life-giving part, is familiar to all readers; but I will here cite two illustrations for the sake of their intrinsic interest. The chick inside its shell develops only when the air can penetrate the pores of that shell. If a varnish be rubbed over the surface of the shell, development is arrested; and although it appears that even varnish does not utterly prevent the penetration of gases, yet it so considerably diminishes it that the chick cannot develop. It is noticeable, moreover, that the precise point at which the development ceases, is that of the establishment of the first circulating system, when the vascular area begins to form. If one of these varnished eggs be freed from its varnish, and once more placed under the hen, it will develop.* Here we observe the absence of oxygen causing a complete arrest of vital changes. In the following case we shall see the reverse, namely, vital changes going on under free access of oxygen, with no other stimulus that can be detected. The case is so remarkable that I have thought it right to verify it by repeating the experiments several times.

---

* See DARESTE: *Comptes Rendus de la Société de Biologie,* 1857, p. 101-120.

Spallanzani, and after him numerous physiologists, found, that if the tail of a tadpole were cut off, it grew again rapidly. M. Vulpian thought of inquiring what became of the tail thus cut off? and he found that not only did it live many days, but it exhibited activity and development: it wriggled when touched, or when exposed to the air, and it developed new parts, as well as new forms.* I cannot say that I was fortunate enough to observe all the curious facts recorded by M. Vulpian; but respecting the main fact, namely, that the cells daily pass from their rudimentary state into that of actual muscle fibres, vessels, and pigment cells—in a word, that a regular process of development takes place—I can confirm his statement. These tails separated from the organism are without food, and the means of procuring food; yet the free access of oxygen is sufficient for a time to keep up the process of vital change.

To absorb oxygen is an animal necessity. The Blood, which is the great agent of Nutrition, must constantly be supplied with oxygen from the air.

It matters not whether the animal lives in air or in water—the real respiratory medium is always the air—

---

* See BROWN SÉQUARD'S *Journal de la Physiologie*, i. 803.

for water, deprived of its air, or of its due proportion of oxygen, is as fatal to aquatic as to terrestrial animals. It matters not by what organ or surface the respiratory exchange takes place, it is always a twofold act of exhalation of carbonic acid on the one hand, and of absorption of oxygen on the other.

The variety of respiratory organs is great. In the Molluscs we find some kinds having no "organs" at all; some kinds having gills, others having lungs, and one kind (*Oncidium*) having both gills and lungs. In the Crustacea we find rudimentary gills. In spiders there are both gills and lungs. Fish have only gills. Frogs, toads, tritons, and salamanders begin life with gills, which disappear and give place to lungs. Reptiles, birds, and mammals, have lungs of different degrees of complexity.

Animals are said to breathe by their skin when the air, either in the water or as atmospheric air, comes in contact with the moist skin in which the blood is circulating.

Gill-Respiration is effected in a similar way: the water, rushing over the delicate surface, parts with oxygen, and takes up carbonic acid.

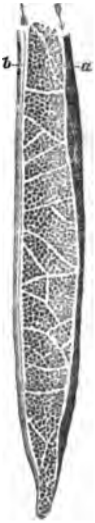
In *Lung-Respiration* the air is no longer outside, but inside the organ: it is drawn in from the atmosphere; the exchange is effected *in* the organ, and the altered air is then driven out, to be replaced by fresh air.

To understand the mechanism of Pulmonary Respiration, let us commence with an examination of the

Water Newt (*Triton*), which presents us with the simplest form of the lung, and will therefore best enable us to understand the more complex forms. On opening the chest of this Newt, recently caught from a neighbouring pond, we observe two elongated air-sacs of thin membrane: these are the lungs.

Let us remove one of these sacs, and place it under the microscope. This is what we see: A delicate membrane, down one side of which we observe the pulmonary artery *a*, and running up the other side, the pulmonary vein *b*; between these trunks, we observe smaller branches and innumerable capillaries, the meshes of which are indicated by the white spots.

Fig. 18.

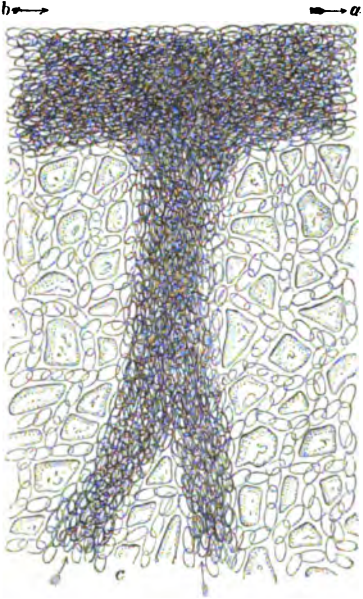


Lung of Triton (after Wagner).

After the air has entered the windpipe it passes into this sac, and there comes in contact with the delicate blood-vessels, through the walls of which the oxygen passes freely into the blood, and the carbonic acid passes out of the blood. How will the air, which is now in the sac, and which is vitiated by the presence of carbonic acid, and the loss of oxygen, be got rid of, to make way for fresh air? Simply by a contraction of the animal's abdominal muscles. If you watch a live Triton in a glass-jar of water, you will see this contraction, and the bubble of gas, which is expelled.

Let us now take a live Triton, and examine its lungs under a magnifying power of 150 diameters; we can only see a very small bit at a time, but that bit will show us the circulation in the capillary network—a wondrous spectacle, indeed.

Fig. 19.



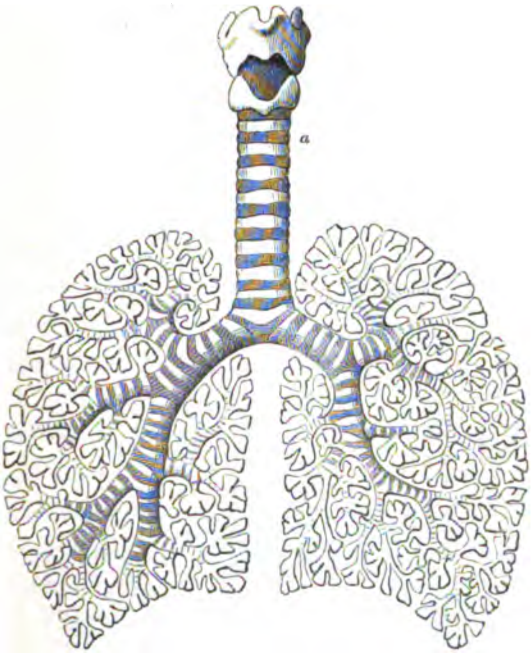
Circulation in Lungs of Triton (after Wagner).

The lung of a man is little more than a repetition on a large scale of this very scheme: a myriad of small air-sacs are crowded together in the cavity of his chest. The Trachea, or windpipe, divides and subdivides into a number of *bronchial tubes,* which, when inflamed, as in violent colds, are the seat of the disease so familiar to all—*bronchitis.* A glance at Fig. 20, on the opposite page, copied from Dr Dalton, will enable the reader to understand the relation of these tubes to the lungs.

The tubes are seen to end in little islets of lung-substance: these are the "pulmonary lobules." If we now apply the microscope, each lobule is seen to be a cluster of air-cells (Fig. 21), of which, it is calculated that the human lungs contain six hundred millions.

This, which is copied from Kölliker, is partly a real and partly a diagrammatic representation. It enables us to understand how the air passes through the bronchial tube into the air-cells, between which, and outside of which, run the capillaries. These vessels are densely crowded; as may be seen in Fig. 22, also from Kölliker, magnified sixty times.

Fig. 20.

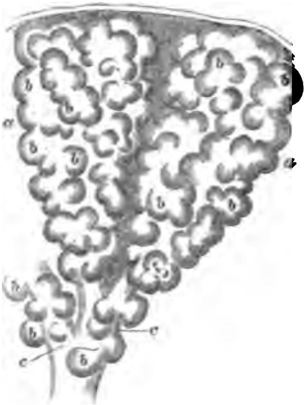


THE BRONCHIAL TUBES OF MAN.

a, Windpipe branching off into minute tubes.

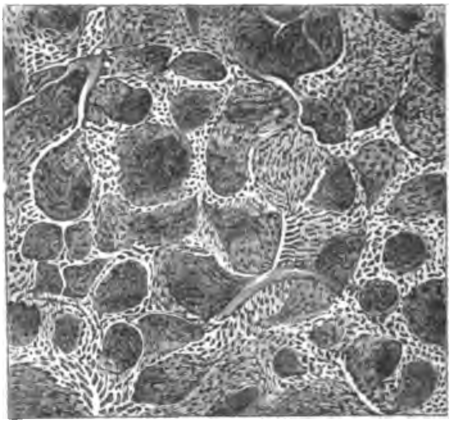
3°. *The Process of Breathing.*—Having thus ascertained the chief points respecting the air breathed and the breathing organ, we must now glance at the process. When we breathe, we draw in the air by our nostrils, which penetrates the trachea or windpipe, from thence passing into the bronchial tubes and tubelets, and from thence into the air-cells. Here it yields part of its oxygen to the blood, receiving carbonic acid in exchange. It was drawn in by a dilatation of the chest, and is driven out

Fig. 21.



A SINGLE BRONCHIAL TUBE WITH AIR-CELLS.

Fig. 22.



CAPILLARIES OF HUMAN LUNGS.

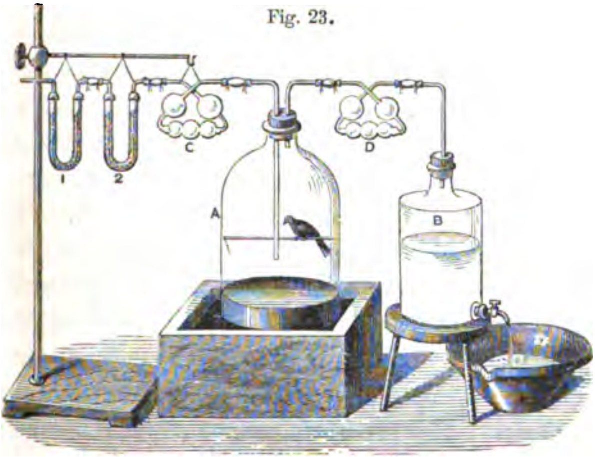
again by a contraction of the chest. Science has accurately measured the amount of air thus inspired and expired—namely, about 20 or 25 cubic inches each time. But we never *empty* our lungs by an expiration; there is always a much larger quantity of air remaining in the air-cells; this quantity varying, of course, with the force of the effort. Herbst found that, while 25 cubic inches was the quantity expelled in ordinary quiet breathing, the quantity would rise to 90, and even 240 cubic inches, by very energetic efforts. It is therefore calculated that an adult man, with a well-developed chest, will retain about 170 cubic inches of air in his lungs, after each expiration, during ordinary breathing; and as 25 inches will be added at the next inspiration, there will be alternately 175 and 200 cubic inches of air acting on the blood which rushes over the vast area of the lungs. The phrase "vast area" is no exaggeration; for small as the bulk of those organs truly is, the amount of *surface* on which blood is exposed to the air in them, has been calculated by Lindenau at not less than 2642 square feet. Is it not wonderful to reflect that, in the course of a single year, 100,000 cubic feet of air have been drawn in and expelled, by something like 9,000,000 of separate and complicated actions of breathing, to aërate more than 3500 tons of blood?

4°. *Tight-lacing.*—The injurious effect of tight-lacing has often been pointed out, and in England, at least, women have pretty generally learned to see the danger, if not always the hideousness, of those wasp-waists once so highly prized. A single fact elicited in the experiments of Herbst will probably have more weight than pages of eloquent exhortation. It is this: the same man who, when naked, was capable of inspiring 190 cubic inches at a breath, could only inspire 130 when dressed: now, if we compare the tightness of women's stays with the tightness of a man's dress, we shall easily form a conception of the serious obstacle to efficient breathing which stays must present; and the injurious effect of this insufficient breathing consists, as we shall see hereafter, in its inducing a *depression of all the vital functions.*

5°. *Alteration of the Air in Respiration.*—In Respiration we draw in and give out a *similar quantity* of air; but this air is by no means of *similar quality.* It has been altered—vitiated. The ancients had no other notion of Respiration than that it served to cool the blood, as the air cools the heated brow. This is not its office. It serves to supply the indispensable conditions of vital changes, by removing carbonic acid from the blood, and supplying its place with oxygen. The air which is expired differs from that which was inspired: it has lost much of its oxygen, and has gained from 3 to 6 per cent of carbonic acid, a large amount of vapour, traces of ammonia, hydrogen, and volatile organic substances. The latter being very putrefiable, gives a fetid taint to the moisture condensed from the air on the window-panes, and to the moisture which soaks into all porous substances in the room. It was pure breathable air when it entered, and is now so vitiated that after a few repetitions of the process it becomes unbreathable. It is made unbreathable by the carbonic acid.

It is not difficult to demonstrate the production of carbonic acid in Respiration. Fig. 23 is an apparatus which beautifully demonstrates it.

Fig. 23.



A bird is placed in the bell-glass A, which is reversed over mercury. A vessel of water, B, serves to establish a current of air as the water flows out, and this current of air entering by the tubes 1 2, which contain pumice-stone moistened with a solution of potass, abandons there its carbonic acid; and the proof that it does so, is seen in the fact that it passes through Liebig's apparatus C (in which there is lime water) *without* producing that milky appearance which would result if any carbonic acid were present, because this carbonic acid would unite with the lime, and form the carbonate of lime, or *chalk*. From C, therefore, the air enters the bell-glass entirely without carbonic acid. It is here breathed by the bird. The air which the bird expires passes from the bell-glass into the apparatus D, and there it meets with lime-water, which it renders milky by precipitating chalk ; and thus proves that carbonic acid has been given out by the bird.

If any one thinks that the carbonic acid may have had some other origin, he can convince himself by simply breathing through a glass-tube into a glass of lime-water: its instantaneous milkiness will assure him that there was carbonic acid in his breath. The quantity of this gas which is momently thrown into the atmosphere by each individual, varies according to sex, age, physical and mental condition, and according to the season of the year, and time of day. We are constantly exhaling carbonic acid, but not in constant quantities. Men exhale much more than women ; during the ages of from 16 to 40 the quantity exhaled by men nearly doubles that exhaled by women of the same ages. In men it is observed that the amount gradually increases from the age of 8 to that of 30, making a sudden start at the period of puberty. From the age of 30 it decreases gradually, till at extreme old age the amount is no greater than it was at 10. In women, a noticeable phenomenon is observed; the amount increases from infancy to puberty, just as in men; but at that epoch the increase suddenly ceases, and remains stationary till the change of life, when the amount increases. Besides such variations dependent on age and sex, there are others dependent on the muscular activity and physical condition of the individual. The amount of carbonic acid exhaled during digestion is greater than that exhaled during fast, and greater in sunlight than in darkness. Wines, spirits, tea, coffee, and narcotics lessen the amount; not, however, because they interfere with the process of Respiration, but because they cause less carbonic acid to be produced by the organism.

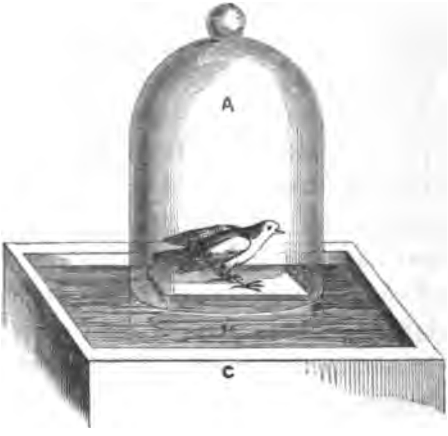
The exchange of these gases, considered simply as an exchange, is a physical fact resting on well-known physical laws. There have been, and there still are, disputes as to whether the gases are *free* in the blood, as in water, or are in a state of slight chemical combination; but the facility with which the exchange is made seems to be as great as if they were free. If blood be shaken in a vessel containing air, it will absorb from that air more than a tenth of its volume of oxygen. It is then *saturated;* and if now poured into a vessel containing carbonic acid, and there shaken, it will abandon almost all its oxygen, and absorb carbonic acid. This is a simple illustration of the interchange effected in the lungs and in the tissues; for, as previously indicated, the delicate walls of the blood-vessels oppose no obstacle to this interchange. It is only necessary that the blood should be brought in contact with an atmosphere, or a fluid, of a composition specifically different from its own. If we substitute hydrogen for oxygen, the animal confined in a vessel containing this gas will be found to exhale carbonic acid with the same facility as when atmospheric air is breathed. No animal can continue long to breathe hydrogen, simply because that gas does not furnish the conditions of vitality; but while the animal breathes in hydrogen, the exhalation of carbonic acid is as perfect as at any other time; thus showing that the exhalation depends on the difference in the nature of the gases in the atmosphere, and in the blood.

6°. *Necessity of Ventilation.*—When we breathe over and over again the same air, we gradually vitiate it by the constant exhalation of carbonic acid, which gradually brings the air up to the point where the difference between it and the blood—as regards the proportions of carbonic acid—disappears. The blood ceases to be arterialised, and the vital functions are arrested. In vain does the air still contain a quantity of life-giving oxygen; the blood cannot take it up, because it cannot get rid of the carbonic acid, and it cannot get rid of its carbonic acid because the conditions of the exchange are absent. To place an animal in air overcharged with carbonic acid, is equivalent to a gradual prevention of his breathing at all. Suffocation results from vitiation of the air in precisely the same manner as from interception of the air. Although burking and gagging are crimes which appal the public, that public seems almost indifferent to the milder form of the same murder when it is called "want of ventilation." In spite of the historical infamy of the Black Hole at Calcutta, our prisons, hospitals, theatres, churches, and other public buildings, were left disgracefully neglected, until, thanks to the energetic labours of our sanitary reformers, public attention was aroused. That thousands have been the victims of public ignorance on this important matter, may be shown by a single example. The deaths of new-born infants between the ages of 1 and 15 days, which in the Dublin Lying-in Hospital amounted in the course of four years to 2944 out of 7650 births, were suddenly reduced to only 279 deaths during the same period, after a new system of ventilation had been adopted. Thus more than 2500 deaths, or 1 in every 3 births, must be attributed to the bad ventilation.

In England the public is daily becoming more enlightened on the subject of ventilation, although a dangerous indifference, springing from want of elementary knowledge, is still prevalent, and taxes the patience of reformers; but in the country where these lines are written, it is painful to observe that even highly cultivated men seem almost insensible to the importance of fresh air. The Germans sit for hours in low crowded rooms, so dense with tobacco-smoke that on entering you cannot recognise your friends; and so vitiated is the atmosphere by the compound of breath, bad tobacco, exhalations of organic putrefiable matters, and an iron stove, that at first it seems impossible for you to breathe in it. Even in their private rooms they breathe a hot, musty, *dry* air, which makes an Englishman gasp for an open window. It is true that after a while you get accustomed to the air. You also get accustomed to that of the smoke-filled tavern. On entering, you felt it would be impossible to stay in it ten minutes; but in less than ten minutes it has become quite tolerable, and in half an hour scarcely appreciable. If you quit the room for a few minutes, and return once more after having breathed fresh air, again you perceive the poisonous condition of the atmosphere, but again you will get accustomed to it, and seem to breathe freely in it.

Was this atmosphere really not injurious? or have your sensations, like sentinels asleep, ceased to warn you of the danger? To answer this, we will first bring forward some experiments instituted by Claude Bernard on the influence of vitiated air (Fig. 24). A sparrow left in a bell-glass to breathe over and over again the same air, will live in it for upwards of three hours; but at the close of the second hour—when there is consequently still air of sufficient purity to permit *this* sparrow's breathing it for more than an

Fig. 24.



A, Bell-glass; M. Mercury; C, Trough.

hour longer—if a fresh and vigorous sparrow be introduced, it will expire almost immediately. The air which would suffice for the respiration of one sparrow suffocates another. Nay more, if the sparrow be taken from the glass at the close of the third hour, when very feeble, it may be restored to activity; and no sooner has it recovered sufficient vigour to fly about again, than, if once more introduced into the atmosphere from which it was taken, it will perish immediately. Another experiment points to a similar result. A sparrow is confined in a bell-glass, and at the end of about an hour and a half it is still active, although obviously suffering: a second sparrow is introduced; in about ten minutes the new-comer is dead, while the original occupant flies about the lecture-room as soon as liberated.

One cannot try experiments on human beings as on animals, but accident and disease frequently furnish us with experiments made to our hand. What has been just related of the birds, is confirmed by an accident which befell two young Frenchwomen. They were in a room heated by a coke-stove. One of them was suffocated, and fell senseless on the ground. The other, who was in bed, suffering from typhoid fever, resisted the poisonous influence of the atmosphere, so as to be able to scream till assistance came. They were both rescued, but the healthy girl, who had succumbed to the noxious air, was found to have a paralysis of the left arm, which lasted for more than six months. Here, as in the case of the sparrows, we find the paradoxical result to be, that the poisonous action of a vitiated air is better resisted by the feeble, sickly organism, than by the vigorous, healthy organism.

This paradox admits of a physiological explanation. In the vitiated air of a German *Kneipe*, as in that of the houses of the poor, we find those who have had time to adjust themselves to it, breathing without apparent inconvenience, although each new-comer feels the air to be vitiated; and because they "get accustomed to it," people very naturally suppose that no injurious effect can follow. Here lies the dangerous fallacy. They get accustomed to it, indeed, and only because they do so are they contented to remain in it; but at what price? by what means? By a gradual *depression of all the functions* of nutrition and secretion. In this depressed condition less oxygen is absorbed, and therefore less is needed in the atmosphere. A vitiated air will suffice for the respiration of a depressed organism, as it would amply suffice for the respiration of a cold-blooded animal. When we enter a vitiated atmosphere, our breathing becomes laborious; the consequence of this is a depression of all the organic functions, and *then* the breathing is easy again, because we no longer require so much oxygen, and we no longer produce so much carbonic acid. Were it not for this adjustment of the organism to the medium, by a gradual depression of the functions, continued existence in a vitiated atmosphere would be impossible; we see the vigorous bird perish instantaneously in air which would sustain the enfeebled bird for upwards of an hour.

Thus does Physiology explain the paradox; but at the same time it points out the fallacy of supposing that bad air can be harmless because we "get accustomed" to it. It is a fortunate circumstance for those who have to breathe bad air, that the organism is quickly depressed to such a point as to render the air breathable, yet no one will deny that depressions of this kind are necessarily injurious, especially when frequently experienced. There is indeed a wonderful elasticity in the organism, enabling it to adapt itself to changing conditions; but a frequent depression of functional activity must be injurious, and fatal if prolonged.

It is interesting to observe the effect of a *gradual* adjustment of the organism, as contrasted with one less gradual. The longer the time allowed, the easier is this adjustment. Thus a bird will live three hours in a certain quantity of air; in the same quantity, two birds of the same species, age, and size, will *not* live one hour and a half, as might be supposed, but only one hour and a quarter. Conversely, the bird which will live only one hour in a pint of air, will live three hours in two pints.

7°. *Respiration a vital Problem.*—Enlightened by these remarkable results, we shall now be able to regard Respiration as a physiological function rather than as a simple physical process. On more than one occasion we have had to protest against the tendency to explain vital phenomena by physical and chemical laws only, without regard to the order of conceptions specially belonging to vital phenomena; and we must repeat that protest in the present case. That Respiration is ultimately dependent on physical laws, no one thinks of disputing; and in the arduous endeavour to detect the operation of those laws, it is natural that men should neglect the still more difficult study of vital laws. But we think it can be shown that however far analysis may trace the operation of the laws of gaseous interchange and diffusion, and the condensing action of moist membranes, these will only conduct us to the threshold ; they will never open for us the temple. These physical laws reveal only one part of the mystery. Respiration is not a simple physical fact. It is the function of a living organism, and as such receives a specific character from that organism. No sooner do we cease to regard the exclusively physical aspect of this function—no sooner do we fix our attention on the organism and *its* influence, than the theory raised on the simple laws of gaseous interchange suddenly totters and falls.

It seems easy to explain why warm-blooded animals cease to breathe in an atmosphere charged with a certain per-centage of carbonic acid, although there may still remain sufficient oxygen to permit a candle to burn in it, and even to permit continued respiration if the carbonic acid be removed. The presence of a certain amount of carbonic acid in the air *prevents* the exhalation of carbonic acid from the blood. As we read the explanation, nothing can seem clearer, and we admire the skill with which the laws of the absorption of gases are brought to bear on the question. But as we pursue our researches, various difficulties arise; and as we extend the inquiry from the respiration of warm-blooded to that of cold-blooded animals, we learn that the fact is not at all true of the simpler organisms. Let us for a moment consider one striking contradiction: the air which has once passed through the lungs of a man, and which, in losing four or five per cent of its oxygen, has become charged with three or four per cent of carbonic acid, will yield but very little of its remaining oxygen when again passed through the lungs; and if this air be breathed over and over again, until the sense of suffocation forces a cessation, the air will still be found to contain ten per cent of oxygen—that is to say, nearly half its original quantity. In air thus vitiated the respiratory process is impossible, but *only impossible for warm-blooded animals in health:* frogs, reptiles, fish, and molluscs, instead of perishing when the air has lost about half its oxygen, continue to breathe, and to absorb oxygen, almost as long as there is any left. Spallanzani, Humboldt, and Matteucci, have placed this beyond a doubt by their experiments; and when we consider how long these experiments have been before the world, it is a matter of surprise that the contradiction they give to all the purely physical theories of Respiration has not been insisted on. If the process depends *simply* on the proportion of gases in the atmosphere, how is it that one animal can continue to breathe in an atmosphere unbreathable by another? If it be *simply* the interchange of oxygen and carbonic acid, and this interchange be frustrated whenever eleven per cent of oxygen has disappeared, the law must be *absolute*: it must be as applicable to reptiles and molluscs as to birds or mammals. Instead of this, we find that reptiles can continue to breathe long after such a limit has been passed; they continue to absorb oxygen as long as even only three per cent remains, in spite of the continually increasing proportion of carbonic acid. How is it that the physical laws of absorption frustrated the Respiration of one class of animals, and were powerless with another class? Why is it that, when a bird and a frog are confined in the same vessel, the frog will continue to absorb oxygen from the vitiated air in which the bird has long perished? Clearly the cause of this difference lies in the difference of the organisms; and we must no longer seek in the mere quantities of gases an explanation of interrupted respiration; we must no longer say that "breathing becomes impossible when the air is charged with a certain amount of carbonic acid, *because* that amount prevents the gaseous interchange;" but we must say that such an amount prevents the gaseous interchange, because it interferes with the organic action of the pulmonary apparatus. The distinction becomes palpable when we have an organism which is not affected by this amount of carbonic acid, and is even more palpable when we see a warm-blooded animal capable of breathing for a long period the air which, under a different condition, it would find unbreathable. We have seen how a bird, with its functions depressed, can continue to breathe for an hour in an atmosphere which immediately suffocated another bird of the same species; whereby it became clear that the lungs of one warm-blooded animal could absorb oxygen from an atmosphere in which there was such a proportion of carbonic acid, that sufficient oxygen could not be absorbed by a vigorous animal of the same species.

The intervention of organic conditions, modifying the simple physical laws of gaseous exchange, is sufficiently evident from what has just been said; but we have as yet no clear insight into the nature of this intervention; we do not know why blood, charged with carbonic acid, cannot in the one case exchange that gas for the oxygen, of which ten per cent still remains, since in another case the same blood *can* effect the exchange when there is even less than ten per cent of oxygen.

Atmospheric air contains only twenty-one per cent of oxygen. But if fifty per cent of oxygen be mixed with fifty of carbonic acid, a warm-blooded animal is suffocated in it, in spite of there being more than double the amount of oxygen present in the ordinary atmosphere. Bernard, who made the experiment, thinks that the carbonic acid in this mixture prevented the oxygen from entering the blood, not only because of its greater solubility, which gives it a tendency to displace the oxygen, but also because of the obstacle it presents to the exhalation of carbonic acid. On the other hand, the extensive and careful experiments of Regnault and

Reiset show that Respiration will take place quite well in an atmosphere which contains as much as twenty-three per cent of carbonic acid, if at the same time it contains as much as forty per cent of oxygen. How are we to reconcile such facts as those just cited? In the one case we see that fifty per cent of oxygen is insufficient, if the amount of carbonic acid be also fifty per cent; in another case we see that forty per cent of oxygen suffices, if the carbonic acid do not exceed twenty-three per cent; and we could explain both by saying, that unless the amount of oxygen nearly doubles that of carbonic acid, respiration is impossible, were it not for the irresistible objection that reptiles breathe in an atmosphere which has become charged with carbonic acid, and has gradually lost all but three per cent of its oxygen.

We have raised difficulties which we cannot pretend to remove. It is enough to have called attention to the physiological problem involved, as a justification of our scepticism in presence of the purely physical explanations. Respiration is not a simple interchange of gases, but an organic function, which chiefly consists in exhaling carbonic acid and absorbing oxygen: whatever interferes with the exhalation or the absorption, checks Respiration, no matter what may be the condition of the atmosphere. As a final proof of the correctness of this conception, we will add that oxide of carbon, by preventing the exhalation of carbonic acid from the blood, prevents all Respiration, whatever amount of oxygen may be in the air. Moreover, experimenters are now agreed that there is no accurate correspondence between the amounts of oxygen absorbed and carbonic acid exhaled, as there ought to be were the process one of simple exchange.

II. SUFFOCATION.—With this explanation of the process of Respiration, let us now approach the terrible phenomenon of Suffocation, or Asphyxia, which is the consequence of a continued interruption of the breathing process. This interruption may be owing either to the air being prevented from entering the lungs—as in drowning, hanging, gagging, &c.—or by the air which enters being too poor in oxygen, or too rich in carbonic acid, or in oxide of carbon. To tighten a cord round a man's neck, to hold his head under water, or to burn charcoal in his ill-ventilated room, are three modes of producing the same result. Every one can understand that if you forcibly prevent an animal from breathing, you must cause its death, because breathing is necessary to life; but every one does not understand that air which has been vitiated by the breathing of animals, or the burning of candles and gas, is kept out of the blood as certainly as if a pillow were pressed upon the face.

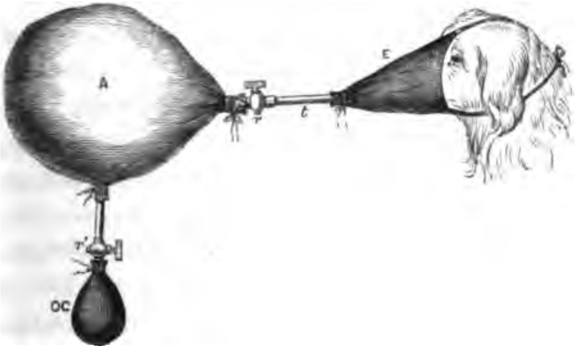
One very general, indeed almost universal, misconception on this subject is, that carbonic acid is poisonous in the blood; but the truth seems to be that the carbonic acid is noxious only when it prevents the access of oxygen. There is always carbonic acid in the blood, both venous and arterial. Its accumulation in the blood is only fatal when there is such an accumulation *in the atmosphere* as will prevent its exhalation; its mere presence in the blood seems to be quite harmless, even in large quantities, provided always that it be not retained there to the exclusion of oxygen. Carbonic acid, when absorbed into the blood, which is alkaline, cannot there exert its irritant action as an acid, because it will either be transformed into a carbonate or be dissolved. Bernard has injected large quantities into the veins and arteries, and under the skin of rabbits, and found no noxious effect ensue. The more carbonic acid there is in the blood, the more will be exhaled, provided always that the *air* be not already so charged with it as to prevent this exhalation.

This is a sufficient answer to the Teetotal argument, that Alcohol *must* be poisonous, because it is said to cause an accumulation of carbonic acid in the blood. This is not true, as a matter of fact; and if true, it would in no way affect the breathing process; the extra quantity of carbonic acid would be got rid of as easily as the usual quantity.

But *oxide of carbon* is a poisonous gas. Every reader will remember the *blue* flame which rises from lighted coals or wood; that is oxide of carbon; and being notoriously poisonous, it has by some writers been selected as the real agent in those numerous deaths occurring from voluntary and involuntary exposure to the fumes of charcoal in closed chambers. Carbonic acid was said to be innocent, and oxide of carbon had to bear the whole infamy. There is no doubt, however, that although carbonic acid is not a poison, it will produce asphyxia, and deaths from charcoal-fumes may occur either from this asphyxia, or from poisoning by oxide of carbon, or from a conjunction of the two. Oxide of carbon is truly called a poison, because its action is deleterious even in slight doses, no matter what may be the state of the atmosphere; but carbonic acid is only deleterious when the quantity in the atmosphere is such that the absorption of oxygen is frustrated.

Carbonic acid passes to and fro, leaving the blood otherwise unaltered; but oxide of carbon really kills the blood. If we take a little venous blood and expose it to oxide of carbon, it becomes instantly scarlet. In appearance the change has been nothing more than would have occurred had oxygen, instead of the oxide, been employed. But in fact, the change has been very considerable.

Fig. 25.



In Fig. 25 we see how Bernard causes a dog to breathe the oxide of carbon. At first the gas, which is in the bladder, is kept from the dog, and the blood flowing from an opened vein is seen to be black. But on turning the cock, and causing the oxide to mingle with the air breathed, the blood is rapidly seen to be scarlet; and unless the operation is suspended immediately, the dog will die suffocated. This scarlet blood will not become black again on exposure to carbonic acid, as would be the case if oxygen had given the scarlet colour. On the contrary, the scarlet colour is retained for weeks.

Prussic acid produces a similar effect. Poisoning by prussic acid, or by oxide of carbon, may be detected by this scarlet colour of the venous blood.

The effect of oxide of carbon is to render the blood-discs incapable of that process of exhalation, on which, as we have seen, the activity of the organism depends. The blood, to all appearance, preserves its vitality, for neither the form nor the colour of its discs is altered; but the blood is really dead, because its restless changes are arrested. Ever wonderful is the fact constantly obtruding itself upon us, that Life is inseparably linked with Change, and that every arrest is Death. Only through incessant destruction and reconstruction can vital phenomena emerge, an ebb and flow of being. The moment we preserve organic matter from destruction, we have rendered it incapable of the restless strivings of Life. A spirit like that of Faust seems ranging through all matter; and if ever it should say to the passing moment, "Stay! thou art fair," its career will be at an end.

Deaths from Suffocation.—We have seen that a current of fresh air prevents carbonic acid from being noxious, unless the quantity be very considerable; and it is on this account that coal, coke, candles, gas, &c. can be burned in our apartments without injury. But it is important to know that a current of fresh air will not always prevent death from suffocation. It is important to know that there is no absolute safety in the fact of the room being ventilated; the mere presence of carbonic acid will not cause suffocation in such a room, but unhappily the carbonic acid is frequently accompanied by oxide of carbon, and it is to *this* we must attribute those accidents which have occurred in manufactories, when the workmen, seated at tables under which coke was burning, have fallen back suffocated. Dr Marye cites a case of asphyxia which occurred in a room, one of the window-panes of which was broken, so that a current of air must have circulated in it. In 1835, a shopkeeper of Paris was found suffocated in his bed, although the room in which he slept was over the shop, communicating with it by an open staircase, and the shop had its windows, opening on the street, imperfectly closed. In spite of this free access of air, the noxious vapours from a stove in which coke and coal were burning, suffocated him.

Gas is a great spoiler of the air; but it has the merit of giving timely warning of the danger, by the horrible smell which accompanies its escape. This smell is perceptible when there is only one part in a thousand parts of air; becomes very offensive when the proportion is

$\frac{1}{750}$ or $\frac{1}{500}$, and is almost insupportable as the proportion increases. If the gas has escaped from a crack in the pipes, and been allowed to mingle with the air in which a free circulation by ventilation is impossible, so that the proportion of gas amounts to $\frac{1}{11}$, it explodes on the introduction of a candle. This was the cause of that terrible accident some twenty years ago in Albany Street, when a whole house was blown up, as if a powder-mill had been ignited. Gas, when burning, is also very injurious, and seems to have a particularly bad effect upon the nervous system, as all persons know who have attended to their sensations. It produces excitement, headache, lassitude, and often sickness. While ventilation is in all cases indispensable, it becomes more and more urgent in proportion to the amount of gas or coke which is burning in the room; and when we reflect on the compound of evils which meet together in a theatre, concert-room, or lecture-room—the air robbed of its oxygen by the burning and the breathing, and loaded with carbonic acid, animal effluvia, and heat, there can be little surprise if a violent headache, or unusual languor, is the result of staying three hours in such an atmosphere.

Death from drowning is also a form of suffocation. On sinking into the water, a man first struggles to the surface, and gasps for breath; but with the air there enters some water, and this makes him cough; in coughing to expel the water, he expels a great part of the air which he had drawn in. In a few seconds his strength fails; he can no longer rise to the surface, the urgent demand for fresh air forces him to open his mouth, and this only allows more water to enter, and asphyxia results. But this is not always the course of the phenomena. Sometimes the man receives such a shock that he is instantaneously deprived of the use of his faculties —he faints in the water, and dies without a struggle. Sir Benjamin Brodie tells a story of a man who was recovered from drowning, and who described it as being a very calm and blissful mode of death. The last thing he was conscious of was, that he quietly counted the pebbles on the bottom.

III. THEORIES OF RESPIRATION.—It is hoped that the foregoing pages have furnished the reader with many important hints for practical guidance, and have duly impressed him with the serious necessity of attending to ventilation in bedrooms, sitting-rooms, and manufactories; but there are also speculative points which might profitably detain us, after we have fully satisfied ourselves on the practical points. A few of these may now be touched on.

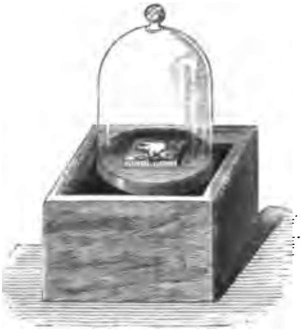
And, first, of that popular theory which declares Respiration to be a process of Oxidation. It probably caused the reader a pleasant feeling of surprise when he first heard that the burning of a candle, the rusting of iron, and the process of breathing, were only three forms of the same process—three names for three different forms of combustion or oxidation. There is, indeed, a fascination in such generalisations. One almost regrets to find them not correct. I am sorry to say this one is not correct. The burning of a candle and the rusting of iron are, indeed, two forms of one process of combustion. They are oxidations; but respiration can no longer be considered, in any sense, as a process of oxidation. The combustion-theory is a mistake from first to last. Respiration, as a process, is two-fold—the exhalation of carbonic acid, and the absorption of oxygen.

The interesting experiments of Priestley will enable us to set forth the differences between Respiration and Combustion. He placed mice in a bell-glass, where in due time they were suffocated by the air which they had vitiated; other mice were introduced, and they expired immediately. In another bell-glass a candle went out, after having in its combustion absorbed a part of the oxygen; another burning candle was introduced, and it was at once extinguished by this vitiated air. In both of these vessels some mint was now placed, where it flourished, and so completely revivified the air, by absorbing its carbonic acid and giving out oxygen, that mice could again breathe in the one, and a candle burn in the other. In these experiments we *seem* to have a demonstration of the identity of Combustion and Respiration—and this, indeed, was the conclusion drawn; but that the conclusion is erroneous, appears from the experiments of Claude Bernard, who takes a bell-glass containing an atmosphere of 15 per cent of oxygen, and 2 per cent of carbonic acid—the rest of the oxygen having disappeared to form water with the hydrogen of the candle which has just gone out. In this atmosphere, in which a candle will not burn, a linnet will breathe at ease for some time. Bernard then reverses the experiment, and makes an atmosphere in which a candle will burn, but in which an animal instantaneously perishes —an atmosphere composed half of oxygen and half of carbonic acid, in which a candle will burn *better* than in the air, because of the greater amount of oxygen; but in which the animal perishes, because, in spite of the amount of oxygen, that oxygen cannot be absorbed. The bird, when about to expire in vitiated air, will be recalled to life if the carbonic acid be removed by the introduction of potash—showing that it is owing to the presence of this carbonic acid that Respiration is impeded; but we cannot thus restore the expiring flame of the candle by removing the carbonic acid. Place lighted candles in two bell-glasses, and as soon as the combustion grows feeble, introduce into one glass some potash to remove the carbonic acid, you will, nevertheless, find that the candles in both glasses will go out at the same instant. The experiment is very simple, and its significance is plain. By it we see the difference between Combustion, which is only oxidation, and Respiration, which is not *oxidation* but *exchange*. In the combustion of the candle the oxidation is everything, and no process of exchange takes place. In the breathing of an animal the exchange is everything, and no oxidation takes place. The candle expires because there is not enough oxygen in the air; the animal expires because there is too much carbonic acid in the air.

Further, to prove that Respiration is an exchange of gases in the lungs, and not a process of oxidation, we need only refer to the experiments of Spallanzani and W. Edwards—experiments so celebrated, that one is amazed to find one's-self citing them in this discussion, which they ought long ago to have closed.

A frog was placed in a bell-glass containing pure hydrogen gas, reversed over mercury: Fig. 26. Before placing it there, all the air had been pressed out of its lungs. For eight hours and a half it continued to breathe this hydrogen, and continued to give out carbonic acid. The same experiment will succeed also with warm-blooded animals, if tried on those newly born; but after they have breathed for some hours they are unable to stand the test. These experiments prove that carbonic acid pre-exists in the blood, and is not formed during respiration by the oxygen as it enters; and prove, likewise, that the respiratory process is a double one of *exhalation and absorption,* which can take place as well with hydrogen as with oxygen: and we are thus forced to exclude the idea of oxidation altogether. Although respiration can take place without oxygen, life will not long continue without it; for, as before stated, oxygen is the power which burns organic matter into life.

Fig. 26.



There is another form of the combustion-theory, held by those who give up the idea of carbonic acid being formed in the lungs. It is this: "The oxygen of the air burns the carbon of the tissues into carbonic acid, and the hydrogen into water." Widely as this hypothesis has been accepted, there is not one bit of direct evidence that carbonic acid and water are ever formed in this way in the organism; it is purely inferential, and there are many reasons against our accepting it even hypothetically. But even on the supposition that such direct oxidations were the source of the carbonic acid and water, we should still have to declare that Respiration was not a process of Combustion. If oxidation of tissue is one *result* of Respiration, it is not the *process.*

Why is death inevitable when the access of fresh oxygen is excluded? The fact we know—of the reason we are ignorant. There still remains a large quantity of oxygen in the blood of the expiring animal; nor will death be sensibly retarded if fresh oxygen is injected into the veins and arteries. How is this? The process of respiration brings oxygen to the blood; yet, if the oxygen be brought there through a more direct channel while respiration is impeded, the animal will die as quickly as if left to itself. Bernard tied a dog's head in a bag, which would in a certain time produce suffocation, and he found that period by no means retarded when he injected oxygen into the arteries.

2°. *The Balance of Nature.*—Quitting for a moment this labyrinth of difficulty and doubt, which alternately fascinates and disheartens us when we strive to gain some explanation of the myriad processes of Life, let us stand apart and contemplate the marvel of respiratory interchange, no longer as an animal function, but rather as a planetary phenomenon; let us endeavour to picture to ourselves the silent creative activity everywhere dependent on this interchange. The forests, the prairies, the meadows, the cornfields, and gardens—the mighty expanse of plant-life covering mountain and valley—these subsist on the carbonic acid which is exhaled from the lungs and bodies of animals. Plants take up this carbonic acid from the atmosphere, mould the carbon into their own substance, and set free the oxygen, once more returning it to the atmosphere. Animals reverse the process, taking up the oxygen, and giving out carbonic acid for the nourishment of plants. This beautiful rhythmus of organic life has been so often described that it has almost become a commonplace, without, however, losing its charm for the contemplative mind.

The dependence of plant on animal, and of animal on plant, united in one mystery, and ever acting each for the advantage of the other, is not an idea to lose its charm by becoming familiar; but it sometimes leads to misconceptions. What, for instance, seems more natural than that the influence of trees planted in our cities should be very beneficial? If trees can thus withdraw the noxious carbonic acid from the vitiated air of cities, would it not be desirable—nay, ought it not peremptorily to be demanded—that as many trees should be planted in our streets as we can find room for? Such conclusions are soon reached by swift logicians. But Nature is apt to elude the grasp of swift logicians, and she repeatedly declines to fall into the most symmetrical of their formulas. Not that Nature is capricious or illogical; but logicians are apt to draw inferences before they have collected sufficient data. Nature, in the present case, point-blank declares that the influence of vegetation on the atmosphere is totally *inappreciable*, unless the atmosphere be in a closed chamber or vessel, and *then* the influence is striking. Human wit has discovered no test delicate enough to appreciate the influence of plants on the free atmosphere in which we live. The depth and compass of this air-ocean are too vast, and the amount of oxygen absorbed by animals too trivial in comparison, for any effect to be appreciable; moreover, the mixture of the gases in the air, and their mutual diffusion, is so rapid, that no difference has yet been detected in the proportions of oxygen and carbonic acid in the air of crowded towns or wooded valleys. The air of cities will hold more noxious exhalations suspended in it, but its gaseous composition will be the same as that of the country. To give an idea of the insignificant part played by animals as vitiators of the great air-ocean, we may mention the calculation made by the distinguished chemist Dumas, that all the oxygen consumed by all the animals on the surface of the globe during one hundred years would not amount to more than the $\frac{1}{8000}$ of the quantity in our atmosphere; and even supposing all vegetation to be annihilated, consequently no oxygen to be returned to the air by the incessant reduction of the carbonic acid, there would still need a period of ten thousand years before the diminution of the oxygen could become appreciable by any instruments we have hitherto invented.

3°. *Varieties in Animals.*—The fact that Respiration is an animal function, dependent upon a peculiar organic apparatus, leads us at once to suspect that it will exhibit great varieties—not only in various animals, but in the same animal under different conditions.

We learn, for example, without surprise, that animals of large bulk consume more air than the smaller animals; horses and oxen more than men; men more than dogs and cats. But, to use an Eastern figure, it raises the eyebrow of astonishment when we learn that the proportion of carbonic acid exhaled by a man and a horse bears no sort of correspondence to the differences in their relative bulk—the proportion being 187 to 16. We are, in like manner, puzzled to find that a full-grown cat only exhales 1⅔ of carbonic acid, where a rabbit produces more than 2. How is this to be explained? Is there not a streak of light trembling on this question when we bring forward the fact previously mentioned, that the vegetable feeders uniformly exhale more carbonic acid than the animal feeders, and that carnivorous animals exhale more than their usual quantity if they are fed on vegetables? Some light may fall from this source, but it does not suffice to clear up the obscurity.

Another interesting problem also arises here:—Although the larger the animal, the greater the *absolute* amount of carbonic acid it produces,* yet the smaller the animal the greater is the *relative* amount it produces. Thus, supposing the production of carbonic acid be estimated according to each pound-weight of the animal, we shall find that the smaller the animal the greater will be its proportion. But it is not size and weight alone which determine the differences in the amount of air consumed; far greater differences will arise from the varieties of organisation. We may accept it as an axiom in physiology, that the activity of Respiration is inseparably connected with vital activity—not simply muscular activity, as some writers maintain, but all processes whatever, involving chemical change within the body. The most striking confirmation of this axiom is perhaps to be seen in the phenomena of hybernation or winter-sleep. No sooner are the vital functions reduced to this extremely feeble condition, in which we may almost say life is suspended, than these hybernating animals are so incapable of ordinary respiration that they may be placed in an atmosphere of pure carbonic acid, and remain there unhurt for four hours; whereas if they were placed in such an atmosphere when their breathing was going on, they would instantly perish.

One would imagine, on hearing this, that our ordinary Sleep would also bring with it a diminution of the quantity of air consumed. And in as far as sleep may be considered a diminution of the vital activity, such a conclusion must be correct. But in how far is sleep a diminution? That is a question not hitherto asked, consequently as yet without an answer. In sleep there is very obvious diminution of some forms of vital activity, but we are by no means sure that the organic changes are so much less rapid on the whole. We are led to this doubt by the experiments of Moleschott and Böcker, which establish that the chief cause of the difference noticed between the amount of carbonic acid produced during the day and night is the influence of *sunlight*; and that a man lying quietly awake will produce *less* instead of more than a man asleep, if the conditions of light and temperature are the same. Sleep, *as* sleep, seems not therefore a diminution of the vital activity; although the sleep which we take at night after the fatigues of the day must of course be considered as accompanied by a diminution. It is quite certain that, partly from fatigue and partly from the absence of sunlight, less carbonic acid is formed at night than during the day. Boussingault found that the same turtle-doves during day and night showed a difference of 94 and 59 on one occasion, and of 75 and 53 on another. Lehmann confirmed the observations.

---

* This applies, of course, only to animals of the same kind. "Vous serez étonné," says Spallanzani, "quand je vous dirai qu'une larve du poids de quelques grains s'approprie presqu' autant d'oxygène dans le même tems, qu'un amphibie mille fois plus volumineux qu'elle."—*Mémoires sur la Resp.*, p. 69. This is because the insect lives so much more rapidly than the reptile.

If it is true that all vital activity increases the amount of carbonic acid exhaled, and if every diminution is accompanied by a corresponding diminution of the amount, we may readily believe that intellectual fatigue, and the lassitude which succeeds mental or emotional excitement, will be accompanied by a corresponding depression of the respiratory function. Nay, even the concentration of the mind on any subject will produce this. Every one knows the state of "breathless attention." Whenever the mind is preoccupied by a powerful impression of some duration, the breathing becomes so feeble that from time to time we are forced to compensate this diminished activity by a deep inspiration. This is the rationale of *sighing*, an action commonly attributed only to grief, but which is the accompaniment of all mental preoccupation. The philosopher, brooding over his problem, will be heard sighing from time to time, almost as deeply as the maiden brooding over her forlorn condition. All men sigh over their work, when their work deeply engages them ; but they do not remark it because the work, and not their feelings, engages their attention, whereas during grief it is their feelings which occupy them.

It is an interesting fact, and one which throws light on the intimate connection between respiration and vital activity, that a very considerable increase in the production of carbonic acid swiftly follows after eating, consequently an enormous reduction in the amount is found to accompany starvation. The fact was established by Spallanzani, and has been repeatedly confirmed. Boussingault found that pigeons, when fasting, did not produce half the amount which they produced when well fed. Spallanzani suggests that the food during digestion gives off carbonic acid, and this passing into the blood, is exhaled in respiration—a suggestion which receives additional force from the fact that vegetable food uniformly produces more carbonic acid in respiration than animal food. But this will scarcely account for the whole of the increase, and we are led to seek for the other cause in the greater activity of the nutritive processes: the fasting animal has a depressed vitality.

Temperature has considerable influence on respiration. The fact has been ascertained by experiment, but it might have been deductively established; for the influence of temperature on the vital activities is well known, and whatever influences them must affect respiration. It is only by the aid of such an axiom that we can find our way amid the apparent contradictions of this subject. The remarkable difference noticed between the capabilities of warm and cold blooded animals in breathing vitiated air, is not less than the difference in the effect of temperature on these two classes. We remember our astonishment on learning from Spallanzani that increase in the temperature brings with it an uniform increase in the amount of oxygen absorbed by molluscs and reptiles; it was a statement in direct contradiction to the well-established fact in human physiology, that more oxygen was absorbed in cold than in hot weather. Our difficulty was lightened, however, when we learned that Spallanzani's statement is only true of cold-blooded animals, and true of them only within certain limits: too great a heat ceases to increase the amount, and gradually diminishes it, as with warm-blooded animals. What are these limits, and why this cessation of increase? The limits are these: take a frog and place it in an atmosphere a little above the freezing-point; as the temperature rises from 36° to 45° Fahrenheit, the amount of oxygen absorbed uniformly increases; it remains nearly stationary from 45° to 57°; at 58° it begins to decrease, and this decrease continues till 104° is reached, and then the frog perishes. The reason is very simple: a certain amount of heat stimulates all the vital functions of the frog, and consequently increases its need for oxygen; when the heat becomes too great it ceases to be a stimulant, and depresses the functional activity, till at length a point is reached when the organism can no longer exist.

On warm-blooded animals the effect of temperature is apparently different, but really the same. Every increase of heat is found to *diminish* their respiration, every increase of cold to *augment* it. Thus it is ascertained that the smaller mammals, at a temperature of 86° to 104° Fahr., consume only half the quantity they consumed at freezing-point. Various experiments on man have elicited the general fact, that under the influence of a moderately cold atmosphere the respiration is increased by one-sixth more than in a moderately warm atmosphere. Precisely as too intense a degree of heat diminishes the respiration of the frog, by enfeebling its vital activity, does too intense a degree of cold diminish the respiration of a warm-blooded animal by enfeebling its vital activity. There are certain limits of temperature within which every increase of heat raises the respiration of the frog, because the increase raises its vital activity; and there are certain limits within which every decrease of heat raises the respiration of the man, because the decrease raises his vital activity; but if these limits be overstepped, the stimulant is changed into a debilitant.

We see this very curiously illustrated by the hybernating animals, such as the dormouse, marmot, bat, hedgehog. They occupy, in this respect, an intermediate position between the cold-blooded and warm-blooded animals; for although they are really warm-blooded animals, the effect of temperature on them is closely allied to that produced on the cold-blooded. With every fall of external temperature their respiration diminishes. Unlike the rest of warm-blooded animals, their organism seems to have little power of resisting the changes of external temperature; they cannot produce heat with sufficient rapidity to counterbalance the loss they sustain from the surface of their bodies when the air is cold. Instead of acting on them as a stimulus, which would accelerate the respiratory process, cold acts on them with a depressing influence, which gradually reduces their respiration almost to zero. But no sooner have they passed into this winter-sleep, and their organic activity has become very feeble, than we can at pleasure reawaken it to any degree by raising the surrounding temperature; and as the vital activity once more begins to manifest itself, the respiration (which is only one form of vital activity) likewise becomes manifest.

Why do we breathe? The foregoing pages have given some answer to the question, *How* do we breathe? but have not hinted at the *why;* yet after reading about the respiratory process, a natural curiosity prompts the inquiry as to its cause. Unhappily nothing but extremely vague answers can be given. We know that the chest expands and contracts with beautiful rhythm, and, mostly, as an involuntary, automatic process. We know that our attention is not required, that no effort is needed, and indeed that no effort of ours can prevent the regular alternation of inspiration and expiration. We can by an effort accelerate or retard these motions, but we cannot prevent them. The process, then, clearly depends on a stimulus given to the involuntary part of the nervous system: it is called into action by nervous stimulus, and physiologists have vainly endeavoured to discover the nervous apparatus which is involved, and the rationale of its action. The pressure of carbonic acid in the air-cells, or of venous blood in the capillaries, may act as a stimulus to the pneumogastric nerve; but what is the rationale of making a new-born child draw breath, by whipping its back and continuation? Generally, the stimulus of the cold air on the child's face suffices to make it draw breath, which it expires again in a well-known cry, to mothers' ears most musical; but this stimulus is often insufficient, and the doctor or nurse initiates the little stranger into that experience of "external local applications" which, in later years, will also be freely used as a stimulus to virtue or learning. The fact we know; but why such "local applications" excite the respiratory activity, we do not know, for we do not know the nervous apparatus which regulates the actions of respiration. It is probable that the researches of physiologists will, ere long, clear up this point, as they have cleared up so many others; meanwhile we must content ourselves with vague answers to our question, Why do we breathe?

CHAPTER VII.

WHY WE ARE WARM, AND HOW WE KEEP SO.

Animals have a temperature of their own—This not owing to a "Vital Principle"—Description of the various instruments employed in measuring animal heat—Mistake of supposing that there are cold-blooded and warm-blooded animals—Man preserves his temperature in all climates and all seasons—His power of enduring great heat—The cooling apparatus of his organism—Perspiration carries off superfluous heat—Difference between feeling hot and being hot—Influence of age, sex, and food on our temperature—Young animals cannot well resist cold—The notion of "hardening" infants erroneous—Is food warmth?—Influence of the seasons—Effect of a cold wind—Why is the east wind felt to be cold?—Where is cold dangerous?—Can cold water be drunk with safety when we are hot?—The theory of animal heat—Respiration is not the cause of animal heat—Heat in dead bodies—Relation between respiration and animal heat in various classes of animals—Hybernating animals—Frogs and tritons in winter and summer.

A BIRD-CAGE hangs above a small aquarium: in the cage there is a bird; in the glass tank, seaweeds, zoophytes, molluscs, and fish. The atmosphere of the apartment varies with those variations of temperature which accompany the earth's daily rotation and annual movement. The summer sunlight streams in through the windows; the icy north wind rushes through the crevices; the shadows of night, and the evaporations of morning, bring with them perpetual risings and fallings of the temperature of that room; and with these risings and fallings there are corresponding fluctuations in the temperature of the glass and water of the tank, the brass and woodwork of the cage. This is according to the law by which an equilibrium of temperature is always established among inorganic bodies. The warmer atmosphere rapidly warms the glass and water—the cooler atmosphere rapidly cools them; it is true that the water will always be somewhat colder than the atmosphere, because it loses heat in evaporation, but nevertheless, as the external temperature rises and falls, that of the water also rises and falls.

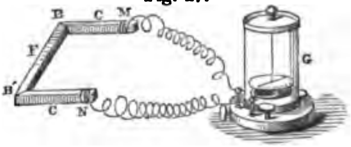
While these changes, so familiar and so easy of explanation, have been taking place, the bird has been neither colder nor warmer; throughout the fluctuations of external temperature it has preserved almost uniformly the very high degree of warmth which, as a bird, belongs to it. Neither the beams of an August sun, nor the nipping east wind of December, have raised or lowered its normal heat at any time more than one or two degrees. You may perhaps imagine that it has been kept warm through the winter by its envelope of feathers, but this is true only to a very slight extent; strip it of its feathers, and you will still find its heat greatly above that of the air; whereas, if a heated substance be enveloped in feathers, and left exposed to the air, it will soon become as cold as the air. Driven from this explanation, you will ask, How is it that the bird is enabled to preserve a steady temperature of a high degree amid unsteady influences from without? The answer is obviously to be sought in the organism and its processes, not in any external influence; and a certain Philosophy, somewhat rash and ready, fond of phrases and impatient of proof, will assure you that the bird, as an organised body, is absolved from the law of equilibrium which rules all inorganic bodies, because the bird is endowed with a "vital principle which suspends the action of physical laws."

This explanation, which to many has seemed satisfactory, labours under two disadvantages—first, that it invokes the operation of a "vital principle," of which we can form no definite conception; and secondly, that the assumed suspension of physical laws is a pure figment. The organism, living or dead, radiates heat with equal facility; but when living, it produces heat to compensate the loss; and when dead, it no longer produces heat, so that it speedily becomes as cold as the external air. The processes of Life do not "suspend" the operation of physical laws, although, by the introduction of more complex conditions, they bring about results which, superficially considered, look like a suspension of those laws. A close analysis always detects the physical laws. No one thinks of attributing to a spirit-lamp, when lighted under a vessel of water, the power of suspending the equilibrium of temperature, because it keeps the water boiling, in spite of the constant loss of heat by evaporation. Without the lamp, the boiling water would speedily cool below the temperature of the air; *with* the lamp, it may be kept indefinitely at the boiling-point, if fresh water be from time to time added to replace what has evaporated. There is no "lamp-principle" suspending physical laws. Nor is there any such mysterious agent in Animal Heat. Just as the temperature of the water is kept constant by the continual reproduction of heat equalling the amount lost, so is the temperature of the bird kept constant by a continual reproduction of heat within; and although the vital processes by which that reproduction is effected are very far from exhibiting the simplicity of the spirit-lamp, and are indeed still involved in great obscurity, yet we know that physical laws are in no sense suspended thereby, and that the living animal has the tendency to establish an equilibrium between its temperature and that of the objects surrounding it.

We have only to extend our investigations and examine the temperature of the other organised bodies—seaweeds, zoophytes, molluscs, and fish—during these changes which *seem* not to have affected the bird, to find that this mysterious "vital principle" suddenly fails altogether. It here abdicates its autocratic power. It suspends no laws; on the contrary, it permits equilibrium to be established unopposed. The seaweeds are, within a very slight fraction, as cold as the water, and get warmer as the water warms. The zoophytes have no appreciable superiority of temperature. The fish are only two or three degrees warmer. Either we must give up the explanation which the vital principle seemed to afford, or we must deny that the cold-blooded animals, as they are called, have any vital principle at all. In vain will a refuge be sought in the greater cooling agency of water over that of air; for although something must be allowed for this, we cannot by it account for the enormous disproportion between the temperature of the fish and the bird; and for these reasons: The Bonito, equally subject to this cooling agency of water, preserves a constant temperature of 20° above the sea; and the temperature of the Narwhal is nearly that of man—namely, 96° Fahrenheit. Moreover, while some marine animals are thus independent of the temperature of water, serpents, lizards, and frogs are dependent on the temperature of the air.

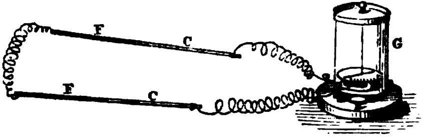
Before entering further into this subject, it will be well to glance at the instruments by which physiologists measure the delicate differences in amount of heat which different animals and plants produce. Of the various thermometers we need say nothing. But there are thermo-electrical instruments which need explanation. Fig. 27 represents a thermo-electric couple. Its construction rests on the fact that two metals soldered together develop electric currents when their solders are maintained at different temperatures. Two bars of copper (C, C) are soldered to a bar of iron (F), and their two extremities (M, N) are in contact with the wires of the galvanometer (G); so long as the solders (B, B′) remain at the same temperature no electric

Fig. 27.



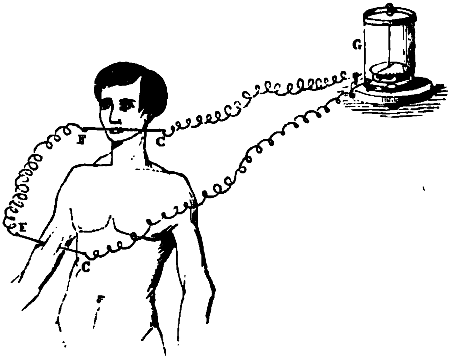
current passes; and the magnetic needle of the galvanometer remains motionless. But the instant one of these solders is heated, the needle moves. It is on this principle that Becquerel invented his *thermo-electric needles with median solder* (Fig. 28). Each needle is composed of a copper and an iron half of equal length and thickness, soldered end to end. At one extremity each iron half (F) is united to the other by an iron wire, long enough to permit of their being kept at some distance apart; at the other extremity each copper half (C) is fixed to the wires of the galvanometer.

Fig. 28.



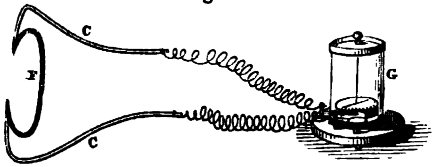
With such an instrument we are enabled to penetrate, without disturbance, into the interior of the animal tissues, and accomplish what no thermometer could attempt. We can test by it the temperature of a muscle, or of the blood in an artery, without troubling the circulation. For instance, let us desire to know the temperature of the muscles of the arm, at rest and in contraction. Fig. 29 will show the method of procedure. One needle is passed through the muscles; the other is placed in the mouth. The patient breathes only by the nostrils during the experiment; and all the time a small thermometer is placed under his tongue, to prove that the temperature of the mouth does not vary. The direction of the needle indicates in which part there is an excess of heat; and this is afterwards calculated.

Fig. 29.



Another application of the same principle is that of Dutrochet's *needles with latero-terminal solder* (Fig. 30). An iron needle (F), bent at both ends, is soldered by these bent ends to two copper needles (C, C). With this instrument he compared the temperature of a living and a dead insect, as represented in Fig. 31.

Fig. 30.



Each insect was fixed on a stick, *d d*, planted in the earth of a flower-pot *a*. The needles were then thrust into corresponding parts of the living and dead insect, and being in communication with the galvanometer by the wires M N, the deviation of the galvanometer gave the measure of the temperature possessed by the living insect. The same instrument serves to measure the heat of plants, Fig. 32. An asparagus, *c*, is freshly cut and placed in water to keep it alive; another asparagus, *b*, is killed, and the two are then tested as the living and dead insects were tested.

Fig. 31.

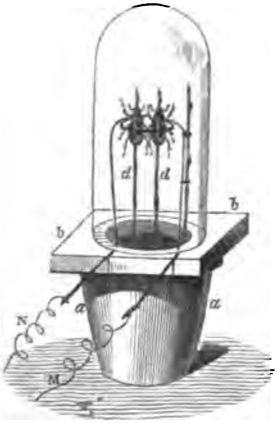
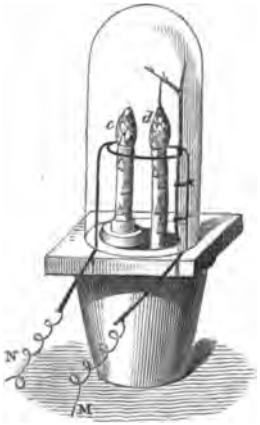


Fig. 32.



There are other methods unnecessary to mention here. The result which has been arrived at is that every living organism has within it a source of self-supplying heat. The amount of heat thus supplied will depend on the amount and nature of the chemical changes which take place within the organism. Even in the microscopic animalcules such a supply exists. We might assume this *a priori*, but we can establish it experimentally: if water be gradually frozen under the microscope, it will be seen that the last drops which solidify are those which surround the animalcules, and have been kept liquid by their heat.

Organic beings are thus distinguishable from the inorganic in possessing, as a necessary consequence of their vital activity, a self-supplying source of heat; and organic beings are distinguishable *among each other* by the rapidity with which this heat is *supplied,* and the facility with which it is *radiated;* and not, as the current classification implies, into animals with warm blood, animals with cold blood, and plants with no heat at all. There are no animals with cold blood, and all plants produce heat. But plants, except during their periods of germination and flowering, when they are sensibly warmer than the air, produce heat so slowly, and part with it so easily, that their temperature always follows that of the medium in which they live; and the so-called "cold-blooded animals" produce heat so slowly that they are never more than two or three degrees above the medium, and sometimes even *below* it, owing to the rapidity of evaporation from their surfaces. Insects—bees, for instance—produce heat with a rapidity equal to that of birds; but they part with it so rapidly that their temperature is little above that of the air. When bees are collected together in the hive, the heat they radiate is found to be very great.

Many writers object to the old distinction of warm and cold blooded animals as unphysiological, and suggest that the distinction should be, that the warm-blooded are independent of, and the cold-blooded dependent on, the external temperature: the one class preserving its normal heat under all variations of the external medium, and the other class growing warmer and colder as the external medium rises and falls. But against this distinction we would urge three arguments. First,—Both classes of animals are dependent on the external temperature, and both are independent of it; they are *dependent*, because it accelerates or retards their vital activities by which their own heat is evolved; they are *independent*, because whatever may be the amount of external heat or cold, their own temperature, being really evolved in their vital processes, is always restrained within certain limits, and is almost always *somewhat* above that of the external medium,* until a limit is reached, and then, if the external temperature continue to increase, they perish, or their heat falls below that of the medium.

Secondly,—The young of many warm-blooded animals are as much dependent on external temperature as frogs and fish; and even the adult animals of the hybernating class are in this category: no sooner does the external temperature fall, than their heat sinks, and this depression continues till they are only three degrees warmer than the air.

---

* I once found the temperature of a "cold-blooded" lizard to be 56° in the mouth, when the air was at 54°. A list of observations on the temperature of the cold-blooded animals will be found in GAVARRET: *De la Chaleur produite par les êtres vivants*, p. 123.

Thirdly, — While the foregoing arguments have shown that the distinction is not tenable in the presence of facts, we would further remark that, granting the distinction to be valid, the cause would still have to be sought, and we should ask, why one class of animals was independent and another dependent on the external temperature? In fixing attention on the physiological differences of *rapid supply* and *rapid radiation*, as the real ground of distinction, we avoid the objections just brought forward; at the same time, inquiry into the cause of animal heat is disengaged from many a perplexing digression.

The marvellous balance between supply and loss exhibited by the human organism, and indeed by that of most warm-blooded animals, may be best seen in the following facts. Our temperature is 98°, and this is the standard, no matter what may be the external heat. In the tropics, the thermometer during several hours of the day is 110°. In British India it is sometimes as high as 130°. In the arctic zones it has been observed by our voyagers as low as 90°, *and even* 102°, *below freezing-point.* Nevertheless, amid such extensive variations of the external temperature, that of the human organism has but slightly varied, and a thermometer placed under the tongue of an arctic voyager will show the same degree of heat as one under the tongue of a soldier before the walls of Delhi. Throughout the scale of 200° which represents the variations of climate borne by our voyagers and soldiers, the average temperature of the human organism remains steady at

98°. We say *average*, because the same man is not always at the same degree; his temperature varies at different seasons, different hours, and under different conditions; and of course different men vary among themselves. Dr Livingstone remarks, "If my experiments are correct, the blood of an European is higher than that of an African. The bulb of the thermometer held under my tongue stood at 100°; under that of the natives, at 98°." * This was most likely nothing more than an individual difference; but the point is worth investigating.

It is the opinion of M. Gavarret, who has earned the right to speak with authority on this subject, that even in our moderate climates the temperature of the adult man will oscillate as much as a degree, *i.e.*, between 36°.50 and 37°.50 centigrade.†

Although the organism can endure a heat greater than its own, yet this would soon be fatal if continued. For a short period the excess of temperature can be resisted; and it is astonishing what a power of resistance we possess. Chabert, the once celebrated "Fire-King," who used to exhibit in public, amazed his audience by entering an oven, the heat of which was from 400° to 600°; and although we have no details as to his own temperature when subject to that heat, we may be sure that it could not have risen many degrees above

---

* LIVINGSTONE'S *Travels in South Africa*, p. 509.

† GAVARRET: *De la Chaleur produite par les êtres vivants*, 1855, p. 100. This work is a perfect encyclopædia of information on the subject of Animal Heat. It is from it we have borrowed the figures given above.

98°, otherwise he would have perished; for the experiments of Berger and Delaroche[*] prove that, when the temperature of animals is raised 11° or 13° above the normal standard, they perish. Workers in iron-foundries and gasworks are constantly obliged to remain for some time in air which is as high as 250°, yet their own temperature remains tolerably uniform. A dog confined in a heated chamber at 220°-236°, in which he remained half an hour, was found to have gained only 7°; and while the external temperature stood as high as 236°, his own stood only at 108°.

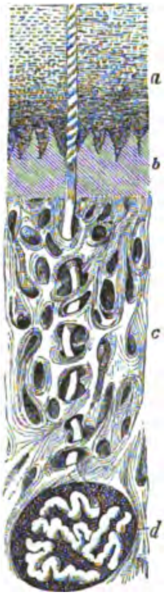
It thus appears that warm-blooded animals, besides their central source of heat, which keeps up their temperature in spite of external cold, have also a cooling apparatus by which their standard of heat is preserved in spite of excessive heat outside. What is this process, which prevents the equilibrium of heat, and can keep the animal temperature more than one hundred and fifty degrees *below* that of the atmosphere? We can easily understand why a kettle of water can be kept at boiling-point in a cold atmosphere, so long as a flame is underneath it; but how can that water be kept *cold* when the temperature of the air is many degrees above boiling-point? A man whose temperature is 98° in an atmosphere of 60°, suddenly steps into an atmosphere of 200°, and yet his own warmth is scarcely elevated. The ordinary explanation of this surprising fact is, that the evaporation and exhalation of vapour and water from the surface are so accelerated by the excessive heat, that they suffice to keep the man's temperature from rising. Let us look more closely into this.

---

[*] Quoted by W. EDWARDS, *De l'Influence des Agens Physiques sur la Vie.*

All over the surface of our bodies there are scattered millions of minute orifices which open into the delicate convoluted tubes lying underneath the skin, and are called by anatomists *sudoriparous glands* (Fig. 33). Each of these tubes, when straightened, measures about a quarter of an inch; and as, according to Erasmus Wilson, whose figures we follow, there are 3528 of these tubes on every square inch of the palm of the hand, there must be no less than 882 inches of tubing on such a square inch. In some parts of the body the number of tubes is even greater; in most parts it is less. Erasmus Wilson estimates that there are 2800 on every square inch, on the average; and as the total number of such inches is 2500, we arrive at the astounding result that, spread over the surface of the body, there are not less than *twenty-eight miles of tubing*, by means of which liquid may be secreted, and given off as vapour in *insensible* perspiration, or as water in *sensible* perspiration. In the ordinary circumstances of daily life,

Fig. 33.



SWEAT-GLAND.

a is the cuticle or scarfskin (epidermis), the deeper layers, dark in colour, being the *rete mucosum*; *b* are the papillæ; *c* the cutis or true skin (derma); *d* the sweat-gland, in a cavity of oily globules.

the amount of fluid which is thus given off from the skin (and lungs) during the twenty-four hours, varies from 1⅜ lb. to 5 lb.; under extraordinary circumstances the amount will of course rise enormously. Dr Southwood Smith found that the workmen in the gasworks employed in making up the fires, and other occupations which subjected them to great heat, lost on an average 3 *lb.* 6 *oz.* in forty-five minutes; and when working for seventy minutes in an unusually hot place, their loss was 5 *lb.* 2 *oz.*, and 4 *lb.* 14 *oz.*

Whatever stimulates the circulation of the blood at the surface, will necessarily increase the action of the sweat-glands. A warm atmosphere or a warm bath immediately causes the surface-circulation to be increased. Muscular exertion does the same. That the ordinary amount of evaporation and exhalation will be greatly raised on our entrance into an atmosphere of 200° is very certain; but the question is, whether this amount is sufficient of itself to account for the enormous difference between the temperature of the animal and that of the atmosphere? We must remember that not only is man's temperature more than 100° lower than iron or wood in such an atmosphere, but it is this amount lower *in spite of the incessant production of heat* taking place in his own organism, by the chemical changes on which vitality depends—a production of heat which will suffice to preserve his temperature at the same height, if, on quitting this atmosphere of 200°, he plunges into a snow-bath. For a short period a man can enter a furnace the floor of which is red-hot, the air being 350°, yet his own heat will remain two hundred and fifty degrees *below* this; and we cannot suppose that, in this brief period, he has lost enough heat by evaporation to prevent his own temperature rising. What, then, is the cause? We must confess inability to answer this question. For some time we fancied an explanation might be gained from the low-conducting power of the animal envelope, which would prevent the external temperature from gaining access to the internal organs. Wrap a jug of ice in flannel, and the ice will not melt, even in a very warm room, until a considerable time has elapsed. It is on this principle that, in China, they *bake ices.* An ice is enveloped in a crust of delicate pastry, and introduced into the oven. The paste is quickly baked, and the ice is still unmelted, having been protected from the heat by its envelope; and thus the epicure has the delight of biting through a burning crust, and then immediately cooling his palate with the grateful contents. But although the envelope of the warm-blooded animal is unquestionably a bad conductor, and would therefore suffice to account for the animal's not getting warmer during a brief exposure to high temperature, this explanation fails when confronted with experiments which show that, during a longer exposure, the temperature has been still at its old limit. The following experiment by Berger and Delaroche is very instructive:—They introduced into a furnace at 126°-143° a porous vase, containing two wet sponges and a frog. The temperature of the sponges and the vase had been previously raised to 101°-105°; that of the frog was 70°. At the close of the first fifteen minutes, the vase, the sponges, and the frog had almost an uniform temperature, *which did not exceed that of warm-blooded animals;* and this was maintained pretty constant during two hours. It is remarkable that, to reach this standard, the vase and the sponges *fell* in temperature about a degree and a half, whereas the frog rose as much as twenty-nine degrees in fifteen minutes. But frog, sponges, and vase maintained themselves from twenty to forty-five degrees *below* the external temperature—namely, at the temperature of warm-blooded animals. From this it would seem as if the temperature of warm-blooded animals was the limit which could be reached by organic bodies coincident with a free evaporation of water from their surfaces. The vase and the sponges were introduced into a furnace very considerably higher than themselves in temperature; and this excess of heat caused an evaporation of their water, which lowered their temperature to that point where the rapidly-rising temperature of the evaporating frog would stop. Now, although the evaporation from the surface of the frog would have had a cooling influence from the first minute of the experiment, yet we see that this cooling influence was not great enough to withstand the rapid rise of the animal's temperature, until the point was reached at which the fall of the vase and sponges had ceased; and this point is the very limit which we find uniformly in the warm-blooded animals, no matter what the external temperature may be.

The temperature of man is constant: neither the fluctuations of the seasons, nor the differences of latitude, bring any variation in his standard. No fact in science is better established; but we must guard against a misconception, and add, that when this temperature is spoken of as constant, it is not the heat of individual parts, nor of individual men at every hour of the day or under all circumstances, it is the *average* temperature of the internal organs. The limits of oscillation are narrow indeed, but within those limits the oscillation is incessant. I found that, when a thermometer is used which marks fractions of a degree so small as one-tenth, if placed in the mouth of a man or dog, it will exhibit an incessant oscillation; and it is well known that very obvious variations occur at different periods of the day, and under different circumstances. Dr John Davy found that, when at rest in a temperature of 37°, his own temperature fell as low as 96°.7; in a room at 92°, he found the heat of a workman had risen to 100°. Gierse observed that, before dinner, his temperature under the tongue was 98°.78, and after dinner, 99°.5.

Unsophisticated Reader.—"We want no scientific authority for the belief that variations take place, since the daily complaints of our fellow-citizens, shivering or perspiring, render the fact too obvious for thermometers to be needed."

Physiological Lecturer.—"Excuse me, sir; but you do want scientific authority. Without the thermometer, you cannot say whether you are warmer or colder than you were."

U. R.—"What! do you tell me I don't know when I feel colder?"

P. L.—"I only tell you that you don't know when you *are* colder. What you may *feel* is another question altogether. Thermometers do not measure feelings."

U. R.—"This is too much! You will tell me next that I ought to trust your thermometer, and distrust my sensations. Before venturing to light a fire in my room, I must place a thermometer under my tongue to see if I am really as cold as I feel; and next July, when I am sweltering in the sun, you will perhaps assure me that I am wrong to complain of the heat, since I am only at 98°—not a degree hotter than I was in December!"

P. L.—"O Unsophisticated Reader! if we are to talk science, let us be accurate. If we are to talk the language of the market and the dining-room, we shall never come to a clear understanding. Were you ever attacked by an intermittent fever?"

U. R.—"Yes; and remember distinctly the vehement shivering-fit which commenced it. Perhaps you will tell me I was not colder then, nor warmer when that fit disappeared and I seemed on fire, eh?"

P. L.—"Your ironical question warns me of your incredulity when I shall answer: No, you were *not* colder when you shivered under the heap of blankets which they in vain threw over you. You were some degrees warmer. The application of that thermometer, which you seem to treat lightly, would have shown that, whatever your sensations may have said, the actual heat of your skin had *risen six or seven degrees;* and when that sensation of cold was succeeded by a sensation of burning heat, the thermometer, which knows nothing of sensations, but measures heat only, would have shown that you were really not hotter than during the cold fit."

U. R.—"That is very staggering. But fevers are exceptional things, so let us come to ordinary life. Do you mean to say that, when I feel cold in winter, I am really not colder than when I come in from the walk which—according to my sensations—has warmed me?"

P. L.—"As an Unsophisticated Reader, liable to chilblains, your feet are doubtless colder and warmer under these two conditions; and the thermometer placed between your toes before you walk will show a temperature of 66° perhaps, while that of the air is 60°; and after the walk, the same test will show your feet to have risen as high as 96°.5. But if I regard you as a scientific *datum,* and think only of your Animal Heat as an average, I am forced to assure you that your temperature, variable in feet and hands, has remained constant in the blood and internal organs. A thermometer under your tongue would show 98° before and after your walk. Exercise had increased the circulation in your limbs, and consequently increased the warmth of those parts; but the source of your heat is the Blood, and that has not warmed or cooled with exercise. Dr John Davy found, after a walk in the open air at 40°, that the temperature of his feet rose to 96°.5; of his hands, to 97°; while his tongue remained at 98°. Another day, after a walk in the air at 50°, his feet were 99°, his hands 98°, and tongue 98°. Here the feet were even warmer than the tongue. I can now answer your question without equivoque. When you feel colder, it is because the circulation in your extremities, or at the surface, is less active than usual, and either exercise or external warmth is necessary to restore that circulation; but the temperature of your blood, and, consequently, of those internal parts more abundantly and constantly supplied with blood—in a word, your Animal Heat—remains unaltered."

Our Lecturer, had he been questioned, would have stated that, in spite of this remarkable constancy in animal heat, there are oscillations even in that of the internal organs. The temperature varies according to Age, Sex, Food, and other circumstances. We cannot, it is true, speak with any confidence as to the exact share which any one of these circumstances has in the variations observed; the case is so complex, and implies the concurrence of so many separate influences, that considerable discrepancies will be found in the results attained by different investigators. Thus the majority of writers agree that in infancy the temperature is higher than in maturity. A thermometer under the armpit of a new-born infant will stand nearly at 100°; and although it falls rapidly to 95°, yet in the course of the next four-and-twenty hours it will rise again to 99°.5. Between the ages of four months and six years, the average is 98°.9, and between the ages of six and fourteen it is 99°.16. On reading these figures the physiologist is tempted to see in them the simple expression of the fact that, during infancy and childhood, the growth is much more rapid than it afterwards is; and this rapidity of growth implies a greater production of heat, because a more rapid chemical action. Nevertheless, the extensive observations of M. Charles Martins * and W. F. Edwards † disturb the simplicity of this explanation, and cast some doubt on it. M. Martins, comparing the temperature of fifty-six ducks and ninety-seven geese, finds that in infancy it is somewhat *less* than in maturity. This fact is in direct contradiction to the physiological explanation; but it may possibly have some connection with the very important fact established by W. Edwards, namely, that young animals are distinguished from the adult less in the *degree* of heat which they attain than in their want of power to resist cold by *rapid production* of heat. This, as we attempted to show just now, is the distinguishing characteristic of what are called the warm-blooded and cold-blooded animals; and we shall now see that it is a characteristic of Age. Edwards removed a new-born puppy from its mother, and left it exposed to the air at 50°-60°. It rapidly grew cold, until at the expiration of three hours it was only two or three degrees above the temperature of the air. A similar result was observed with new-born rabbits, in a shorter space of time. With the new-born guinea-pig nothing of the kind was observed. It had a temperature equal to that of its mother; and this it preserved whether left with the mother or removed from her. Dogs, cats, rabbits, and other warm-blooded animals, therefore, seem in their earlier periods of existence to resemble the cold-blooded animals, and to be dependent on external warmth ; but this is not true of all the warm-blooded classes. Edwards divides those classes into two groups, one of which comes into the world cold-blooded, the other warm-blooded. If we examine these groups closely, to discover some external sign by which they may severally be known, we find that one group comes into the world with its eyes closed, and the other with its eyes open. The reader, probably, thinks this sign of very small value, until we beg him further to remark that the puppy, whose temperature was so dependent on external warmth during the early part of its existence, becomes less and less so as he grows older, till at the end of a fortnight he is almost as capable as his parents of resisting external cold—and at this epoch his eyes are open. Thus the cold-blooded period is precisely coincident with the blind period.

---

* *Mémoire sur la Température des Oiseaux du Nord,* in BROWN-SÉQUARD: *Journal de la Physiologie,* i. 22.

† W. EDWARDS: *De l'Influence des Agens Physiques,* p. 132.

Is, then, Animal Heat dependent upon vision?—is it in any degree regulated by vision? The phenomena are, indeed, intimately connected, but their connection is not that of cause and effect: it is that of two effects determined by one cause. Animal Heat is evolved by the vital changes which take place in the organism; and only when that organism has attained a certain degree of development has it the power of evolving sufficient heat to resist external cold: now, the development of the eyes is an indication of the degree of development reached by the organism, and no sooner is the animal sufficiently developed to use its eyes, than it is also sufficiently developed to preserve its normal temperature. The young puppy cannot see, and is forced to remain near its mother to be warmed by her; but the young guinea-pig sees perfectly well, and runs about seeking food for itself. It is the same with birds. Young sparrows, taken from the nest where they were kept warm by their mother and by each other, rapidly lost about 30°, although the external air was moderate (63°), so that their own temperature fell to three degrees above that of the air. A similar result was obtained with the air at 72°. As these birds are born without feathers, their loss of heat might be supposed to be owing to the absence of the warm covering which protected their parents. It is not so, however. Edwards completely stripped an adult sparrow of its feathers, and exposed it to air at 65°, in company with a young sparrow taken from the nest; the young one had the advantage of down, and in some parts of feathers to protect it, nevertheless its temperature quickly fell to two or three degrees above that of the air, while that of the adult scarcely varied, and remained 36° above that of the air. Although the young sparrows, and those birds which are born imperfectly developed, require the heat of the nest and of their mother, the young chick preserves its temperature as well as the grown hen; but the chick quits the egg in a state of development which permits it to run about and feed itself.

Have these facts any application to man? Are *we* born cold-blooded, like blind puppies? Not exactly; yet the same laws are in operation in our organisms as in the organisms of the puppy and the sparrow. Edwards relates that one of his patients gave birth to a seven-months child, with such ease that the child came before assistance could be got. He arrived two or three hours afterwards, and found the child vigorous, well swaddled in clothes before a good fire; yet its temperature, even under these circumstances, was only 90°, or five degrees below that of the average of a child born at the proper period. Every precaution was taken to keep the infant warm; had not such precautions been taken, it would have perished as a puppy would have perished.

Maternal instinct has in all ages and in all climates taught women to keep their infants warm. Philosophers have at various times tried, by logic and rhetoric, to thwart this instinct. Philosophy has been eloquent on the virtue of making infants "hardy," and has declared that cold baths and slight clothing must be as "strengthening" to the infant as to the adult. Listen to none of these philosophers, ye mothers! They are to be suspected when they are talking physiology, for under such circumstances they are the worst of guides, deceiving themselves and you by that fatal facility which intellectual power gives them of making ignorance look like knowledge, and of so speciously arraying absurdity that it looks like plain common-sense. It is bad, very bad, to listen to grandmothers, mothers-in-law, and nurses: their heads are mostly mere lumber-rooms of crotchets and absurdities; but it is better sometimes to listen to them than to philosophers who inspire more respect, and cannot irreverently be treated as "old women." Maternal instinct must not be perverted by such unphysiological teaching as that of "hardening" infants. It is true that strong infants can endure this process, but it is certain that in all cases it is more or less injurious; for the universal law is, that the younger the animal, the feebler its power of *resisting* cold, in spite of its possessing a higher temperature than the adult.

An interesting fact is elicited by Edwards from his researches, namely, that although the younger the animal the less its ability to resist cold, this peril is to a great extent evaded by the comparative impunity with which the young animals can be subjected to a fall in their temperature. The adult better resists external cold; but if the resistance be overcome, there is greater difficulty in re-establishing the normal heat. In proportion as the faculty of developing heat increases, the faculty of developing it after a considerable fall decreases. One sees the beneficial operation of this law in nature. The most careful bird is forced to quit her young from time to time in search of food, and during her absence they necessarily become colder; if she is absent long, or if the nest be not very warm, they will undergo a loss of heat which would be perilous to an adult. But no sooner does she return to warm them, than they regain their temperature with facility.

Old people are commonly said to have a lower temperature than those of middle age; but Dr Davy's observations do not confirm this; he found no such difference, nor are we aware of any evidence by which the notion can be established. It is true that cold is not so well resisted in old age. Herein old age and infancy agree.

The influence of *Sex* has not been much investigated; yet, considering the differences in the blood and respiration of the sexes, we might expect to find some striking results elicited from a careful comparison of temperatures. The only extensive investigations with which we are acquainted are those of M. Martins, previously cited: from one hundred and ten observations on ducks and drakes, he finds the temperature of the females to be somewhat higher than that of the males; but their temperature is also more *variable*, differing between individuals more than is the case with the males. Between women and men there seems to be no appreciable difference, which is the more striking from the known differences in their blood and respiration.

"Food is warmth," says the physiologist; and in one sense this is strictly true, namely, that Food, by rendering a continuance of the vital processes possible, must bring with it the heat to be evolved in those processes. But it is *not* true in the sense in which the aphorism is frequently employed, namely, that Food is the fuel which is burned for animal heat (like coke in a furnace), and that particular kinds, the so-called Respiratory Foods, are those we ought to employ as fuel. The warmth you feel after eating a hearty dinner is not really an increase of your temperature, but a *diffusion* of it to the extremities and the surface. Place a thermometer under your tongue before dinner and after it, and you will find that, in spite of your sensations, the thermometer points to the same degree at each period. Yet, as this admits of another interpretation, we shall adduce the less equivocal observations of M. Martins. The ducks belonging to a miller near Montpellier were fed well on grains every morning before being turned out to enjoy themselves in the river, and every evening on their return they were fed again; close by, there lived a poor waiter on the lock, who also kept ducks, but could not afford to feed them on grains, like his richer neighbour, so that they were reduced to forage for themselves. Here accident had arranged the conditions of a good scientific experiment. Living in the same air, the same temperature, and in the same lock of the river, these two flocks differed only in respect of the grain on which one was daily fed. The influence of food would, therefore, here be manifest. What did observation detect? A superiority of temperature amounting nearly to a degree centigrade (about

1⅘° Fahr.) in favour of the well-fed ducks. M. Martins adds that he has since then often been able to affirm whether a bird has been well or ill fed, by simply ascertaining its temperature.

On a superficial consideration, this would seem to be convincing evidence that those physiologists are correct who assert "food to be warmth," in the crude sense of food being fuel; but closer attention will show that the evidence supports our view of food. Indeed, M. Martins has furnished us with irresistible evidence; "for," as he remarks, "we shall greatly deceive ourselves if we imagine that a better quality of food will suffice to raise the temperature in *a short period.* Two drakes, after five days of abstinence, were found to have a temperature of 41°.83 centigrade (about 107° Fahr.); I then fed them entirely on bran and herbs, and twenty-five days afterwards their temperature was 42°.14 (not quite 108° Fahr.) Two other drakes, with a temperature of 41°.40, were fed abundantly on maize and hay: at the end of twenty-five days of such diet their temperature was 41°.76"—that is to say, actually *less* than those which had been ill fed! This may seem to be in contradiction with M. Martins' previous observation on the well-fed and ill-fed ducks; but the contradiction is only superficial: the reader will notice that, although the temperature of these well-fed drakes was actually less than that of the ill-fed, it was also less when the experiment began; and if we compare the rise in the temperature which took place in both, we shall find that in the ill-fed it was only 0°.30, and in the well-fed

0°.36. This difference, slight as it may seem, is in favour of the well-fed; and when such slight elevations are continued month after month, they may, and will, attain a superiority amounting to one degree. Although, therefore, this experiment confirms the previous observation of the influence of generous food in elevating the degree of animal heat, it strikingly discredits the notion that the food is burned as fuel in the organism.

"Food is warmth," because food furnishes the pabulum of the tissues, and warmth is evolved in the chemical changes which go forward in the formation and destruction of the tissues. But food is not fuel only, as some physiologists would have us believe. If anything is burnt, it is the tissues, not the food; our warmth comes from the organic processes which make and unmake the tissues. The proof of this is seen, not only in the foregoing experiments, but even more convincingly in the experiments on starvation which Chossat and Martins have performed. We shall not here repeat those of Chossat, because they are well known, and the results are accessible in almost every text-book; but those of Martins will be new to our readers, and may therefore briefly be indicated. He took four drakes, and submitted them to several successive periods of abstinence, separated by periods in which they were abundantly fed. They were left in a tub of water in which to bathe, and their habits were unaltered; nothing but the solid food was withheld during the days of the experiment; and in order to avoid the diurnal variations of temperature from complicating the problem, the thermometer was always applied at the same hour of each day. The birds commenced the period of fasting with a temperature of 42°.20 (108° Fahrenheit), at the close of a period of ample nourishment.

After 24 hours' abstinence the temperature *sank* to 41°.84.
„ 48 „ „ „ *rose* to 41°.89.
„ 72 „ „ „ *rose* to 41°.91.
„ 96 „ „ „ *rose* to 41°.94.
„ 120 „ „ „ *sank* to 41°.62.

Here we see that twenty-four hours' fast have produced a striking reduction of temperature; and those writers who attribute warmth to the combustion of food may fancy they see evidence for their opinion in such a fact; but, as the fast is prolonged, the temperature does not continue falling; it rises: so that, after ninety-six hours of complete abstinence, the temperature has risen nearly to what it was when the animal was crammed with food. We have only to add, that this is in perfect accordance with the observations of Chossat on pigeons. It is true that, after the fifth day, the temperature suddenly sinks; but the mere increase, as the abstinence is prolonged during the first four days, is sufficient to show that animal heat is not evolved by the combustion of food.

Having examined the influence of Age, Sex, and Food, we may now cast a glance at the influence of the Seasons. Although man preserves his standard of 98° in the tropics and in the arctic zone, he does so in virtue of the power his organism possesses of adjusting itself to changing circumstances. We adjust ourselves to the changing seasons. In winter we are as warm as in summer, because in winter we produce more heat, and lose less by evaporation and exhalation. A cold day in summer is incomparably more unpleasant and injurious than a day of equal temperature in autumn; and the coldest day in summer would be mild to us in winter. The reason is, that in summer the cold day finds us unprepared. The organism during summer has been adjusting itself to the production of less and less heat, and if a cold day now occur, we have less power of resistance; we are somewhat in the condition of the infant animal, which has not yet acquired its full power of heat-making. It is on this principle that we may explain the death of animals exposed during summer to a degree of cold which in winter would scarcely lower their temperature.

We are not all blessed with the same capacity for rapidly developing heat; we are not all blessed with the same activity of the circulation. Yet each is apt to make himself the standard. B. shivers, and complains of the cold; thinks he must have the fire lighted, though it be June. C. is amazed that any one can possibly be cold on such a day; C. is quite warm. Perhaps, after reading these pages, B. will learn to understand that it is quite possible for C. to be comfortable in this temperature; and C. will learn to sympathise with the less fortunate B.'s, who shiver when he is warm. The differences may arise from two causes:

the heat-producing capacity may be less, or the circulation feebler. The stimulus of the external cold increases the activity of the organic processes in one man, and depresses it in another. That this is the real cause, will appear on examining the influence of cold on the various classes of warm-blooded animals. One class—the hybernaters—is so incapable of resisting cold by an adequate increase of its own temperature, that it falls into a torpor; other classes are forced to seek external warmth in nests and holes, as we seek it in warm clothing and heated rooms; others, again, need nothing but their own temperature. In spite of the active respiration of a mouse, it needs a warm nest, and, unless in active exercise, will perish if exposed to a temperature which we should consider moderate: we, again, should perish in a temperature which the cat or dog could endure without uneasiness.

Among men there are some who resemble the mouse, and others who resemble the cat. The slightest fall of temperature causes the first to put on warmer clothing, or to light the fire; at which their robuster friends are liberal in sarcastic allusions, spoken or thought, and are somewhat impatient of this "coddling." These sarcastic friends are the cats.

It is important to bear in mind, however, that this inadequate production of heat does not always translate itself by the expression of "chilliness;" the effect of cold is often totally unlike that of a chilly sensation: it produces a vague uneasiness, a feeling of depression, resulting from the lowering of the organic activity;

and many periodic forms of disease are probably connected therewith. Without positively "feeling cold," the person so affected need only enter a well-warmed apartment, to be at once aware of a reinvigorated condition.

There is a misconception respecting cold which may here be rectified. Capt. Parry tells us that with the thermometer at 55° below zero — that is to say, 87° below freezing-point,—if no wind be stirring the hands may remain uncovered for a quarter of an hour without inconvenience; whereas with a fresh breeze, even when the thermometer is at zero, few persons can keep the hands exposed without pain. The fact is familiar to all, that wind makes the bitterness of a winter day; and that even in summer an east wind is bitter and sometimes painful. But why is it so? Why are our sensations at variance with the thermometer? Why do we *feel* the cold to be greater on a windy day? The reason is that we *are* colder; we *feel* the cold to be greater because our loss of heat is more rapid. The air is a bad conductor, and unless it be blowing upon us, thus continually bringing fresh air in contact with our warm bodies, we produce heat fast enough to replace what is carried off; but when the air carries off heat faster than we can produce it,—when the evaporation from our skin is more rapid than the replacement, then we have a continuous sensation of cold, because then our skin really is cold. A dry wind, like the east wind, is cold because it promotes a rapid evaporation; a moist wind, like the west wind, is warm because its moisture retards evaporation.

Dr Watson, in his Lectures, gives a very useful caution against the readiness with which we may confound the effects of long exposure to cold with those of intoxication. "There is too much reason to believe that poor wretches, who have been picked up by the constables in the streets at night during periods of hard frost, have been supposed to be drunk, when in truth they were only stupefied by cold."*

When is cold dangerous? It is always dangerous when continuous: a dash of cold water, or of cold air, produces a reaction; but a continued chilliness will produce disease. There is a danger in wet feet, but this danger, as every one knows, does not reach the sportsman, or the pedestrian, unless he sit still with wet feet; it is the chilliness which comes on when evaporation is not compensated by production of warmth, that causes the evil. Again, it is dangerous to drink cold water after violent exercise; but not at all dangerous to drink it during the exercise, before the body has begun to cool. The organism can withstand the most sudden alternations of heat and cold, but they must be sudden, and the organism must be in vigour. A Russian heats himself in a vapour bath and immediately plunges into snow. But if he were to wait some minutes after quitting the vapour bath, and allow his body to begin cooling, the snow bath would probably kill him.

---

* WATSON: *Practice of Physic*, i. 91.

THEORY OF ANIMAL HEAT.—It will be necessary to devote a few paragraphs to that theory of "Why we are warm, and how we keep so," which has obtained general acceptance. There is indeed a powerful and ever-increasing minority which rejects that theory, and as this minority seems to me to have the best of the argument, I will first state what the theory is, and then briefly criticise it.

Animal Heat, it is said, is the effect of which Respiration is the cause. In Respiration, oxygen is absorbed, which burns the carbon of the food into carbonic acid, and the hydrogen into water; in these acts of oxidation, heat is generated, for no combination of a combustible substance with oxygen can take place without disengaging heat. No matter whether such oxidations take place in the body or out of it, rapidly or slowly, at a low temperature or at a high one, the amount of heat set free by the combination of a given quantity of oxygen with a given quantity of carbon or hydrogen, is always and everywhere the same. The oxidation of the carbon of the food will liberate precisely as much heat as if, instead of being spread over a long time, the combustion had taken place in a vessel of pure oxygen. Chemistry assures us of these facts. Physiology assures us that oxygen is incessantly absorbed in the lungs, and that carbonic acid and water are as incessantly exhaled; and further assures us that, concurrently with this absorption of oxygen and exhalation of carbonic acid and water, there is an amount of heat generated which would be generated by an equivalent combustion of carbon and hydrogen out of the organism. "It is obvious," says Liebig, "that the amount of heat liberated must increase or diminish with the quantity of oxygen introduced in equal times by respiration. Those animals, therefore, which respire frequently, and consequently consume much oxygen, possess a higher temperature than others, which, with a body of equal size to be heated, take into the system less oxygen."

Such is the so-called "chemical theory of Animal Heat." We have already seen how little confidence is to be given to the notion of the food being burnt in the organism; and when, therefore, we hear "the carbon of the food" spoken of as passing into carbonic acid, and disengaging heat, we must, if we accept the theory, declare that what is burnt is the carbon of the tissues made from that food. The oxygen which is absorbed in the lungs does not then and there combine with carbon in the blood, and generate its due amount of heat; this, which was formerly believed, is now given up by all competent physiologists. In giving up this idea, we must follow the course of the oxygen in the blood, until we detect it, *flagrante delicto*, in the act of burning the carbon; but this has hitherto escaped all research. *We are in utter ignorance as to the origin of carbonic acid in the organism.* We have many plausible explanations as to how it *may* arise, but how it *does* arise we do not know. It is extremely doubtful, according to Robin and Verdeil, whether *any* direct oxidation of carbon takes place at all, and is quite certain that much of the carbonic acid is not so produced.[*] Without venturing further on ground so delicate, we will sum up in the words of the distinguished chemist Regnault, who has specially studied this question: "It was long believed (and many chemists still believe it) that the heat produced by an animal in a given time is precisely equal to that which would be produced by the burning in oxygen of the same amount of carbon and hydrogen which is found in the carbonic acid and water exhaled in that time. It is very probable that animal heat is entirely produced *by the chemical actions which take place in the organism,* but the phenomenon is too complex to admit of our calculating it according to the quantity of oxygen consumed."[†] The simple fact that the carbonic acid exhaled, at times, contains *more* oxygen than has been absorbed, although perfectly intelligible when we remember the influence of food on the exhalation of carbonic acid, is of itself enough to destroy all confidence in such calculations.

As an example of the heat which is formed in the organism by the processes of life, not reducible to mere oxidations, let us consider the simple fact of muscular contraction. That heat is liberated every time a muscle contracts is well known, and the experiments of Matteucci and Helmholtz show that this heat is quite

[*] ROBIN et VERDEIL: *Traité de Chimie Anatomique,* ii. 38 *seq.,* 87, 168, 462, and iii. 185 *seq.* Compare also BERNARD: *Liquides de l'Organisme,* i. 441 *seq.,* on the absence of proof that water is formed in the organism.

[†] REGNAULT: *Cours Elémentaire de Chimie,* ii. 868.

independent of any circulation of the blood. The former placed several frogs' legs in a glass and surrounded a thermometer with them; on irritating their nerves so as to produce muscular contractions, the temperature rose in the glass. Now unless it be maintained that oxidation necessarily precedes or accompanies every contraction of a muscle, it will be obvious that oxidation is not the sole source of Animal Heat; and when we reflect on the enormous amount of muscular contractions of all kinds which take place within the organism, we shall see that from this source alone a large amount of heat must arise. The same is true of all organic processes.

While, therefore, it is still undecided whether carbonic acid and water arise in the organism by a process of direct oxidation, the theory of Animal Heat, which is based on such an assumption, must necessarily be held questionable. Meanwhile we may look a little closer into the evidence which declares that Animal Heat is the direct product of Respiration, rising and falling with it, dependent on it, as effect upon cause. That a mass of evidence can be adduced is perfectly true, because, whatever theory we may form, we must still perceive that *an* intimate relation necessarily exists between Respiration and Animal Heat; if only on the ground that all vital processes are intimately related, and in the organism one function is necessarily dependent on another. The question, however, is not whether *an* intimate relation exists; but whether *the* causal relation exists, whether the two phenomena are in invariable correspondence, the one never feeble when the other is energetic—the one never acting after the other has ceased.

Disregarding the mass of evidence which may be adduced in favour of the correspondence, let us here fix our attention solely on some striking exceptions. The cases are by no means very rare in which a corpse has preserved a high temperature for many hours; and as Respiration must altogether have ceased, these cases have great significance for us. Dr Livingstone mentions a case which came under his own eye, of a Portuguese lady, who died of fever at three o'clock in the morning of the 26th April. "The heat of the body continued unabated till six o'clock, when I was called in, and I found her bosom as warm as ever I did in a living case of fever. This continued for three hours more. As I had never seen such a case in which fever-heat continued so long after death, I delayed the funeral till unmistakable symptoms of dissolution occurred." Mr George Redford informed me of a case which he had under his own eye. A soldier, given to drink, died, I forget from what cause, and the next day Mr Redford was quite startled at finding the body still warm. Dr Bennett Dowler, of New Orleans, has likewise observed that, in many cases, the temperature *rises* after death; and as these observations are cited by so eminent an authority as Professor Dunglison, we must give them a credit which might perhaps be refused to the cases previously mentioned. Dr Dowler found that where the highest temperature during life was 104° under the armpit, it rose to 109° in ten minutes after death; fifteen minutes afterwards it was 113° in an incision in the thigh; in one hour forty minutes, it was 109° in the heart. Three hours after all the viscera had been removed, an incision in the thigh showed the temperature to be 110°.

When we remember that, even after death, processes of growth and secretion have been observed to take place, there is nothing incredible in these examples of continued heat after death; but we cannot see how the advocates of the Respiration theory reconcile such facts as the complete absence of Respiration during several hours, with no diminution of Animal Heat. According to theory, the two phenomena are in immediate dependence, the intensity of heat corresponding with the energy of respiration; but here there is no respiration, nor has there been any for some hours, yet the heat continues to be produced. If it is said that the oxygen breathed during life suffices to keep up the oxidation after death; the answer is, that on this reasoning *all* bodies should continue warm many hours after death.

There are, moreover, numerous facts which show a similar want of correspondence between the energy of respiration and the intensity of heat. In tetanus, for example, the temperature has been known to rise to 110°—an amazing height; yet no corresponding increase of respiration is noted. In women the energy of Respiration is strikingly inferior to that in men; according to Barral, 40 per cent less. Yet, although they "burn" so much less carbon than men, their temperature is scarcely lower, if lower at all. We lay more stress on this fact, because it is the expression of the normal condition of the organism. In all cases of disease there is a possibility of some totally new conditions which render our inferences inapplicable; but in the natural breathing of ordinary men and women we may expect to see the unobstructed action of the law which connects Respiration with Animal Heat. According to theory, women ought to have a very much lower temperature than men, for they exhale very much less carbonic acid in respiration, and must therefore "burn" less carbon. According to fact, women have as high a temperature as men. It looks plausible when we read that the amount of heat liberated must increase or diminish with the quantity of oxygen introduced in equal times by respiration; yet this plausibility becomes troubled when we find Animal Heat sometimes bearing no such relation to the amount of inspired oxygen. The woman is as warm as the man, with feebler respiration.

When we take a general survey of the animal kingdom, the correspondence between energetic respiration and high temperature is very striking, and affords that evidence to which allusion was made just now in favour of the current theory. The cold-blooded animals are all feeble breathers, and the most energetic breathers are the warmest-blooded. A mollusc, a fish, a frog, a quadruped, and a bird, represent the various stages of this correspondence. The absorption of oxygen is smallest in the mollusc, and greatest in the bird. The mollusc has the temperature of the medium in which it lives, or is so slightly raised above it that our ordinary instruments detect no elevation. The bird keeps a constant temperature of 110°.

So long as we content ourselves with such generalities, the evidence is ample. Indeed, we might, *a priori*, conclude that a general correspondence would necessarily be observed, because of the general connection which two organic functions always exhibit; but no sooner do we descend from generals to particulars —no sooner do we compare animals with each other, than the correspondence suddenly ceases to lend its aid to the theory. It is true, as a general fact, that birds have a higher temperature and more energetic respiration than quadrupeds. It is true, moreover, as a general fact, that in birds the highest temperature is found in those of the most energetic respiration, the active hawk or swallow being warmer than the barn-door fowl; but this is true only as a general fact: and if we continue comparing birds together, we shall find that the active predatory petrel has uniformly a much *lower* temperature than the domestic duck. Here the correspondence suddenly fails. "If we bear in mind the current ideas respecting the production of animal heat," says Brown-Séquard, parenthetically, "there is assuredly something strange in the fact that the class of birds to which the petrel belongs has not a higher temperature than we find to be the case. The active life of these birds, their extreme vigour, the rapidity of their flight, their food, so rich in fatty principles, and the warm climate in which many of them live, are so many circumstances which ought to give them a high temperature." As M. Brown-Séquard does not doubt the current theory, he is inclined to attribute this discrepancy to the occasional fasts which these birds are subject to. Being, on the whole, less well fed than domestic ducks, they are therefore, he thinks, lower in temperature. This will doubtless have its influence; yet differences in the temperature of birds cannot be wholly attributed to it: for, as M. Martins finds, ducks have a higher temperature than geese. Now, inasmuch as the goose is as well fed as the duck, as well covered with feathers, and as energetic in respiration, we should, *a priori*, expect it to have a higher temperature than the duck, because it is a general law that the smaller the animal the greater the rapidity with which it parts with its heat; yet the inexorable thermometer shows the duck to have a higher temperature than the goose.

A mouse eats eight times as much food, in proportion to its size, as a man, and its respiration is, according to Valentin,* eighteen times more energetic; yet its temperature is little higher than that of man, and its power of resistance to cold is incomparably lower. Birds eat six to ten times as much as a man, in proportion to weight, respire much more vigorously, and lose less heat by evaporation; nevertheless, they are only a few degrees higher, and their power of resistance to cold is in general much less. Valentin says that a dog consumes twice as much oxygen as a man, in proportion; yet the difference in their temperature is very slight.

---

* VALENTIN: *Text-Book of Physiology* (translated by W. BRINTON), p. 351.

These illustrations suffice to show that no invariable constancy can be found between Respiration and Animal Heat: even should the theory we are criticising ultimately turn out to be correct, the objections we have urged will still retain their force, not, indeed, against the truth of the theory, but against its inconsiderate interpretation; they will retain their value as indications of the presence of *physiological conditions*, and will show how varieties in the organism modify the operation of the general physical laws; thus removing the question of Animal Heat from the hands of the Chemist, and replacing it in the hands of the Physiologist. Treating the question as a physiological one, we are forced to consider Animal Heat as determined by the energy of two processes, one of production and another of radiation. Some organisms produce heat more rapidly than others, and some part with it more rapidly. The temperature of the organism will be determined by a balance of these processes. Insects produce heat with great rapidity; but they part with it so rapidly that their temperature is as low as that of the reptile, which produces heat slowly. The hybernating animals part with their heat more rapidly than other warm-blooded classes—part with it too rapidly for the maintenance of their necessary warmth when the external temperature falls, and thus, the balance being destroyed, they sink into the condition of cold-blooded animals. If a young bird and an adult of the same species be exposed to the same degree of cold, although they have both the same temperature at starting, the young bird will in a short period be found to have a much *lower* temperature than the adult, because its production of heat has not been sufficiently rapid to keep pace with the loss. The physiological causes which determine this rapid loss in the insect, the hedgehog, and the young bird, have yet to be investigated, and may at once be surmised to be different in each case. In the insect, the rapid loss is probably owing to the smallness of its size, and the free penetration of the air through its body. In the hedgehog and young bird, the actual loss may not be greater than in other animals of the same size; but the effect of cold on their organisms may be such as to materially retard those processes on which the production of heat depends. So clearly is the production of heat regulated by the general condition of the organism, that, at different seasons of the year, the same organism will produce different amounts of heat at the same temperature. In winter, the organism is in its greatest heat-producing condition; in summer at its lowest. If subjected in summer to a temperature of zero, its power of resistance will be found very inadequate to such a degree of cold; whereas in winter its power of resistance is so great as to make this degree of cold perfectly endurable. The usual explanation is, that there is a greater amount of oxygen contained in a similar volume of air in winter than in summer, so that at each inspiration a greater amount of combustion is rendered possible. But this is unsatisfactory. In the first place, in winter as in summer, the temperature of the mouth and lungs is constant, and the cold air entering would be warmed before the oxygen reached the blood; so that, unless the oxygen is in a different condition in winter than in summer (as some maintain), no solid argument can rest on the difference of the temperature of the air. In the second place, the experiments of W. Edwards do not admit of being thus explained. He placed sparrows in a glass vase, the air of which was maintained at freezing-point, in the months of February and July. This air, consequently, contained the same amount of oxygen in each case: and as the apparatus was in each case the same, and the birds the same, every variation in the result would be owing to the temporary condition of the organisms. In February the temperature of the birds only fell one degree centigrade during the first hour, remaining stationary there during the two succeeding hours; whereas in July it fell more than three degrees in the first hour, and continued to fall till the close of the third hour, when it had lost as much as six degrees. This experiment by no means tallies with the proposition laid down by Liebig, that "in different climates the quantity of oxygen introduced into the system by respiration, varies according to the temperature of the external air; the quantity of oxygen inspired increases with the loss of heat by external cooling; and the quantity of carbon or hydrogen necessary to combine with this oxygen must be increased in the same ratio ;" for, on the contrary, we see here that the same temperature of the external air will at different seasons lower the temperature of the same bird *one* degree or *six* degrees. The cause cannot lie in the external air and its amount of oxygen, but in the organism, and its different conditions in winter and summer.

Has it never occurred to you, when standing beside a pond in early spring, that there was something paradoxical in the fact of frogs and toads crawling at the bottom, and never once rising to the surface to breathe? They are animals with lungs, and in summer live mostly on land, perishing, indeed, if unable to get out of the water from time to time ; yet during the whole winter, late autumn, and early spring, they pass their time under water. Puzzled by this fact, we applied to a zoologist for an explanation, and received in reply one of those explanations with which the majority of mankind are willing to be content, namely, a restatement of the fact in different language. Our own experiments and observations gave no explanation. We found, for instance, the two species of newt—land and water newts—behave very differently. Both have gills when young, and lungs in a more developed condition. When the gills of the land-newt disappear, the possibility of living under water disappears : the animal quits the water for ever, and you meet him on your staircase, while his companion the water-newt is still in the aquarium, and only occasionally thrusting his head above water. As the season advances, the water-newt also feels the need of occasionally quitting the water, and he will lie basking on the bit of stone or wood for hours together, descending into the water as the coolness of evening descends. To keep him under water for many hours in the hot weather, is to kill him. It is the same with frogs and toads; and the reason was made clear to us by the experiments of W. Edwards. He found that as long as the temperature of the water is no more than fifteen or sixteen degrees above freezing-point (47°-48°), frogs will live the whole year round without once rising to the surface. In this condition they *breathe only by the skin.* He has shown the relation which surface-respiration bears to pulmonary respiration in these animals; and he finds that the skin exhales sufficient carbonic acid, and absorbs sufficient oxygen, to supply all their needs of languid life at this temperature. No sooner does the temperature of the water rise, than the vital activity of the frog increases; and with this increase there is a greater need of oxygen, a greater production of carbonic acid—in other words, a greater energy of Respiration, for which the skin no longer suffices, the lungs are called upon to do their work; they cannot do this work in the water; and if the frog be prevented from rising to the surface, it is prevented from breathing, and it perishes. We see this in spring. The frog, or newt, occasionally rises to expire carbonic acid, and absorb oxygen by its lungs. It then dives under the surface again. As the season advances, the risings become more frequent, till in the hot weather the frog lives chiefly on land, and the newt also is forced to expose itself to the air.

These facts do not accord with the hypothesis of Animal Heat being the direct effect of Respiration; for we do not see the frogs get warmer because their Respiration has become more extensive, but their Respiration becomes more extensive because they are warmer: warmth has increased the activity of their vital functions, and has increased Respiration, which is one of these. We know how intimately dependent the vital functions are on temperature, and in Chap. VI. we saw how Respiration in the cold-blooded animals uniformly increases in energy as the external warmth increases, up to a certain point; and we are therefore able to understand how it is that a low degree of vital activity will be found coincident with a feeble respiration and low temperature, while a high degree of vital activity is coincident with energetic Respiration and a high temperature, without our being forced to admit that this coincidence implies a direct causal relation between energy of Respiration and Animal Heat.

In conclusion, we may say that the hypothesis generally adopted respecting the production of Animal Heat is very far from possessing the evidence demanded by science. It may be true; we do not think it is true; and we are persuaded that it is not proven. It rests on two pillars, the very foundations of which are insecure. The first of these is the chemical hypothesis of direct oxidation of the carbon and hydrogen. The second is the assumed invariableness of the relation between intensity of heat and energy of respiration. We are not warranted in affirming either of these propositions; all we are warranted in affirming is this: Animal Heat is evolved in various chemical and physical changes which occur in the processes of life, and is consequently in direct correspondence with their energy, rising in intensity as they become more active, and falling as they fall. We have every reason to believe that oxygen is the great *inciter* of such changes, the indispensable condition of vital activity; but we have no direct evidence that these changes are all oxidations; we have direct evidence that some of them are not oxidations, but are dependent on Respiration only as one organic process is dependent on another, and as Respiration itself is dependent on them.

END OF THE FIRST VOLUME.

2

THE

PHYSIOLOGY OF COMMON LIFE

2

THE

PHYSIOLOGY OF COMMON LIFE

BY

GEORGE HENRY LEWES

Author of "Seaside Studies," "Life of Goethe," &c.

IN TWO VOLUMES

VOL. II.

WILLIAM BLACKWOOD AND SONS
EDINBURGH AND LONDON
MDCCCLX

PREFACE.

THE object of the following Work differs from that of all other works on popular science in its attempt to meet the wants of the Student, while meeting those of the general reader, who is supposed to be wholly unacquainted with anatomy and physiology. The many excellent Treatises which exist are only suited to the advanced student; they assume a knowledge, and a facility of apprehension, which can only issue from a practical familiarity with the subjects.

The remembrance of my own wants and difficulties led me, many years ago, to the belief that the work which was really the most suitable for general culture, would also be of material assistance to the Student. Accordingly, although there are many topics, interesting and important to the physiologist, which find no place in this work, because they are without the range of general culture, I have done my best to render the exposition, of all the topics selected, on a level with the science of our day.

In pursuance of this object I have been forced to depart very widely from the practice of other popular writers, who consider themselves bound to act as "middle-men" between scientific authorities and the public, and to expound facts and doctrines as they find them. I could not adopt this easy and convenient plan. I could not bring myself to publish, on the authority of respected names, statements which I knew to be false, and opinions which I believed to be erroneous. After having laboured earnestly to get at the truth, it would have been disloyal to contribute in any way to the spread of what I believed to be error. All that I felt bound to do, was to state impartially the facts and opinions current among physiologists; and, when those opinions seemed inadmissible, to state the reasons for their rejection. There is therefore a great deal of criticism, and much original matter in this work.

It is in the chapters on the Nervous System that the greatest amount of dissent from current opinions will be found; and it is there that the reader will most probably feel the greatest difficulty in agreeing with me, especially if he be versed in the doctrines of the schools, and not very familiar with the subjects through direct observation and experiment. I can only say that the views there put forth are the result of very considerable research, embracing molluscs, bees, beetles, spiders, locusts, crabs, fishes, frogs, tritons, lizards, chickens, moles, mice, rats, cats, dogs, sheep, pigs, calves, oxen, and men.

The literature of each subject has been given with as much fulness as seemed useful; and never at secondhand, unless specially acknowledged. The deficiencies in this respect will be found greatest in the English department; the reason of which is that my purse is not long enough to command the Transactions, Journals, and Reviews in which my countrymen have recorded their labours; and I have not the advantage of commanding a public library. It has been my desire to render every one his due; but of course a great many claims have been passed over in ignorance.

March 1860.

CONTENTS OF THE SECOND VOLUME.

CHAPTER VIII.

FEELING AND THINKING.

Page

Object and scope of the inquiry—Psychology and Physiology two separate sciences—The Brain not the exclusive organ of the Mind: the two established doctrines—What is nerve force?—Structure of the Nervous System—Are nerves conductors only, and what do they conduct?—Error of likening nerves to the conducting-wires of a battery—Independent action of nerves—Neurility the property of a nerve—Sensibility the property of a nerve-centre—The distinction between property and function—Refutation of the supposed distinction between sensory and motor nerves—Where is the Sensorium?—Opinions of elder writers; modern opinions—Proof that Sensibility depends on histological, not on morphological structure, and that all nerve-centres possess it—Sensibility, Sensation, and Consciousness—Ambiguities of language: different meanings of "Consciousness"—Can there be sensations of which we are unconscious?—Sensation and Perception—Unperceived sensations—The Law of sensibility—Reflex actions and Reflex feelings—Sensations unattended to in reverie and sleep—Illustrations of sleepers being conscious of sensations—General consciousness: Systemic Sensations: the feeling of existence—Systemic-consciousness, Sense-consciousness, and Thought-consciousness—Note to Chapter VIII., ... 1-80

CHAPTER IX.

THE MIND AND THE BRAIN.

SECTION I.—THE CEREBRUM.

Recapitulation—Prejudice respecting the Brain as the sole organ of the Mind—Three sources of our knowledge: Experiment, Disease, Comparative Anatomy—Description of the Brain—Evidence of Comparative Anatomy against the idea of the Brain as the sole seat of sensation and

Page

will—Nervous systems of Mollusc and Beetle compared—Weight of the Brain—The convolutions; are they the seat of intelligence?—Functions of the Cerebrum—Effect of pressure on the Brain: curious case—The researches of Flourens—State of Animals when the Cerebrum is removed—Persistence of sensation and volition after removal of the Cerebrum—Experiments of Flourens, Bouillaud, Longet, and Dalton—Relation of the Cerebrum to the nerves—Forms of Sensibility peculiar to the Cerebrum—Intellect and Emotion—How can identical convolutions have different functions?—Phrenology—Explanation of the difficulty—Relation of the Intellect to the Senses, and of the Emotions to the Viscera—Other functions of the Cerebrum, ... 81-120

CHAPTER IX.

(*Continued.*)

THE MIND AND THE BRAIN.

SECTION II.—THE CEREBELLUM AND MEDULLA OBLONGATA.

Description of the Cerebellum—Gall's hypothesis—Experiments of Flourens to prove that the Cerebellum co-ordinates muscular movements: arguments against such an hypothesis—Flat contradictions—The experiments of Schiff—Evidence furnished by Comparative Anatomy; and by Disease—Absence of the Cerebellum not accompanied by absence of co-ordination: Combette's case—The Cerebellum as a seat of sensation and volition—Has the Cerebellum any share in intellectual operations?—Description of the Medulla oblongata—Is the Medulla distinct from the Spinal Chord?—The Medulla a seat of volition—Why it is considered a seat of sensation—Is there a "vital point?"—Experiments of Flourens contradicted by the experiments of Schiff and Brown-Séquard—Influence of the Medulla on the heart's action—Why a "shock" of grief or joy causes death—Pain the cause of death—Summary of the functions of the Brain, ... 121-150

CHAPTER IX.

(*Continued.*)

THE MIND AND THE BRAIN.

SECTION III.—THE SPINAL CHORD AND ITS FUNCTIONS.

Structure of the Spinal Chord—Imperfection of our microscopical knowledge—Difference between an organism and a mechanism: the organic mechanism is vital and sensitive—Theory of Reflex-Action: Marshall Hall, Müller, and Prochaska—Error of excluding sensation—Grainger's advocacy of the theory—Schrœder's reflex-fibres—Sensation without the

Page

brain: experiments on insects—Marshall Hall's experiments—Spontaneous motion in headless animals—Reflex-action in the Brain—Hydrophobia—Voluntary and involuntary actions—What is volitional control? —Volition dependent on sensation—Growth of a volition—Control over our thoughts—Involuntary actions becoming voluntary—Spinal Chord a sensational centre: views of earlier writers; the deductive evidence; the inductive evidence—Evidences of spontaneity and choice in decapitated animals—Two centres of sensibility established in one animal—Are walking and swimming reflex-actions?—Experiments on puppies and rabbits—Examination of the evidence against the sensibility of the chord—What share has the Spinal Chord in our actions?—Note to Chapter IX., Section III., ... 151-272

CHAPTER X.

OUR SENSES AND SENSATIONS.

How many senses have we?—Definition of terms—New classification of the sensations—ORGANIC-SENSATIONS—The Muscular Sense: proofs of its existence; experiments to show that its seat is not the skin, but the muscles—Electrical disturbance causing nervous-action — SURFACE-SENSATIONS: various sensibilities of the skin—TOUCH: tactile corpuscles not organs of touch; how we localise all our sensations: all sensations referred to the surface; feelings in amputated limbs; some parts of the skin more sensitive than others—Feeling of "pins and needles" in the leg—TASTE: organ of; savoury substances must be soluble; dare we trust our instinct in taste? after-tastes—SMELL: organ of; are the olfactory nerves the nerves of smell?—Are they nerves at all? —Example of perfect sense of smell in the absence of the olfactory nerves—Odorous substances—Variations and uses of smell—HEARING: organ of; cases of perforated tympanum; varieties in sounds; a musical ear; effect of sounds on the emotions; subjective sounds; direction of sounds, and how discriminated—SIGHT: organ of—Are images transmitted to the brain?—Description of the Retina—Is it nervous?—It is insensible to Light; vision commences in a change of temperature—Subjective vision: curious examples—Single vision with two eyes: the paradox explained—Why are objects not seen inverted?—Colour-blindness—Psychological importance of the new view of the senses, . 273-345

CHAPTER XI.

SLEEP AND DREAMS.

What is sleep?—Sleep and Death—Characteristics of sleep—Irresistible nature of the demand for sleep—Story of Dr Solander—Sleep under control—Heat and cold predisposing causes—Duration of sleep—Instances of refreshment from "forty winks"—Sleep of children and old people—Is sleep the period of repair?—Error of the ordinary opinion—Influence of sleep on the organism—A new theory of dreaming, . . . 346-374

CHAPTER XII.

THE QUALITIES WE INHERIT FROM OUR PARENTS.

Page

The contradictory statements current—Mr Buckle's rejection of the Law of Inheritance examined—The general characteristics which are inherited—Differences in families—Peculiarities inherited—Are the effects of accident transmitted?—Habits and tricks inherited—Influence of the second parent—Vices, Diseases, Idiosyncrasies inherited—Longevity inherited—Predisposition to Insanity—The respective influences of father and mother—Refutation of the notion that the father gives the animal organs, and the mother the vegetal organs—Curious examples—Children of great men—Necessary fusion of the parental influences—Perturbing causes: Atavism; Potency of the individual; Age; Sex—Summary of the evidence—Is marriage justifiable where the hereditary "taint" exists, ... 375-411

CHAPTER XIII.

LIFE AND DEATH.

The mystery of Life—Suspended animation—Is there a Vital Principle? —Examination of the arguments in favour of it—Does Life control chemical affinities?—Does Life precede Organisation?—Life an aggregate unity—What is Life?—Definitions which have been attempted—Cell-life—Life of the parts and life of the whole—The three laws of Life—Vegetal and Animal Life—Criticism of Bichât: Vegetal organs double and symmetrical; the law of intermittence not confined to animal functions—Life in relation to the external medium—Duration of Life—The four Ages—Longevity—Old Age—Death the inevitable residue of Life—Death of the parts—Death of the organism—Apparent Death—Signs of Death—Conclusion, ... 412-454

INDEX, ... 455

THE

PHYSIOLOGY OF COMMON LIFE.

CHAPTER VIII.

FEELING AND THINKING.

Object and scope of the inquiry—Psychology and Physiology two separate sciences—The Brain not the exclusive organ of the Mind: the two established doctrines—What is Nerve-force?—Structure of the Nervous System—Are nerves conductors only, and what do they conduct?—Error of likening nerves to the conducting-wires of a battery—Independent action of nerves—Neurility the property of a nerve—Sensibility the property of a nerve-centre—The distinction between property and function—Refutation of the supposed distinction between sensory and motor nerves—Where is the Sensorium?—Opinions of elder writers; modern opinions—Proof that Sensibility depends on histological, not on morphological structure, and that all nerve-centres possess it—Sensibility, Sensation, and Consciousness — Ambiguities of language: different meanings of "consciousness" — Can there be sensations of which we are unconscious?—Sensation and Perception — Unperceived sensations — The Law of Sensibility—Reflex actions and Reflex feelings—Sensations unattended to in reverie and sleep—Illustrations of sleepers being conscious of sensations—General consciousness: Systemic Sensations: the feeling of existence—Systemic-consciousness, Sense-consciousness, and Thought-consciousness.

IN the mighty web of things there are no threads more wonderful than Sensation and Thought; nor have any more constantly solicited the attention of philosophers, from the earliest dawn of speculative inquiry to the angry contests of to-day. They have been problems ever-alluring and ever-baffling: one moment the threads seemed to be within the easy grasp of an outstretched hand, only to vanish again into the inextricable confusion of tangled mystery.

FEELING and THINKING are of too profound an interest, and too closely allied with all vital phenomena, not to find a large place in the "Physiology of Common Life." But what place must we give them? How must these difficult subjects be treated? Their very depth and extent of interest oblige us to select only those aspects which fall strictly within the scope of this work. They have psychological aspects, and physiological aspects, both of great importance; but as our business here is not to discuss any but physiological problems, we must leave untouched the discussion of the many vexed questions of Psychology, and confine ourselves to what are strictly the physiological aspects of Thought and Sensation.

Psychology is the science of Mind; Physiology is the science of Life. All who recognise the former as a science, declare its aim to be the elucidation of the laws of Thought, the nature of the Soul, and its prerogatives. This science may seek—and I follow those who think it *ought* to seek—important means of investigation in the laws of Physiology; just as Physiology itself must seek important aids in Chemistry and Physics. But as an independent branch of inquiry, its results cannot be held amenable to physiological canons; their validity cannot be decided by agreement or disagreement with physiological laws. To cite an example: Psychology announces that the mind has different faculties, and that each of these faculties may have a temporary exaltation, or a temporary suspension. This fact seems established on ample evidence, and is valid in Psychology, although hitherto no *corresponding* fact in Physiology has been discovered—neither the anatomy of the brain, nor any knowledge of the brain's action, can be adduced as furnishing the *evidence;* and if Psychology were absolutely amenable to the conclusions of Physiology, we should here have to doubt one of the most indisputable of psychological facts. On the other hand, it is no less certain that the physiology of the Nervous System must be studied free from all control on the part of psychologists. If we do not prescribe conclusions for them, neither must they prescribe conclusions for us. To them we leave the laws of thought. To us they must leave the difficult task of ascertaining the relation between Sensation and the various parts of the Nervous System. After this explicit statement of the object and scope of the following chapters, I trust no psychologist will object to my conclusions if they fall short of, or run counter to his. He must consider himself out of court. Our science does not pretend to cope with the mysteries of his. Those mysteries will most probably for ever remain unsolved; meanwhile the labours of physiologists have made it possible that there should be at least a science of those vital phenomena connected with the Nervous System; and "thus," to use the fine expression of Professor Huxley, "from the region of disorderly mystery, which is the domain of ignorance, another vast province has been added to science, the realm of *orderly mystery.*"

The reader will have remarked that the Nervous System is mentioned, and not simply the Brain; although, inasmuch as the Brain is universally held to be the organ of the Mind, the sole centre of Sensation and Thought, there might seem to be no reason for our treating any other part of the Nervous System in this inquiry. My reason is simple: I do not agree in this opinion respecting the Brain as "the organ" of the mind. One of the principal conclusions to which fact and argument will direct us in these pages will be, that the Brain is only *one* organ of the Mind, and not by any means the exclusive centre of Consciousness. I am aware of the paradoxical air such a statement must have in these days; because the hypothesis that the Brain is the sole organ of Consciousness, after having been execrated as a heresy, and having had to battle through a stormy period of illiberal opposition, has now settled down among the respectable and respected conclusions of philosophy. If I venture to dispute, and disprove, this conclusion, it is because, after having for many years devoutly believed it, I have been gradually led, in the course of very various researches, not only to a different conception, but also to understand the source of the original error.

It will be understood that by the word Mind we do not designate the intellectual operations only. If the term were so restricted, there would be little objection to our calling the Brain the organ of the Mind. But the word Mind has a broader and deeper signification; it includes all Sensation, all Volition, and all Thought: it means the whole psychical Life; and this psychical Life has no one special centre, any more than the physical Life has one special centre: it belongs to the whole, and animates the whole. The Brain is a part of this whole, a noble part, and its functions are noble; but it is only the organ of special mental functions; as the liver and the lungs are organs of special bodily functions. It is a centre, a great centre, but not *the* centre. It is not the exclusive Sensorium. Its absence does not imply the absence of *all* consciousness, as I shall prove by experiment. It cannot therefore be considered as *the* organ, but only as *one* organ of the Mind.

The ancients seem to me to have been nearer the truth than the moderns; they made Mind one form of Life. Their *animus*, mind, was but a form of *anima*, soul or life.* In the Greek writers, the word soul (ψυχή) is used to express a Vital Principle. And this use of the word has become traditional; but is discarded by the physiologists and psychologists, who not only separate Mind from Life, but give Mind a special centre—the Brain.

This doctrine of the Brain is understood in two different ways by two contending schools. The one regards the Brain as the *original agent;* the other regards it as the intermediate *instrument.* According to the one, the Brain thinks as the stomach digests. According to the other, the Brain is the instrument of Thought, played upon by the Mind, as a piano is played upon by a musician. These two schools, however opposed, meet on neutral ground when they come to Physiology. It matters little which side we take: the observed phenomena are the same, whether the brain be regarded as an organ, or an instrument. A piano out of tune will yield discordant music, let the performer be never so skilful. A penny-whistle can never have the clang of a trumpet. It is obvious, therefore, that on the structure and state of the instrument will depend the quality of the sounds produced. This conclusion is the only one which concerns the physiologist; he is not bound to decide between the two contending schools; he is only bound to ascertain, if possible, what functional relation exists between Mind and Brain. As far as I have been able to see into this matter, the Brain is the most important organ in a complex apparatus of organs—the Nervous System; and this Nervous System has one general property—Consciousness. There are various special forms of Consciousness, but they are all intimately allied. Those forms of Consciousness which we class under the head of Intelligence and Emotion, are mainly, if not wholly, dependent on the Brain; but there are other forms of Consciousness besides these, there are other centres of Sensation and Volition besides the Brain.

---

* "Nunc animum atque animam dico conjuncta teneri
Inter se, atque unam naturam conficere ex se."

Lucretius, iii. 137.

The evidence on which I venture to depart so widely from the established doctrine, will be brought forward in the course of the following pages; meanwhile this explicit statement of the point of view adopted will keep the attention alert, and will also explain why the Nervous System in general, and not the Brain only, must occupy us. It is a system eminently deserving of study. By its agency we are brought into those relations with the external world which give rise to Sensation. It also regulates and co-ordinates all the processes of life. It gives unity to the wondrous multiplicity of organs and their actions, making each depend on each, and all co-operate to one end. It bestows on animals their respective rank in the scale of beings: the more perfect development of this system is the invariable index of a more complex and effective organisation, since by it a multiplicity of different organs is reduced to unity.

I. The Nature of the Nerve-force.—What is the force which acts in the nerves, and which brings various parts of the organic mechanism into union? From the earliest days of Science there have been hypotheses explaining the nature of this force. The hypothesis of "Animal Spirits" reigned undisturbed for centuries. These spirits were said to be secreted by the grey surface of the Brain; then, descending down the Spinal Chord to the nerves, they returned back the same way. When a nerve was tied, or divided, the passage of the animal spirits was prevented. This hypothesis is dethroned; but let no one insult it: erroneous it may have been, but it served its purpose as an hypothesis, and helped to explain the observed facts quite as well as any succeeding hypothesis has done. The "nervous fluid" which replaced it was not a whit more scientific; and the hypothesis that Electricity is the nerve-force, although having more evidence in its favour, is open to still more serious objections, because it inevitably tends to confuse and mislead both theory and practice. Whether nerve-force be, or be not, identical with Electricity, must for the present be considered an open question.* But we must bear in mind that even should the evidence finally turn out to be in favour of the electrical hypothesis, the physiologist would still have to consider nerve-force as something special—if it be electricity, it is electricity under certain special conditions, which give it a distinctive character, *only* found in the living nerve. It will always be nerve-force for him; as light will always be light to the student of optics, although the philosopher may prove light to be correlated with heat and motion.

For reasons presently to be adduced, it will be of great advantage to have a particular name for this nerve-force; and as I shall show that what is commonly expressed by the term "nerve-force" is really decomposable into *two* very different forces, corresponding to two very different nerve-tissues, I propose two names: *Sensibility* (already in use, although restricted to the Brain), as the property of ganglionic substance;

---

* See the arguments *pro* and *con* in LUDWIG: *Lehrbuch der Physiol.* TODD and BOWMAN: *Physiological Anatomy.* LONGET: *Traité de Physiologie.* FUNKE: *Lehrbuch der Physiol.*

and *Neurility*, as the property of nerve-fibre. If the etymology of the latter term is open to exception, the advantage of its being readily intelligible, and of going well with Sensibility and Contractility, may be a compensation. Of course, no stress is laid on this innovation: it is simply suggested as a convenient term, requisite to give precision to our language when talking of complex nerve-actions.

When a nerve-fibre is stimulated, its Neurility is called into play. If the fibre be in connection with the Brain, or Spinal Chord, the effect is a *sensation.* If the fibre be in connection with a muscle, the effect is *contraction.* If the fibre be in connection with a gland, the effect is *secretion.*

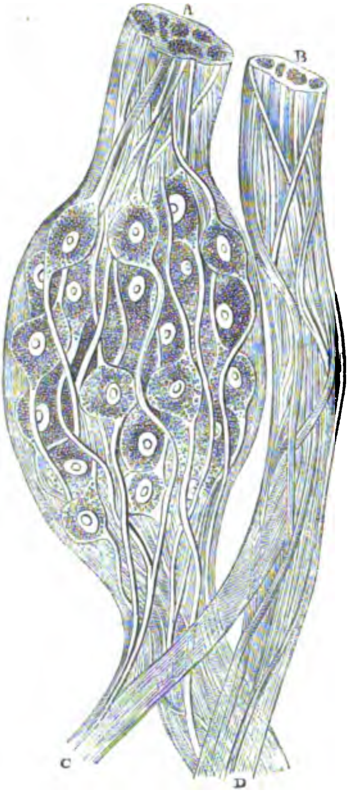
But before this can be fully appreciated, we must describe the general characters of the Nervous System.

II. STRUCTURE OF THE NERVOUS SYSTEM.—The nervous *system* is composed of, I°, nerves, and, II°, ganglia, or centres. The nervous *tissues* (for there are two) are composed of nerve-fibres and nerve-cells. Fig. 34 (on the next page) represents a spinal ganglion with nerve-trunks: the ganglion is seen to be composed chiefly of cells; the nerve-trunks of fibres.

These *nerve-fibres,* which, when arranged in bundles, and enveloped in a sheath of connective tissue, form nerves and nerve-trunks, are, strictly speaking, cylinders, containing a more or less liquid substance.

The appearances represented in fig. 35 differ from those of the fibres in fig. 34. They are, however, what is always seen under the microscope, after the tissue has been subjected to pressure. The fibres present a varicose aspect, which arises from the semi-liquid contents being displaced. The size of the fibres varies considerably.

Fig. 34.



A **Spinal Ganglion**, with its two roots—posterior A and anterior B.
(After Leydig.)

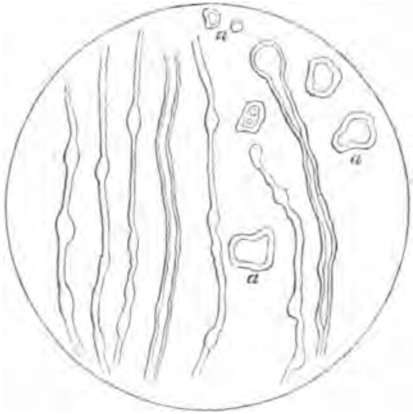
Whether nerve-fibres are *fibres*, or *tubules*,* is a question we cannot pause to consider here, since it is one which at present can have but an anatomical interest. For the sake of convenience we may speak of them as fibres. In bundles of greater or smaller calibre they form nerves; and isolated, or united, they have one constant property, which I propose to name Neurility.

The ganglia, or centres, are characterised by a very different tissue; namely, cells and granules. Of the granules I can here say nothing, except that they are too often neglected as of no account. The cells are peculiar, and often readily distinguishable from all other animal cells. In size they vary from the 300th

---

* Leeuwenhoek (*Select Works*, ii. 303) was the first to describe them as tubules; and Ehrenberg subsequently revived that opinion. It is, however, a delicate and difficult question, and turns upon

Fig. 35.

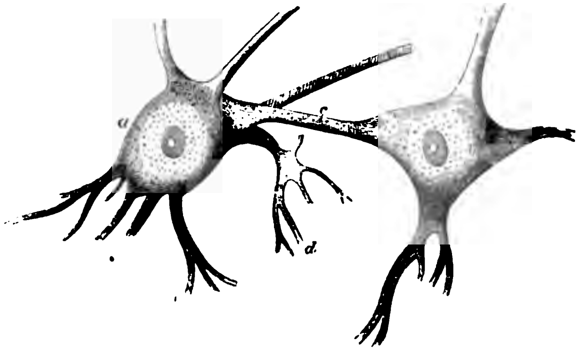


NERVE-FIBRES FROM WHITE SUBSTANCE OF THE BRAIN.
a a a the semi-liquid contents pressed out and floating as irregular drops.
(After Dalton.)

whether the thread which we find forming the central axis of the nerve-fibre is normal, or the product of coagulation. Comp. BIDDER und KUPPFER: *Untersuchungen über die Textur des Rückenmarks*, p. 28; and KÖLLIKER: *Gewebelehre*, 1859, p. 278. Indeed, on this as on most questions connected with the histology of the nervous system, the student will find an immense mass of material, the following being the most important:—KÖLLIKER: *Mikros. Anat.* and *Gewebelehre.* LEYDIG: *Lehrbuch der Histologie.* GERLACH: *Gewebelehre.* STILLING: *Ueber den Bau der Nerven-primitiv-faser.* BIDDER und KUPPFER (as above). LENHOSSEK: *Mémoire sur la Structure intime de la Moëlle épinière* in the *Annales des Sciences*, 1857, tome vii. FAIVRE: *Histologie du Système Nerveux.* TODD and BOWMAN: *Physiological Anatomy.* Unhappily everybody differs from everybody on more than one point; and the student, if sincere, will often find himself differing from them all.

to the 4000th of an inch. In shape they also vary. Here are two from the grey matter of the spinal chord. They are seen to send off prolongations of their substance in various directions, and *sometimes* one of these forms the union between two cells, as in *c*, fig. 36. It is also taught by most writers that these prolongations are the beginning, or the ending, of nerve-fibres; in other words, that the connection between fibres and cells is always of this direct kind.* In fig. 34 we have this theoretical view represented. Without disputing the statement that fibres are sometimes traced thus in connection with cells, I have to declare that, in more than a thousand preparations, I have only seen it half-a-dozen times; therefore, to make this supposed connection between cells and fibres

Fig. 36.



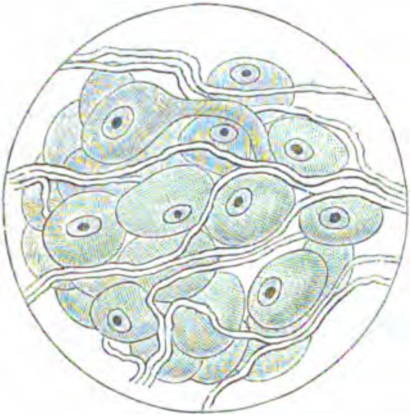
NERVE-CELLS FROM SPINAL CHORD.—(After Gratiolet.)

---

* For the literature of this subject see STILLING: *Nerven-primitiv-faser*, p. 101-3; or SCHROEDER VAN DER KOLK: *Bau und Functionen der Medulla Spinalis und Oblongata*, 1859, pp. 5 *et seq.*

the basis of any physiological inferences, will be in the highest degree incautious. Yet this is what many German and French writers are now doing. The ordinary aspect of cells and fibres in a ganglion is more like fig. 37.

Fig. 37.



GANGLIONIC STRUCTURE.—(After Dalton.)

These cells, and the granules in which they are imbedded, have a greyish colour; and it is to them that what is called the grey or "cineritious" matter of the Brain and Spinal Chord owes its name. There are, however, certain grey fibres very abundant in the sympathetic system. Nevertheless it may be understood that when the *grey matter* is spoken of, the cellular or ganglionic substance is meant; when the *white matter* is spoken of, nerve-fibres are meant.

A nerve-centre is a ganglion, or mass of ganglia fused together. It has everywhere the same property, which is Sensibility. The chief centres are congregated together within the skull and spine, forming what is called the cerebro-spinal axis. A general idea of this axis, and the principal nerves which issue from it, may be seen in fig. 38, on the opposite page.

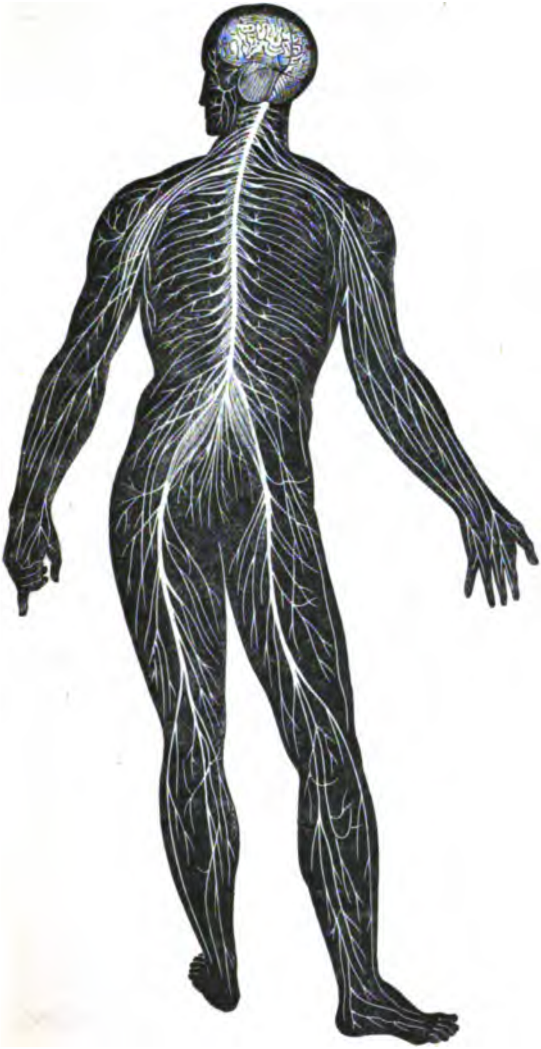
III. ARE THE NERVES CONDUCTORS ONLY? AND WHAT DO THEY CONDUCT?—Nerves are very generally likened to telegraphic wires, carrying messages to and from the centres, or to the conducting wires of a galvanic battery.* But, except as a loose and superficial analogy, this is not acceptable; and it is based on a misconception so important that we must pause a moment to consider it. The misconception is, that the centres *produce* a force which the nerves, as passive conductors, *transmit*. The analogy is to the plates of a battery producing the electricity, which the wires conduct. I think there is ample and decisive evidence to show that the nerves have a force of their *own*, the property of their tissue, which is far from being the product of nerve-centres, and is wholly *unlike* that produced by the centres.

If we cut off the leg of a frog recently killed, and

---

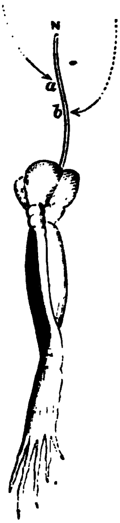
* In many cases this metaphor has been an hypothesis. The Italian anatomist Rolando claims the merit of not only having, before Sir Charles Bell, distinguished the two kinds of nerves, but also of having asserted each to be conductors of two kinds of electricity, positive and negative. ROLANDO: *Ricerche anatomiche sulla struttura del Midolo spinale*, 1824, p. 4. The conception of the Brain as a galvanic battery of which the nerves are the conducting wires, is often attributed to Sir John Herschel. It was an obvious conception when once the nerve-force was supposed to be identical with electricity; and it has been very misleading.

Fig. 38.



dissect out a portion of its sciatic nerve, as in fig. 39, N, it will be easy to show that this nerve, although separated from its centre, has still its peculiar force; for no sooner are the two poles of a galvanic battery brought in contact with this nerve (as at *a* and *b*), than the muscles of the leg are contracted, as they would be contracted during life, when the centre stimulated the nerve.

Fig. 39.



Nor must it be supposed that this contraction occurs because the nerve conducts electricity to the muscles. The same effect will follow, under proper conditions, if the nerve be simply pinched or pricked. Anything which stimulates the nerve, causes the muscles to contract. But if the nerve be *dead*, electricity fails to cause a contraction. Claude Bernard found that the *woorara* poison had the effect of destroying the irritability of the nerves, and leaving the muscles unaffected: when, therefore, he had poisoned a frog with woorara, the galvanic battery applied to the nerve no longer made the muscles contract; but applied directly to the muscles, it made them contract. Indeed, one of the earliest experiments was enough to show that the force in the nerve does not come from the centre, but is excited in the nerve itself. Willis tied the phrenic nerve, and found that the diaphragm was moved when the nerve was irritated *below* the ligature—that is, below the point where its separation from the centre was effected—but no motion could be caused in the diaphragm when the nerve was irritated *above* the ligature.

But, it may be asked, did not this nerve which we see acting when separated from a centre, *originally receive* its force from that centre? and is not the force which we excite in it, the remains of what has been produced in the centre, and accumulated in the nerve? The answer is an unequivocal, No. If the nerve were merely a reservoir, and not a spring, it could never *recover* its force after having once been exhausted; but exact experiment has determined that while a nerve separated from a centre may be so exhausted by repeated irritations, that it ceases to produce any contractions; yet if it now be allowed repose, under proper conditions, it recovers its activity, and, on being irritated, will again cause muscular contractions.* Schiff found that a frog's nerve, which had been separated from its centre, but not otherwise disturbed, retained its power of exciting contractions as long as thirteen weeks. It is therefore impossible to regard the force in the nerves as a *residue* of what was produced in the centres, seeing that the nerve itself has the power of *restoring* its wasted force.

When the conducting wire is separated from the battery it loses at once all galvanic power: it is a bit of wire, and it is nothing more. All its galvanism came from the battery, and this it could only conduct, not create. But the nerve, when separated from its centre, still retains its force: any irritation of such a nerve will excite that force, just as the stimulus from its centre would excite it. The battery is removed, and lo! the wire is found to be galvanic.

---

* Comp. Schiff: *Lehrbuch der Physiol.*, i. 112.

That the nerve itself, even during absolute repose, is the workshop of a development of force, of chemical and electrical changes, has been demonstrated by Dubois-Reymond. That the nerve, so long as it is *living*, so long as the necessary nutritive changes can be kept up, has a property not derived from the centre, but belonging exclusively to the nerve itself, is no less demonstrable. Without implying any hypothesis as to the nature of this force, I have called it Neurility (in the sense of *excitability*, but without the misleading suggestions of that word). Numerous physiologists have noticed the facts which prove that the nerves are something more than passive conductors, but I am not aware that any one has seen the full significance of these facts, and has proposed to connect them with a property inherent in nerve-fibre, and essentially differing from the property inherent in ganglionic substance, or nerve-cells. In a paper read at the meeting of the British Association in Aberdeen* I suggested the desirableness of a new nomenclature to replace the very lax expressions now in use; and as an example, the old term "nerve-force," which confounds together the activity of the nerves and that of the centres, was shown to need two distinct substitutes, one expressing the activity of the nerves—*Neurility;* and one expressing the activity of the centres—*Sensibility.*

---

* *On the Necessity of a Reform in Nerve-Physiology.*

Neurility simply means the property which the nerve-fibre has, when stimulated, of exciting *contraction* in a muscle, *secretion* in a gland, and *sensation* in a ganglionic centre.

IV. WHAT IS SENSIBILITY?—This very interesting and important question has become involved in a cloud of obscurities which would vanish if that strictness of nomenclature, to which I have just alluded, were adopted in physiology, as in chemistry and physics. The philosophic reader will learn with surprise that many eminent men are at present disputing whether the seat of sensation is itself sensitive, whether the seat of intelligence is also the seat of consciousness, and whether there are not separate parts of the spinal chord devoted to the *transmission of sensibility*—as if Sensibility were a thing to be carried from one part of the body to another!

It is found that cutting, pinching, tearing, or galvanising the grey matter of the brain and spinal chord produces no sensation whatever. The fact is doubtless surprising at first. We know that the brain is a seat of sensation, that impressions made on the nerves going to the cerebro-spinal centres produce no sensation unless those centres are affected; yet we see a man or an animal submit without the slightest wincing to have a slice of his brain cut off, or the grey matter of his spinal chord pricked and cut. The explanation is easy.

Sensibility is the inherent property of the ganglionic tissue forming the grey matter of the nerve-centres. This property is stimulated into activity by the Neurility of the nerves, but is *not* stimulated by those mechanical irritations which suffice to stimulate the nerves. The pinch, or prick, which awakens the activity of the nerve-fibre, leaves the nerve-cell dormant. You are not surprised if the pistol hangs fire when the percussion-cap is removed, or spoiled. In vain the hammer falls, no explosion takes place; because, to produce an explosion of the gunpowder, a spark is necessary, and that spark is received from the percussion-cap. It is the same with Sensibility. The hammer first acts on the cap; the cap does not transmit the *blow*, it produces a spark, and the spark makes the powder explode; the irritation acts on the nerve, the nerve does not transmit this irritation, it has its own Neurility excited, and *this* awakens the Sensibility of the centre—when the nerve goes to a centre; when it goes to a muscle it awakens Contractility.

It is absolutely indispensable that we should bear distinctly in mind this fundamental position: that Sensibility is the *property inherent in ganglionic tissue*—the one peculiar "force" belonging to all nerve-centres, as Neurility belongs to all nerves. But in order to a right comprehension of this position, as well as to a philosophical investigation of the many problems which assail us, it is necessary that we make another reform in our phraseology, and cease to confound the idea of *property* with that of *function*.

The property of a nerve, for instance, is that which belongs to it as a nerve, and not that which arises from its connection with any other organ: it depends on *structure,* as the elasticity of steel depends on the structure of steel, or the transparency of crystal depends on the structure of the crystal.

The function of a nerve is very different: it is the *use* to which that property may be applied; and depends on the connections established between the nerve and other parts.

Property is invariable so long as the structure remains.* Functions are as variable as the varying nature of organic connections.

These principles are elementary. They lead at once to a most important axiom: *Identity of structure everywhere implies identity of properties.*

No matter how different the *uses* to which a given substance, say iron, may be applied, it always and everywhere retains its properties *as* iron: we may fashion it into nails, anchors, windlasses, anvils, or cannon, and get very different results; but the properties of iron remain persistent through all these differences of use. This, you will perhaps remark, is a truism. It is so; and yet I do not recall a single writer on the nervous system who has not, on important occasions, entirely overlooked it. Two examples will suffice to show this. Physiologists have established a distinction between sensory and motor nerves, which distinction many of them refer explicitly, and others implicitly, to a difference in the *properties* of these nerves : one set of nerves are said to have the property of conducting sensitive impressions, and the other set of conducting motor stimuli—one having a centripetal and the other a centrifugal conductibility. We shall presently examine the validity of this supposed distinction ; for the present it is enough to say that although both nerves are identical in all fundamental characters —although their structure is identical—physiologists have no misgiving in attributing to them different properties ; which is like saying that of two bars of iron forming a crank, one is capable of being rendered magnetic, and the other not.

---

* Under identical conditions, of course; the same substance under different conditions will manifest very different properties. Hence allotropism.

The second example is more directly to our present purpose. Sensibility is ascribed to the ganglionic substance of the Brain, or some portion of the Brain, and denied to other masses of ganglionic substance, absolutely identical in all the fundamental characters. Nevertheless no physiologist, to my knowledge, has been aware of this violation of a first principle. No one has seen that to rescue this opinion from such a contradiction he must prove the ganglia themselves to have a structural distinction, corresponding with this supposed distinction of property. Nor could this be proved. The contrary is demonstrable. There is no fundamental difference : similar cells, similar granules, similar fibres, and similar connective tissue are found in the one and in the other : nothing is found in the one that is not in the other: they are identical. The differences, such as they are, are *morphological*, not *histological*—differences of form, not of *structure*—and it will hereafter appear that these accessory differences do not affect fundamental characters.

It is simple logic, therefore, to conclude that there being one *common tissue*, there must be one *common property*, in Brain, Medulla, and Chord, however various the *functions*, or uses, to which this property may in each case be applied. Experiment clearly verifies what logic thus deductively concludes, namely, that the Spinal Chord is in all animals a seat of Sensibility; and in some animals the all-important seat. This will be demonstrated in a future chapter. The only escape from the logic of this argument is to deny that Sensibility is a property at all; and to call it the function of certain nerve-centres. But against such an issue, two obstacles are placed: First, if we refuse to consider Sensibility as the property of ganglionic tissue, we shall have to declare what *is* the property of that tissue, manifest wherever the tissue is in activity, and common to all nerve-centres; Secondly, I shall hereafter show that all the phenomena we indicate by the term Sensibility are exhibited when those nerve-centres, generally supposed to be its exclusive organs, are removed.*

---

* Until the reader has considered this experimental evidence which proves Sensibility to belong to all the centres, I must ask him to suspend judgment on this important point. Indeed I may say, once for all, that inasmuch as the new views advocated in these chapters belong to a system related in all its parts, they cannot always be judged without reference to the whole.

Contractility is the property of muscular fibre; and however various the uses, or functions, of the muscles—extensors, flexors, sphincters, voluntary or involuntary—this fundamental property is found in all.

Neurility (*i.e.* the power of exciting a muscle, a gland, or a nerve-centre) is the property of the nerve-fibre; and however various the uses, or functions, which various nerves may serve—as, for example, those of sight, hearing, and taste, those of locomotion and those of secretion—the same fundamental character is found in all.

Sensibility is the property of ganglionic substance, and however various the uses, or functions, which different centres may serve—those of Respiration being very different from those of facial Expression, and these again from those of Perception, and so on—the same fundamental character is found in all.

The only known stimulus which will excite Sensibility in a centre, is that of the active nerve-fibre. By this means impressions on the skin become sensations in the centres. By this means one centre plays on another and awakens its Sensibility; an impression on the skin not only excites a sensation in the spinal chord, but runs up the brain, and there excites another sensation. While Sensibility seems capable of being awakened only by this one stimulus, Neurility seems capable of being awakened by a variety of stimuli; and among these we must reckon the activity of a centre, for no sooner is a centre excited, than the nerve-fibres in connection with it are excited also.

V. ARE THERE TWO KINDS OF NERVE: SENSORY AND MOTOR?—Before proceeding further into the interesting question of Sensibility, it is necessary to notice another application of those logical canons which ought to direct our investigations, but which here also have been strangely overlooked.

It is the universal opinion of physiologists, that there are two distinct kinds of nerve, named respectively the sensory and motor, the one being capable of exciting sensations, but no motions; the other of exciting motions, but no sensations. This doctrine dates from Hippocrates and Galen, and has received continual support from the frequent cases of paralysis in which there was a loss of sensibility in a limb without loss of motion, or a loss of motion without loss of sensibility. A terrible case came to my knowledge some years ago. The patient had a complete, though temporary, paralysis of motion, while preserving his sensibility unaffected. He lay motionless, and had the horror of hearing with painful distinctness all that was said about his condition, and of feeling with painful distinctness all the stimulants which were applied to awaken his sensibility. He was tortured in the belief that he felt nothing, because he could not by any movement, or cry, express the pain he felt. Fortunately this condition was not of long duration, and he recovered sufficiently to indicate the suffering to which he had been exposed.*

---

* In the French translation of Sir Charles Bell's *Nervous System of the Human Body* there is an interesting record of a man who from a

Early in the present century Sir Charles Bell made an anatomical discovery which has immortalised him, and which seemed to furnish the final scientific confirmation of the long-suspected distinction between the two sets of nerves. It would lead us too far were we to enter upon the history of this famous discovery; enough for the present if a brief statement of what is now the doctrine of the schools be laid before the reader.

There are nerves which, on being stimulated, excite muscular contractions: these are the *motor* nerves, and we can follow them into the very substance of the muscles, where they end, we know not how. There are nerves which, on being stimulated, excite sensations: these are the *sensory* nerves, and we can follow them into the skin, or the organs of sense. Finally, there are *mixed nerves*, which excite both contractions and sensations: but these, although their fibres are contained within a common trunk, are really two different nerves, having different *origins* in the centres, and different *endings* in the muscle and skin. For the sake of simplicity, we will confine ourselves to the sensory and motor nerves which issue from the spinal chord in thirty-one pairs, and which, shortly after their issue, unite to form trunks.

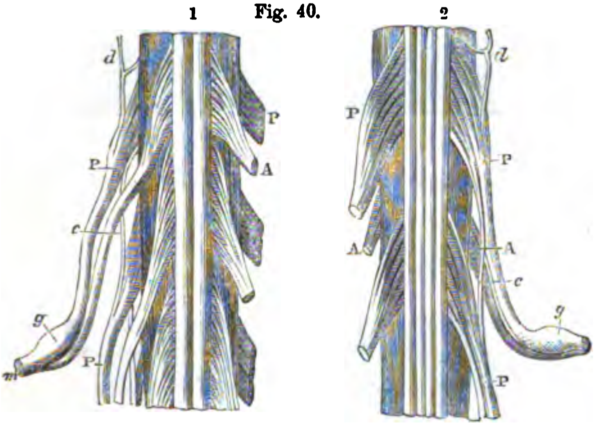
The Spinal Chord is formed of two halves, much as the cerebrum is formed of two hemispheres. Each half sends forth its own pairs of nerves; and each pair

---

fall had entirely lost his sensibility on the right side, though preserving his power of movement; and had entirely lost the power of movement on the left side, though preserving his sensibility.

has a double root—one root issuing from the *anterior* columns of the chord (in animals the *under* part), and the other root from the *posterior* columns (in animals the *upper* part). In fig. 40 the anterior and posterior columns are represented with two pairs of roots. The anterior root A is seen to join the posterior P just below the ganglion *g*, after which the two become one

Fig. 40.

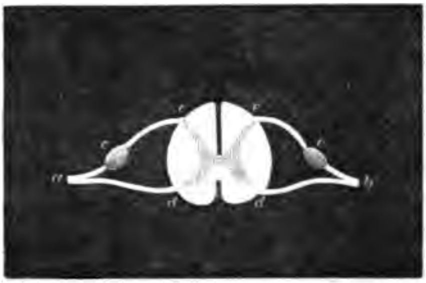


SPINAL NERVES AND SPINAL CHORD.—(After Bernard.)

1, Anterior view; 2, posterior view. A is the anterior root of the nerve, and P the posterior root. One pair in each figure is represented with the division made below the ganglion (*g*). The other pairs are divided above the ganglion. At *c* and *d* are filaments connecting two posterior roots.

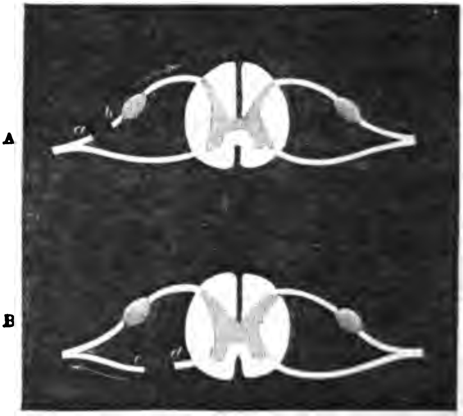
trunk, to divide and subdivide like the branches of a tree. The following diagram of a *transverse* section through the Chord will render the arrangement more intelligible. These posterior and anterior roots (fig. 41, *e* and *d*) are called respectively the sensory and motor roots. It is maintained that the fibres furnished by the one root have the power of transmitting impressions only *to* the Brain, whereas the fibres furnished by the other root have the power of transmitting impressions *from* the Brain to the muscles: the posterior root is *afferent*, and awakens sensation, but never motion; the anterior root is *efferent*, and awakens muscular contraction, but never sensation. The assumed proof of this important position is as follows: If the posterior root be divided (as at *a b*, fig 42, A) before the union with the other

Fig. 41.



TRANSVERSE SECTION OF SPINAL CHORD WITH NERVE-ROOTS.
(After Dalton.)

Fig. 42.



root, no amount of irritation of the cut end, *a*, will produce muscular contraction, but the slightest irritation of the cut end, *b*, will produce sensation. This shows that the posterior root is the channel for sensitive impressions, and not for muscular contractions.

If, on the other hand, the anterior root be divided (as at *c d*, fig. 42, B), the reverse is noticed—the cut end, *d*, may be irritated without producing the slightest sign of sensibility, whereas irritation of the cut end, *c*, is instantly followed by contraction of the muscles to which the nerve is distributed. This shows that the anterior root is the channel for muscular stimuli but not for sensitive impressions.

It was incumbent on me to expound the views held by all the physiologists of our day respecting the two kinds of nerves. Having discharged that duty to the public, there is another which remains. I believe that the view just expounded is erroneous, founded on a misconception, and in open contradiction with first principles. In two papers read before the Aberdeen Meeting of the British Association,* I pointed out the source of the misconception, and the evidence which destroyed the supposed distinction.

The anatomical discovery of Sir Charles Bell is now unimpeachable.† As far as demonstration can go, it has been demonstrated that the anterior nerves move the muscles to which they are distributed; and the posterior nerves convey sensory impressions from the skin, to which they are distributed. The opponents have long been silenced. Europe is convinced. But this discovery has been supposed to imply a physiological distinction respecting the properties, or functions, of the two nerves; and against this interpretation Professor Arnold, of Zurich, many years ago raised his voice, declaring that Bell and his followers had made the initial mistake of confounding *muscle*-nerves with *motor*-nerves.* But Arnold's voice was lost in the chorus of Bell's followers. Indeed, neither the state of anatomical knowledge, nor the possession of a general doctrine, permitted him to give his arguments and experiments their full value; and I have not met with a single physiologist who has embraced his views. Nevertheless I think Arnold was right; and having reached the same conclusion by a different route, I feel it a duty not to shrink from opposition to the teachers who most claim respect.

---

* *On the Supposed Distinction between Sensory and Motor Nerves;* and, *A Demonstration of the Muscular Sense.*

† While these sheets are passing through the press, M. AMÉDÉ PICHOT has published the history of this discovery in his brief but very agreeable biography: *Sir Charles Bell: Histoire de sa Vie et de ses Travaux.*

Referring to the distinction already established between property and function (p. 21), our first effort must be to ascertain whether the supposed difference between the two nerves is one of property, or one of function. It cannot be one of property, because the structure of the two nerves is identical,† and identity of structure, we know, implies identity of property. Moreover, it has been proved experimentally by Dubois-Reymond and Schiff, that both nerves conduct in both directions.

---

* Arnold: *Ueber die Verrichtung der Wurzeln der Rückenmarksnerven*, 1844.

† It is unnecessary here to enter on the detailed proof. I have done

Is the difference one of function? There are doubtless many who never believed it to be one of property, but conceived that it arose simply from the fact that the anterior nerves played upon muscles only, and therefore only awakened Contractility; and that the posterior nerves played upon the centre only, and therefore only awakened Sensibility. This was the opinion I formerly held. But if this has been the opinion held by some physiologists, they have not given it precise and scientific statement—they have not urged the necessity of founding the idea of function on *anatomical distribution*. Had they done so, Anatomy would speedily have convinced them, as it convinced me, of the error of regarding the nerves as essentially distinct in function.

so in the paper read before the British Association, and will only add that SCHIFF rests the structural distinction on the fact shown by his experiments, that although nerves which have been divided grow together again, a motor-nerve will not unite with a sensory nerve. The fact is valuable; but as we are at present utterly in the dark respecting the nutrition of nerves, we can found no conclusion as to their properties on any differences they may exhibit in nutrition. SCHIFF has himself proved that motor nerves conduct *both* ways—centripetally and centrifugally; and for my purpose that is all the proof necessary. The want of some recognised doctrine could alone permit so eminent a man as KÖLLIKER to write as follows: "In respect of nerve-fibres, Anatomy is unable to discover any difference between sensory and motor; but this can in nowise furnish Physiology with a ground for supposing that the nerves have similar functions!"—*Gewebelehre*, 1859, p. 352.

Messrs Todd and Bowman, in their valuable work, remark that it is not necessary to suppose any *intrinsic* difference of structure in the nerves capable of producing effects so manifestly different. "The actions of a nerve depend upon the nature of its *central and peripheral connections*." * Very true. But what does Anatomy teach us respecting these connections? It teaches that as regards the *centre*, the two nerves *agree;* and so far they must agree in function. It further teaches that as regards the *periphery*, the two nerves *differ;* and so far they must differ in function. Both are directly connected with the ganglionic substance of the Spinal Chord, and both must therefore have a similar functional relation to it: but as one is directly connected with the muscles, and the other with the skin, their functional relations to muscle and skin will of course be different. Nothing can be plainer; yet by an oversight which will one day appear astounding, physiologists have one and all disregarded the very important fact that both anterior and posterior nerves are similarly connected with the Spinal Chord, and consequently must exercise a similar influence on it.† Surely if one nerve can excite the ganglionic centre with which it is connected, a similar nerve, similarly connected, must also similarly excite that centre? To assume that of two nerves having the same property and similar connections, one only should exercise that property, and the other be utterly powerless, is a license of imagination which Logic must reprove.

---

* TODD and BOWMAN: *Physiol. Anat.*, i. 231.

† Thus SCHROEDER VAN DER KOLK, in his important work, maintains that the Medulla Oblongata is the seat of Perception, because *there* the sensory fibres terminate in nerve-cells; yet he denies that the motor fibres, which *also* terminate in nerve-cells (according to him), can have any sensory function. See *Bau und Functionen der Medulla Spinalis* in several places. He also explicitly says that the grey matter of the Chord is only concerned with co-ordinating movements; whereas the same grey matter in the Medulla Oblongata is sensitive!—pp. 73, 83.

Observe, I say the relation is *similar*, not the *same*. It requires but a moderate acquaintance with microscopic anatomy to be aware that the anterior and posterior roots differ in their distribution over the Spinal Chord; indeed, it is partly upon this difference of distribution that I explain to myself the different *forms of sensibility* excited by each root. But underlying this diversity there is a fundamental agreement: both are in direct connection with the ganglionic substance, and both must excite its activity. Hence they may be called similar, though not the same. The form of sensibility excited by the anterior root is as unlike the form of sensibility excited by the posterior root, as the sensation of Sound is unlike the sensation of Light, which are nevertheless similar, in being both sensations.

Thus does Anatomy, philosophically interpreted, teach the sensory function of both nerves. The same aids will make clear to us wherein the nerves differ. It has already been stated that the difference in their peripheral connections necessarily brings about a corresponding difference in their functions. The function of moving a muscle is assigned to the nerves distributed to muscles; the function of conveying impressions of touch, temperature, tickling, &c., is assigned to the nerves distributed to the skin, where alone these impressions can be made. So intimately is function dependent upon anatomical distribution, that the very nerve which is sensitive to touch and temperature, if the stimulus reach it through the skin, is *not* susceptible to either (but only to pain), if the stimulus be *directly* applied to the nerve.*

The posterior nerves, being distributed to the skin, can, it is obvious, only exercise a motor function in as far as they are related to moving organs, or muscles. Now *in* the skin there are such organs, insignificant enough, it is true, and only discovered within the last few years: these are muscular fibres surrounding the hair-follicles; and it is these which the posterior nerves cause to contract. If there were no muscular fibres in the skin, the posterior nerves would have no motor function; but they would have the possibility of exercising such a function. As, however, there are such fibres, the nerves have this function—not a very energetic or striking one, perhaps, but the same in kind as that of the anterior nerves.

---

* I found that pinching or cutting one of the sensory nerves of a frog *under* the skin of the back, produced no sign of sensibility whatever; whereas pricking the skin to which this nerve was distributed immediately caused the frog to leap. VOLKMANN many years ago drew attention to the great differences in the sensibility of a nerve when irritated at its periphery, or at the trunk; and Professor SCHIFF of Berne, to whom I mentioned the experiment just alluded to, assured me that it was quite consonant with all he had observed—and few have had larger experience.

Thus does Anatomy assure us that both nerves are sensory, and both nerves motor. With this torch in our hands, let us enter the otherwise obscure path of experimental inquiry. It is unnecessary to prove the motor function of the muscle-nerves; let us inquire into their sensory function. If the muscle-nerves can excite any Sensibility at all, it must be that of what we call the Muscular Sense, by which we adjust the manifold niceties of contraction required in our movements.* In a future chapter (OUR SENSES AND SENSATIONS) this muscular sense will be examined in detail, and the proof furnished that this form of sensibility persists when all *external* stimuli fail to awaken any sensation whatever. For the present we must be content with the fact that there is a peculiar sensibility derived through the muscles, and that this must have for its channels either the posterior, or the anterior nerves. So much is beyond question. Now it has been proved by the experiments of Arnold, Brown-Séquard,† and the present writer, that the posterior nerves may be divided without destroying this muscular sensibility. Indeed Brown-Séquard divided *all* the sensory roots of the four extremities of a frog, yet not only did this frog execute its ordinary muscular adjustments, but when its nose was irritated with acid, the fore-leg was employed to rub the acid away. Perhaps you think that this frog moved its limbs by an effort of the will? To anticipate such an objection, I etherised a frog, removed its brain, and skinned it: the animal was thus rendered insensible to all *external* stimuli, and without a brain to direct its movements; yet it adjusted its muscles, and rubbed away the acid from its irritated nose (the only spot sensitive to external irritations, because the only spot which retained its skin) as perfectly as an uninjured frog would have done.*

---

* That muscles communicate with centres and awaken sensations (which can only be done through their nerves) is thus admitted by TODD and BOWMAN: "A continued or violent irritation of a motor nerve in some part of its course, causing spasm or convulsive movement of the muscles it supplies, *may be propagated along its whole length to the centre,* and may *there give rise to irritation of neighbouring fibres, whether motor or sensitive,* exciting more *convulsion and pain.*" —*Physiol. Anat.,* i. 232.

† ARNOLD: *Rückenmarksnerven,* p. 111-113. BROWN-SÉQUARD'S Lectures in the *Lancet,* 3d July 1858.

---

* I seize this opportunity to defend myself, and other experimentalists, from the charge of cruelty, in performing experiments on live animals. The question is too large to be argued here; but as a purely personal matter I may say, that all my friends know my great fondness for animals of every kind, and the almost excessive shrinking from the sight of pain which renders it unendurable for me to see animals suffering. Had it not been for the discovery of ether and chloroform, I should never have pushed my investigations to the length of experiment on live animals, where great pain could be supposed to follow. But it is now becoming almost universal, not simply out of tenderness to animals, but out of regard for accuracy, to render animals *insensible* before performing any serious operation. A frog or salamander, after being etherised, remains utterly insensible for more than two hours; and dogs leap upon the operating-table on which they have before been etherised, with as much alacrity as if they had been fed there. It is the *wounding*, and not the *wound*, which causes pain. Cut a rabbit's spine in two, and you may see him in a few minutes quietly eating, as if nothing had been done to him. That some experimenters put animals to great pain, and sometimes do so with unjustifiable thoughtlessness, is true; but cruelty is never their motive: they inflict pain, not for the sake of exercising power, but for the sake of gaining that scientific knowledge which is to lessen the pain of future men and women.

The conclusion seems irresistible, that the muscular sensibility is derived through the anterior, or muscle-nerves. What is to oppose such a conclusion? Nothing but the negative argument, which is furnished by the experiment of dividing the roots of the nerves (see p. 28). If the reader who has followed the exposition thus far will consider this experiment attentively, he will see that, instead of being the victorious experiment it is commonly believed to be, it does not touch the question at all. I admit, without reservation, the fact that, when the anterior root is irritated, the animal gives no sign of sensation. But I affirm that those who demand such a sign, demand a kind of evidence which *cannot*, in the nature of things, be given, let the anterior root be never so unequivocally sensitive. Simple logic assures us that the sensibility excited by the muscle-nerves cannot be the *same* as that excited by the skin-nerves, any more than the sensibility excited by the optic nerve can be the same as that excited by the auditory nerve. The argument I am combating would, if admitted, serve to prove that the optic nerve was not sensory; for the optic centre will not respond to odours, sounds, heat, or cold; and when it does respond to a stimulus, will only respond as a sensation of light—that is to say, its own special form of sensibility—whatever stimulus awakens it. Cut the optic nerve in two—pinch it, prick it, burn it, and you produce no pain, nothing like it, nothing but the sensation of a flash of light. Now, if we are justified in attributing muscular sensibility to the anterior nerves, it is obvious that these nerves when irritated can only excite muscular sensations, and no others. It is further obvious that the signs of such sensations must be very different from those of other sensations. Let us suppose that an irritation of the anterior root, by pricking or galvanism, *does* awaken this muscular sensibility, by what sign *could* the fact be known? The irritation produces no *pain;* it can only produce that sensation which accompanies, or precedes, adjustment of the muscles, or one of the vague but diffusive sensations which muscular sensibility contributes to the general consciousness. The direct *response* to such a sensation would be an adjustment of the muscles to which the particular nerves were distributed; but this cannot take place, because the connection between these nerves and muscles is cut off. What sign could be manifested? Evidently none at all. The negative argument, therefore, which this experiment was supposed to furnish, does not touch the question; and the positive proof that the anterior nerves minister to muscular sensations, remains unaffected.

Having proved the sensory function of the anterior nerves, it is not difficult to prove the motor function of the posterior nerves. They cannot move the muscles, simply because they are not distributed to the muscles; but they move the muscular fibres of the skin, and cause the contractions of the skin. Claude Bernard, seeing the necessity of explaining the existence of sensibility in the muscles and contractility in the skin, boldly assumed—and has stated it as if it were a fact—that the posterior nerves sent off filaments to the muscles, and the anterior nerves sent off filaments to the skin. But anatomy will not support that assertion.

There has been no collection of facts to throw light on the movements of the muscular fibres in the skin; but it is worth inquiring what share they have in the erection of quills upon the fretful porcupine, or the hairs on the back of an irritated dog. Division of the posterior roots would, I should anticipate, prevent the quills of the porcupine from rising up. But there is no experimental evidence on this subject at present.

The conclusions to which we are thus led are as follows: There is no fundamental distinction between the two nerves; both are sensory, and both are motor; but they are so in different degrees. They are, strictly speaking, distinguishable as muscle-nerves and skin-nerves: the muscle-nerves being the channels for muscular-sensations and muscular-movements; the skin-nerves being the channels for skin-sensations and skin-movements. But inasmuch as the muscle-nerves have an energetic motor function, far surpassing that of the others, the name of *motor* may continue to designate the anterior nerves; and further, as the skin-nerves are channels of more *intense* and more *various* sensations than the muscle-nerves, the name of *sensory* may continue to designate the posterior nerves—it being understood that these are merely verbal distinctions employed for convenience.*

---

* Some experimenters have endeavoured to prove that one half of the grey mattor of the Spinal Chord is devoted to motion, and the other half to sensation; but the decisive experiments of SCHIFF have

We have now gone over the chief points needful to be remembered respecting the structure and properties of the Nervous System, and it is hoped the reader's patience has not been too severely taxed. To have passed more lightly over these points would have rendered our future progress less secure, and we should have been less able to grapple with those questions of more general interest, which cluster round FEELING and THINKING. Something like a scientific basis has been laid down; let us now see to the superstructure.

VI. WHERE IS THE SENSORIUM?—The Sensorium means the seat of sensation, or consciousness. The term *sensorium commune* means that there is one particular seat for all sensations whatever. Where is that important seat?

According to the principles laid down in these pages, Sensibility is the fundamental property of ganglionic tissue, and inherent in this living tissue, as Contractility is inherent in the living muscular tissue. It is therefore clear that we must make the Sensorium, or seat of Sensibility, coextensive with the nervous centres.

This is by no means the opinion of Physiologists. They aver that there is only one centre, or one group of centres, which is endowed with Sensibility. As they have no logical basis for this position, we need not be surprised to find them by no means agreed respecting the centre, or centres, which are thus arbi-

---

shown that both halves are motor and both sensory; and I regard this as a final proof of the double function of each nerve-root connected with each half.

trarily selected for the Sensorium. It seems a matter of fancy with the majority; and as one man's fancy is as good, or bad, as another's, we find one stoutly affirming that the Sensorium is *not* where another positively places it. Boutekoe, Lancisi, and others, placed it in the *corpus callosum;* why not, since the selection seems arbitrary? Willis placed it in the *corpora striata;* Descartes in the *pineal gland;* Vieussens in the *centrum ovale;* Boerhaave in the boundary-line of the grey and white substance; Mayer in the *medulla oblongata,* and Camper in the *pineal gland, nates, and testes.* They might just as well have placed it in the jawbone or eyebrows, as far as any scientific warrant can be given for one of these opinions. And if their modern successors have been less palpably absurd, they seem almost equally to have wanted any guiding principle.

Much more philosophical was the conception of Whytt. "As the schoolmen," he says, "supposed the Deity to exist in every *ubi,* but not in *any place,* which is to say in Latin that he exists *everywhere,* but in English *nowhere;* so they imagined the soul of man not to occupy space, but to exist in an indivisible point. Yet whoever considers the structure and phenomena of the animal frame, will soon be convinced that the soul is not confined to an indivisible point, but must be present at one and the same time, if not in all parts of the body, yet at least wherever the nerves have their origin; *i.e.,* it must be at least diffused along a great part of the brain and spinal marrow."*

---

* WHYTT: *Essay on the Vital and other Involuntary Motions,* 1751, p. 380.

Some years later the admirable Prochaska adopted a somewhat similar view. He conceived that the Brain (*cerebrum* and *cerebellum*) was devoted to the purely intellectual operations—to the "soul-sensations," as he calls them—and the rest of the spinal axis, including the under portions of the Brain, formed the *sensorium commune*, devoted to the "body-sensations." He defined the sensorium much as Whytt did: "it is coextensive with the origin of the nerves." Like Whytt and Unzer, he rightly perceived that the sensorium must extend to the spinal chord, because decapitated animals manifest actions "which cannot take place without the consentience and intervention of the nerves; for the decapitated frog, if pricked, not only withdraws the punctured limb, but creeps and leaps, which cannot be done without the consensus of the sensorial and motor nerves, the seat of which consensus must necessarily be in the spinal chord."*

In this, as in so many other questions, Prochaska seems to me to have been very near the truth. He falls short in the want of a clear conception of the relation between property and tissue. Had he made the identity of ganglionic substance the ground for assuming an identity of property in all ganglia, his own admirable conception of the Reflex Actions would not have been displaced by the modern, and, as I think, erroneous, Reflex Theory, but would have furnished convincing evidence of the truth of his views respecting the sensorium.

---

* UNZER and PROCHASKA *On the Nervous System* (translated by Dr LAYCOCK), p. 430.

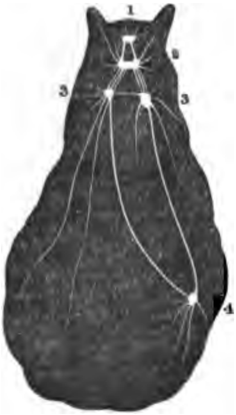
According to the view adopted in these pages, the *sensorium commune*, or general seat of Sensibility, is the sum total of all the nerve-centres; while each centre is itself a small sensorium. But although all nerve-centres agree in having one *common tissue*, and one *common property*, they differ more or less among themselves in their organic connections with other parts, and must therefore differ in their *functions*. It is the neglect of this distinction which has for so long masked the truth.

A careful inspection of the various ganglia suffices to show, that while the *arrangement* of their tissues is somewhat various, the tissues themselves remain identical. In the Brain, for example, the white matter is inside, and the grey matter outside; whereas in the Spinal Chord the reverse takes place; and in some other Ganglia the white and grey are intermingled. But such variations will not alter the fundamental properties. It may be anticipated, indeed, that men sharing the ancient prejudice respecting the Brain as the exclusive seat of sensation, will point to the different morphological structure of the Brain as a proof that its properties must be different from those of the Spinal Chord. But I think the evidence is irresistible which shows, First, that the Sensibility of the Brain depends on *histological*, not on *morphological* structure—in other words, on the tissue itself, and not on any arrangement of that tissue; and Secondly, that the Spinal Chord *is* a seat of sensation. The first point only can be touched on for the present.

It will scarcely be denied that Insects, Crustaceans, and Molluscs are endowed with Sensibility. Those who deny them every vestige of Intelligence will nevertheless admit that they can *feel.* Unless we adopt the hypothesis of Descartes, that all animals are mere machines,* we must admit that, in spite of the varieties in their nervous systems, they have one thing in common—Sensibility.

But now, what does comparative anatomy teach us? It teaches that, corresponding with this *property*, which all animals have in common, there is a *tissue* they have in common; but with this fundamental *histological* resemblance, there is a great *morphological* diversity. Nerve-tissue is everywhere similar; but the arrangements of that tissue are very various. In the simplest form of the Nervous System yet discovered—namely, that of the *Ascidian* (an animal shaped like a flask with two short necks, remaining for ever fixed on one spot)—it consists of one ganglion and a few nerve-threads. Higher in the scale we meet with a mollusc such as the *Aplysia* (sea-hare), having five ganglionic masses (fig. 43).

Fig. 43.



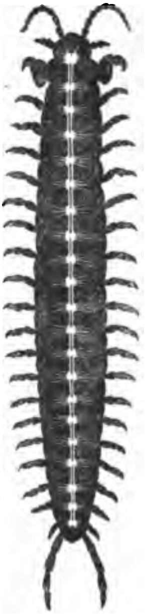
NERVOUS SYSTEM OF APLYSIA.
(After Dalton.)

A still higher development, and one approaching the vertebrate form, is seen in fig. 44, where the ganglia are arranged symmetrically along the axis of the body. These *forms* are, it is obvious, very unlike those of man or vertebrate animals; yet if we examine the *tissue* of these ganglia, we shall find it to be as much like the ganglionic tissue of men, as the muscles of these animals are like the muscles of men. From this resemblance in structure we conclude that there is a corresponding resemblance in property; and finding that insects and molluscs manifest signs of Sensibility, we have no difficulty in assigning Sensibility to this very structure.

---

* I shall hereafter endeavour to show that although the animal organism is in many respects a mechanism, it is always a *sensitive* mechanism.

Fig. 44.



NERVOUS SYSTEM OF CENTIPEDE. (After Dalton.)

The ganglia placed in the head of an insect are, morphologically, utterly unlike the Brain of a vertebrate animal; nevertheless having similar *organic relations* —that is to say, being in connection with the higher organs of Sense—we have no hesitation in speaking of the brain of a Bee. What amount of intelligence the bee may possess we do not know; but we see that it has Sensibility and Volition; and anatomists generally regard the ganglia connected with the organs of Sense as the seat of this Sensibility and Volition. For my present purpose it is enough to take note of this admission. If the cephalic ganglia of an insect can be credited with Sensibility and Volition, in spite of their morphological unlikeness to the Brain, and in virtue of their possessing a similar tissue, and similar organic connections; surely the ganglia of the Spinal Chord may be credited with Sensibility and Volition, in spite of their morphological unlikeness to the Brain, seeing how very closely they resemble the Brain in tissue?

Let no one suppose that the mere position of the Brain and cephalic ganglia, endows them with this property of Sensibility. Properties do not originate in anatomical positions. Unless the Sensibility were present, no connection with the organs of Sense would avail. In experiments on the Sensorial Centres of Insects (not yet published), I found that the ganglia styled "Brain" are in no respect different from the other ganglia in structure or in property; the only differences observable arise from the organs of Sense, or Motion, with which they are connected: *i.e.* their *functions* differ. And we shall hereafter see that, in man and vertebrate animals, the differences in the functions of the various organs of the nervous system are all forms of Sensibility, determined by their different connections with other parts of the body.

The view adopted by modern physiologists is very different; but the facts remain the same, whichever interpretation be adopted; and I only urge mine because it is based on certain simple and indisputable principles, and makes the present chaotic condition of our knowledge a little less chaotic. The current view is this: Sensibility belongs only to the centres within the skull; all other centres have only the property of *reflecting* impressions. By this reflection of impressions is meant, that when an impression is made on a sensory nerve, and by it carried to the spinal chord, the impression there becomes *reflected* into a motion—the motor-nerve carries the impulse to a muscle; and thus an action results, unprompted, or unaccompanied by any sensation whatever.

In direct opposition to this, I maintain that unless an impression on the sensory nerve excites a sensation in the centre, *no* motion whatever takes place. But the proof of this must come later on.

VII. SENSIBILITY, SENSATION, AND CONSCIOUSNESS. —We have now reached the cardinal question in nerve physiology, the clear answer to which must necessarily determine our answer to many other questions.

It is somewhat remarkable that while many physiologists might be disposed to concede that all nervous centres were endowed with Sensibility, they would almost unanimously reject the inevitable consequence, that all nervous centres, in action, give rise to *Sensation,* and thus furnish elements to the general *Consciousness.* They have no difficulty in admitting that Contraction is the active state of Contractility in a muscle; but that Sensation should be the active state of Sensibility in a nerve-centre, does not seem to them so clear. Guided in their use of language by the deep-rooted prejudice respecting the Brain, they establish a wide distinction between "sensitive impressions" and "sensations," and hence they find no difficulty in speaking of "unfelt sensations," and of "unconscious sensibility." It is not with a view to accuracy in language only that I suggest the desirableness of getting rid of such terms; no impression can be sensitive without sensation. Let us see if greater accuracy of language will not help us in our inquiry. It is maintained on all hands that sensation is impossible without consciousness. Let this be granted. But when Consciousness itself comes to be spoken of, we find that the Brain is regarded as its exclusive seat; and the conclusion of course is that no sensation can be produced by an impression, unless that impression reach the Brain. If an animal without its Brain is shown to exhibit every sign of sensation, exhibited when the Brain was present, the physiologists argue that we must not suppose the brainless animal really *has* sensations; far from it: it has only *sensitive impressions* which produce *reflex actions,* without any Consciousness on the part of the animal. To have sensations and to be conscious of sensations, is one and the same thing. To *have* a sensation and to *know* that we have it, are two things, not one thing. Knowledge cannot exist without consciousness; but consciousness may, and often does, exist without knowledge.

But what is meant by Consciousness? Unhappily there are scarcely two writers who precisely agree in their use of this term.* Some use it as the synonym of the soul; others as a distinct faculty. It is sometimes employed to designate sensation, and at others only those sensations which usurp the attention. All we can attempt in this place is to fix the precise meaning of our terms, and stick to that; and as Consciousness more generally implies sensation than anything else, we will elect that meaning, and see if by it we cannot clear up manifold contradictions.

---

* Mr Bain fills no less than six large pages with an enumeration of the various meanings given to it by English writers. See "The Emotions and the Will," p. 599.

The fact that we have a sensitive organism is indisputable; not less so the fact that this organism is incessantly excited by external and internal stimuli. Every such excitation of the sensitive organism must be a sensation. These sensations will necessarily be very various, as the organs excited, and the exciting causes are various; but they must all be sensations, they are all active states of the general property of sensibility. *Ergo*, they must all be elements of Consciousness.

The established doctrine affirms, or implies, that only those excitations of sensibility which are sufficiently vivid to predominate among the myriads fleeting over our organism, and thereby to attract attention, are properly called sensations. Of these we are said to be conscious. The rest are considered as non-existent—unconscious impressions, which may lead to actions, but are not sensations.

It is of course permissible to define terms as you please. Writers may call that, and that only, a sensation which separately solicits the attention; they may declare that unless it is "perceived," no sensation can exist. So long as this language is rigorously preserved, it may be unobjectionable. But it cannot long be preserved. Insensibly writers are led by it into the glaring contradictions of unfelt feelings and unconscious consciousness.* For example, the chest expands and contracts in respiration; and, if we attend to it, a peculiar sensation is perceived accompanying the process; but if attention be elsewhere directed, the sensation is not perceived. Now we know that in both cases a sensory stimulus, playing on the respiratory centre, was reflected as a motor stimulus on the muscles, and we are therefore forced to adopt one of two alternatives:

Either, the same sensation was evoked in both cases, although *perceived* only in the first;

Or, Attention is itself the creator of the sensation. This latter alternative is too purely metaphysical for the physiologist, who in vain endeavours to conceive the Attention *endowing* a sensory nerve with such a quality.†

On this point let us hear Müller: "Sensation itself must be distinguished from attention (the direction of the mind to sensations), and from the faculty of forming ideas from sensations. Attention appears to be a function of the cerebral hemispheres; by their removal the animal is rendered stupid, but sensation remains. Among a certain number of simultaneous sensations we are able to direct our attention to a single one so as to perceive it not only more distinctly than the rest, but definedly and in its whole intensity."* We may also quote the remarks of Gerdy on the popular confusion of terms. "If," he says, "it is necessary that there should be a perception for the existence of a sensation, it will follow that the words *sense* and *sensitive* have a double meaning, the one *patent*—to wit, that the organs indicated are sentient; the other *occult*—to wit, that the organs are not sentient in themselves, but only in conjunction with perception. Whence it results that the senses are not sentient, because they do not perceive; and the Brain alone has this privilege, because it perceives although it is itself insensible." †

---

* "The subject has been mystified by Arnold and others, by discussions about sensations with, and without, consciousness, as if the latter phrase were not a contradiction in terms; and even Professor Alison writes in the following manner—'In order that these sensations may be felt;' as if the phrase were not one of perfect tautology."—MARSHALL HALL: *New Memoir on the Nervous System*, p. 35.

† The eminent physiologist LUDWIG unequivocally declares that we only receive a sensation so long as we attend to it.—*Lehrbuch der Physiologie*, 1858, i. 593; and many others might be cited to the same effect.

Every one will admit that *attention does not create the sensation*. All it can do is to *isolate* one sensation from the *crowd of simultaneous sensations;* nay, one may ask whether attention itself be anything more than that very isolation of one sensation from the crowd? Sir Henry Holland suggests that attention is only the direction of the consciousness; ‡ but leaving this matter to the psychologists, let us content ourselves with discriminating the elements of sensation from those of perception. Sir William Hamilton, whose multifarious learning embraced even the recondite questions of physiology, has announced the law: That a nervous point yields a sensation felt as locally distinct in proportion as it is isolated in its action from every other point.* This law of local distinction must be extended to every distinction of sensations: only when one impression is isolated from the several simultaneous impressions, can it so far arrest attention as to be perceived. Let an example be cited:

---

* MÜLLER: *Physiol.;* by BALY, i. 829.

† GERDY: *Physiologie philosophique des Sensations*, p. 20.

‡ HOLLAND: *Chapters on Mental Physiology.*

During the performance of an orchestra we may single out a particular instrument, or group of instruments, following them with attention. It is obvious that in doing so we never endow these instruments with any real increase of intensity, or with any new power: the amount of sensation they awaken in us is not exalted by our attention; nevertheless, because we have isolated that sensation from the harmonious concord of sounds, amid which these instruments move, we perceive sounds which before were unperceived, and which by less experienced ears always would remain unperceived, merged in orchestral thunder. In common language we are said not to hear the particular instruments, when we cannot separately follow them. Yet nothing is more certain than that we do hear them. We are affected by them. Suddenly silence those instruments in the very climax of a crescendo, and the storm of the others will not prevent our distinctly perceiving *some* difference.

---

* HAMILTON: *Dissert. to Reid's Works,* p. 862.

We shall do well to hold fast by the maxim that to *have a sensation*, and to be *conscious* of it, are one and the same thing; but to *have a sensation*, and to *attend* to it, are two different things. Attention is the direction of the consciousness—not the consciousness itself. We shall do well also to hold fast by the distinction very properly established between Sensation and Perception. That this distinction is constantly overlooked, is the cause of much disagreement, and it is the confusion of the two terms which lies at the root of the current doctrine on the Sensorium.

Let us open Todd and Bowman's work, and read the following passage, in which these eminent physiologists express with great explicitness the view I am opposing: "A state of the sensitive organs and a corresponding perception by the mind, must concur to produce sensation: either condition may exist alone, but then the phenomenon is not a true sensation in the acceptation here given to the word. Thus light falling on the eye in sleep *excites the whole visual sensitive apparatus* while the organ of perception is inactive." *

Without denying to these writers the privilege of defining their terms as they please, I must ask what advantage there can be in blending together the two words Sensation and Perception, inasmuch as we must afterwards invent a term to express the excitement of the sensitive apparatus? This excitement is specific, and its sensational character is visible in the effects produced by it—for the sleeping man, thus excited,

---

* TODD and BOWMAN: *Physiological Anatomy*, i. 402.

perceives no object, but he *feels* the sensation, and moves away his head from the light in consequence.

It will be more exact, and more convenient, to restrict the term Sensation to the simple reaction of the Sensitive Organism.

There is, I am aware, an unfortunate ambiguity in our popular language, which renders it plausible to say that unless a sensation is perceived, it is the same as if it did not exist. Yet closer scrutiny detects that we certainly must have many sensations which are not perceived at all. Thus if two sharp instruments be made to press gently on each arm, we shall have two distinct sensations, both perceptible. Whether these two sensations are simultaneous, or successive, matters not, the two impressions on the sensitive nerves excite two sensations in their centres. But now, while the left arm retains its sensibility, if the instrument be pressed so forcibly on the right arm as to cause pain, we become totally unaware of the sensation in the left arm—we can no longer perceive the sensation caused by the instrument, which nevertheless is pressing as before. Is the sensibility in the left arm destroyed? Not at all. Does the sensation cease to exist during the period when we cease to be aware of it? No: the impression has continued; the relation of the sensitive nerve to its centre has been uninterrupted, and the sensation must consequently have continued. It will be perceived, as soon as the pain in the other arm abates.

While I am writing these lines the trees are rustling in the summer wind, the birds are twittering among the leaves, and the muffled sounds of carriages rolling over the Dresden streets reach my ear; but because the mind is occupied with trains of thought these sounds are not perceived, until one of them becomes importunate, or my relaxed attention turns towards them. Nevertheless, when unperceived, the sounds reached my ear, and excited sensory impressions: if these sensory impressions are not to be called sensations, because they were not perceived, they must have *some* name given to them, and a name which will indicate that they are affections of the sensitive organism. They were not lost; they were not altered in character because their subsequent effects were not manifest in Thought; they were not without their influence in adding to the sum of general Consciousness. It is because they were states of Sensibility that they must be called sensations.

To elucidate this we must first consider the Law of Sensibility: *No sensation terminates in itself; it must either discharge its excitation in some secondary sensation, or in some motor-impulse. Generally it does so in both together.*

A self-terminating sensation is as inconceivable as a self-terminating motion. The wave of force is propelled onwards, and for ever onwards, now in this direction, and now in that. An impression made on the skin is transmitted by the sensory nerve to the ganglion, and this ganglion, being excited, excites the motor nerve in connection with it, producing a muscular contraction. If this ganglion and pair of nerves were all the nervous system, or were isolated from the others, nothing more would ensue; the sensory stimulus would have discharged itself in a muscular contraction, and what is called a Reflex Action would have been produced. The stimulus of light, for instance, falling on the eye, causes the muscles of the iris to contract. But inasmuch as every ganglion is connected with other ganglia, and inasmuch as we have previously seen that the nerve-fibres which connect the two, have their Neurility excited when one of these ganglia is excited, the sensation awakened in the first ganglion not only discharges itself in a muscular contraction, or Reflex Action, but also in another sensation,—it plays on a muscle, and it plays on a centre. This *secondary* sensation, I have called Reflex Feeling, for the sake of marking its resemblance, as a physiological process, to Reflex Action. This secondary, or reflex, feeling may in turn discharge itself in an action, or in another feeling.

To take a rough illustration: A fly settles on your hand while you are writing. The tickling sensation may either cause you to withdraw your hand, by reflex-action, without attracting any attention to it, without exciting any reflex-feeling; or it may excite your attention, without causing you to withdraw your hand. Or, finally, it may cause you both to withdraw your hand, and to attend to it. In the first of these cases you would be said not to have felt the fly; your attention was elsewhere. But if you had not felt it, if the sensation had not been excited, no withdrawal of the hand would have followed.

We shall hereafter have to consider this point more thoroughly; enough for the present if we recognise that a sensation, once excited, *must* discharge itself in a reflex-action, or in a reflex-feeling, and *may* discharge itself in both.

In those parts of the organism which are so arranged that the easiest, readiest path for the issue of a sensation is that of muscular contraction, we shall find that every stimulus produces Reflex-Action—in other words, an action not indeed unaccompanied by sensation, but unaccompanied by *secondary* sensations, or Reflex-Feeling. The rhythmic motion of the heart, and the movements of the intestines, are examples. They do not go on without producing sensation; on the contrary, the feeling excited by them constitutes an integral part of the general Consciousness; but habitually they excite no *recognisable trains of secondary sensation;* and in popular language we are said not to feel them. Winking and Breathing are in like manner reflex-actions; but they are both liable to force double issues, and to excite reflex-feelings as well as reflex-actions,—we are then said to *feel* that we are winking or breathing. Contrasted with these, there are parts of the nervous mechanism in which the easiest path for a sensation is that of another sensation; one feeling excites, by reflex, another feeling, one idea calls up another idea; and this either with, or without, exciting a reflex-action. If a tone awakens your recollection of one bar of a particular melody, you will find it is only by an effort that you can prevent the whole of that melody from passing over your mind; and this melody may either excite reflex vocal expression, or be continued without such reflex-action.

Habits, Fixed Ideas, and what are called Automatic Actions, all depend on the tendency which a sensation has to discharge itself through the readiest channel. In learning to speak a new language, to play on a musical instrument, or to perform any unaccustomed movements, great difficulty is felt, because the channels through which each sensation has to pass have not become established; but no sooner has frequent repetition cut a pathway, than this difficulty vanishes; the actions become so automatic that they can be performed while the mind is otherwise engaged; and sometimes, if once commenced, they must continue. We have all our tricks of phrase or gesture, which no effort can prevent. We utter, as meaningless expletives, phrases which originally cost us trouble to learn. It is in vain that people laugh at us for the iteration of "you know," "well then," and similar phrases, which are often ludicrously inappropriate; they have become automatic—the paths of discharge have been established, and along these paths the sensation must discharge itself. The same thing is observable in the region of ideas. Old associations, old beliefs, are not to be displaced. A man may be thoroughly convinced to-day by the logic of his opponent, and yet to-morrow he will be heard uttering his old convictions, as if no one had ever doubted them. His mind cannot move except in the old paths. It may be noted as the peculiar characteristic of vigorous intellects, that their thoughts are ever finding new pathways instead of moving amid old associations. The vigorous thinker is one who thinks for himself; the vigorous writer is one who expresses what he means, and does not suffer one phrase automatically to determine another. If he has a manner, or mannerism, it is his own. Inferior minds think the thoughts of others, and write the phrases of others. Hence, as Goethe says, in this world there are so few voices and so many echoes.

Returning from this digression, we may now understand that although a sensation must discharge itself, and may do so in various directions, now exciting muscular contractions, and now trains of thought, we are not to suppose the sensation itself is dependent on these effects; or to suppose, as the dominant doctrine does, that, unless a train of thought be excited, no sensation at all has been excited. Sensation is simply the active state of Sensibility, which is the property of ganglionic tissue.

The mill-wheel, at first so obtrusive in its sound, ceases at length to excite any attention. The impressions on our auditory nerves continue; but although we hear them, we cease to think about them: the same reflex-feelings are no longer excited. It is held, indeed, that we cease to hear them, in ceasing to be "conscious" that we hear them; but this is manifestly erroneous. Let the wheel suddenly stop, and there is an immediate corresponding sensational change in us; so much so, that if it occurs during sleep, we awake.

Let the wheel move with a slower movement, and we shall at once be aware of it. Let it creak, and we hear it. Now, it is clear that unless we are all the while sensible of the sounds of the wheel, no alterations in the *degrees* of sound will affect us. If the sensation of sound has ceased, the *cessation* of that sound cannot awaken us. The truth seems to be that at first the sound of the wheel was *obtrusive*—excited reflex-feelings—gave determinate directions to our thoughts. It afterwards ceased to excite these feelings, and the sensations became *merged* in the general sum of sensations which make up our total Consciousness. "The habit of hearing the same sounds," says Jouffroy, "renders us sometimes highly sensitive to them, as in the case of savages; sometimes again almost insensible to them, as exemplified in the apathy of the Parisian or Londoner for the noise of carriages. If the effect were physical—if it depended on the body and not on the mind—there would be a contradiction, for the habit of hearing the same sound either blunts the organ or sharpens it; it could not at once have two, and two contrary, effects—it could have only one. The fact is, it neither blunts nor sharpens; the organ remains the same—the same sensations are determined; but when these sensations interest the mind, it applies itself to them, and becomes accustomed to their discrimination: when they do not interest it, it becomes accustomed to neglect, and does not discriminate them."*

---

* Quoted in HAMILTON: *Lectures on Metaphysics*, i. 328.

One cannot help the equivoques of language. It will always seem absurd to say we can have sensations and not be conscious of them, because the word consciousness has become restricted to those sensations to which the attention is directed. The explanation given by Whytt is worth attending to: "When I say we are not conscious of certain impressions made on the mind by the action of material causes on the organs of the body, I mean no more than that we have no such consciousness or perception of them, as either convinces us of their existence when present, or enables us, by the help of memory, to recall them when past." * Consciousness is here considered as identical with Perception; it is generally considered identical with Sensation. And the question recurs: Can there be sensation without perception?

Nothing is more certain than that we have many sensations which are not perceived at all, of which we are said to be wholly "unconscious." They are either so faint in themselves, or so familiar, they are either so submerged in stronger sensations, or so incapable of exciting trains of Reflex-Feeling in the preoccupied mind, that we are neither "conscious" of them when present, nor capable of remembering them afterwards. For example: it may happen that we fall asleep during a sermon (such cases have occurred), or while a book is being read aloud. The sound of the speaker's voice is heard, but the words gradually cease to be perceived. Page after page is read aloud, exciting no perception at

---

* WHYTT: *Of the Vital and Involuntary Motions,* p. 287.

all in our minds; but has there been no sensation excited? We have not *heard*, but have we not been *affected* by the sounds? To prove that we have, is easy. Let the reader suddenly cease, and if our sleep be not too profound, we at once awake. Now, unless the sound of his voice affected us, it is clear that the cessation of that sound could not have affected us. Or let us suppose our sleep to be unbroken by the cessation of the sound; even this will not prove that we have been unaffected by the sounds, it will merely prove that those sounds, or their cessation, did not awaken a reflex-feeling. For let the reader, in no louder tone, ask, "Are you asleep?" and we start up, with round eyes, declaring, "Not at all." Nay, should even this question fail to awaken us, the speaker need only utter some phrase likely to excite reflex-feeling—such as "There's the postman," or "I smell fire," and we start up.

I remember once trying the experiment on a wearied waiter, who had fallen asleep in one of the unoccupied boxes of a tavern. His arm rested on the table, and his head rested on his arm: he snored the snore of the weary, in spite of the noisy laughter and talk of the guests. I called out "Johnson," in a loud tone. It never moved him. I then called "Wilson," but he snored on. No sooner did I call "waiter," than he raised his head with a sleepy "*yessir*." Now, to suppose, in this case, that he had *no* sensation when the words "Johnson" and "Wilson" reached his ears, but had a sensation when the word "waiter" reached his ears, is to suppose that two similar causes will not produce a similar effect. The dissyllable "Johnson" would excite as potent a reaction of his Sensibility as the dissyllable "waiter:" but the secondary sensations—the reflex-feelings—were different, because the word "Johnson" was not associated in his mind with any definite actions, whereas the word "waiter" was so associated as to become an automatic impulse.*

Two sisters are asleep in the same bed, and a child cries in the next room. The sounds of these cries will give a similar stimulus to the auditory nerve of each sister, and excite a similar *primary* sensation in each. Nevertheless, the one sister sleeps on undisturbed, and is said not to hear the cry. The other springs out of bed, and attends to the child, because she being accustomed to attend on the child and soothe it when crying, the primary sensation has excited secondary sensations, or reflex-feelings, which lead to accustomed actions. Could we look into the mind of the sleeping sister, we should doubtless find that the sensation excited by the child's cry had merged itself in the general stream of Consciousness, and perhaps modified her dreams. Let her become a mother, or take on the tender duties of a mother, and her vigilance will equal that of her sister; because the cry will *then* excite a definite reflex-feeling, and a definite course of action. But this very sister, who is so sensitive to the cry of a child, will be undisturbed by a much louder noise; a dog may bark, or a heavy waggon thunder along the street, without causing her to turn in bed.*

---

* Dr Carpenter tells a similar story of Admiral Codrington, who, when a midshipman, could always be awakened from the profoundest slumber if the word "signal" were uttered; whereas no other word disturbed him.

Although during sleep the nervous centres have by no means their full activity, they are always capable of responding to a stimulus, and Sensation will always be produced. When the servant taps at your bedroom door in the morning, you are said not to hear the tap, if asleep; you do not perceive it; but the sound reaches and rouses you nevertheless, since when the second tap comes, although no louder, you distinctly recognise it. In etherised patients, Sensation is constantly observed returning before any consciousness of what is going on returns. "I was called," says Mr Potter, "to give chloroform to a lady for the extraction of ten teeth. The first five were extracted without the slightest movement, but as the operation proceeded, sensation returned, and I was obliged to use considerable force to keep her in the chair during the extraction of the last tooth. She came to herself very shortly after, and was delighted to find she had got over all her troubles without having felt it the least in the world." †

There is, I repeat, an unfortunate equivoque in language which makes it sound absurd to speak of unperceived sensations. Men have for so long confounded Sensation with Perception, because the two are so constantly blended, that it is startling to hear of one occurring without the other. But even the physiologists, whose views are here combated, fall into the same equivoque of language, and speak of "Unconscious Sensibility." In spite of all such verbal difficulties, we must steadily bear in mind that a sensation, being the active state of any one ganglion, is not dependent for its existence on the activity of any other ganglia which it may, by reflex-movement, excite. Every excitement of a nerve-centre produces a sensation; the sum total of such excitements forms the general Consciousness, or sense of existence.

---

* Compare an interesting personal example given by JOUFFROY, quoted in Sir W. HAMILTON'S *Lectures*, i. 331.

† *Lancet*, 10th July 1858.

We do not see the stars at noonday, yet they shine. We do not see the sunbeams playing among the leaves on a cloudy day, yet it is by these beams that the leaves and all other objects are visible. There is a general illumination from the sun and stars, but of this we are seldom aware, because our attention falls upon the illumined objects, brighter or darker than this general tone. There is a sort of analogy to this in the general Consciousness, which is composed of the sum of sensations excited by the incessant simultaneous action of internal and external stimuli. This forms, as it were, the daylight of our existence. We do not habitually attend to it, because attention falls on those particular sensations of pleasure or of pain, of greater or of less intensity, which usurp a prominence among the objects of the sensitive panorama. But just as we need the daylight to see the brilliant and the sombre forms of things, we need this living Consciousness to feel the pleasures and the pains of life. It is therefore as erroneous to imagine that we have no other sensations than those on which the attention falls—those which we distinctly recognise—as to imagine that we see no other light than what is reflected from the shops and equipages, the colours and splendours which arrest the eye.

The amount of light received from the stars may be small, but it is present. The greater glory of the sunlight may render this starlight inappreciable, but it does not render it inoperative. In like manner the amount of sensation received from some of the smaller ganglia may be inappreciable in the presence of more massive influences from other centres; but though inappreciable it cannot be inoperative—it must form an integer in the sum.

The reader's daily experience will assure him that over and above all the particular sensations capable of being separately recognised, there is a general stream of Sensation which constitutes his feeling of existence—the Consciousness of himself as a sensitive being. The ebullient energy which one day exalts life, and the mournful depression which the next day renders life a burden almost intolerable, are feelings not referable to any of the particular sensations; but arise from the massive yet obscure sensibilities of the viscera, which form so important a part of the general stream of Sensation. Some of these may emerge into distinct recognition. We may feel the heart beat, the intestines move, the glands secrete; anything *unusual* in their action will force itself on our attention.

"What we have been long used to," says Whytt, "we become scarcely sensible of; while things which are new, though much more trifling, and of weaker impression, affect us remarkably. Thus he who is wont to spend his time in the country is surprisingly affected, upon first coming into a populous city, with the noise and bustle which prevail there: of this, however, he becomes daily less sensible, till at length he regards it no more than they who have been used to it all their lifetime. The same seems to be the case also with what passes within our bodies. Few persons in health feel the beating of their heart, though it strikes against their ribs with considerable force every second; whereas the motion of a fly upon one's face or hands occasions a very sensible and uneasy titillation. The pulsation of the great *aorta* itself is wholly unobserved by us; yet the unusual beating of a small artery in any of the fingers becomes very remarkable."

A large amount of sensation is derived from the muscular sense, yet we are not aware of the nice adjustments of the muscles, regulated by this sensibility, when we sit or walk. No sooner are we placed in an exceptional position, as in walking on a narrow ledge, than we become distinctly aware of the effort required to preserve equilibrium. It is not the novelty of the position which has increased our sensibility; that has only caused us to attend to our sensations. In like manner, the various streams of sensation which make up our general sense of existence, separately escape notice until one of them becomes obstructed, or increases in impetuosity. When we are seated at a window, and look out at the trees and sky, we are so occupied with the aspects and the voices of external Nature, that no attention whatever is given to the fact of our own existence; yet all this while there has been a massive and diffusive sensation arising from the organic processes; and of this we become distinctly aware if we close our eyes, shut off all sounds, and abstract the sensations of touch and temperature—it is then perceived as a vast and powerful stream of sensation, belonging to none of the special Senses, but to the System as a whole. It is on this general stream that depend those well-known but indescribable states of Consciousness, named "feeling well" and "feeling ill"—the *bien être* and *malaise* of every day. Of two men looking from the same window, on the same landscape, one will be moved to unutterable sadness, yearning for the peace of death; the other will feel his soul suffused with serenity and content: the one has a gloomy background of Consciousness, into which the sensations excited by the landscape are merged; the other has a happy background of Consciousness, on which the sensations play like ripples on a sunny lake. The tone of each man's feeling is determined by the state of his general consciousness. Except in matters of pure demonstration, we are all determined towards certain conclusions as much by this general consciousness as by logic. Our philosophy, when not borrowed, is little more than the expression of our personality.

Consciousness, in the general sense, is the sum total of all our sensibilities, the confluence of many streams of sensation. Consciousness, in the particular sense, is only another term for Sensibility: we have as many different forms of consciousness as we have different kinds of sensation. While some of these confluent streams are so slight that they are no more appreciable than are the stars at noonday, others are so equable and constant, that they attract no attention unless their currents are disturbed. Thus healthy men are scarcely able to distinguish the sensations excited by the circulation of the blood, or the movement of the intestines; yet a slight alteration in these sensations will be felt at once.

We may here conclude the discussion of this question by affirming that the evidence proves the inadequacy of the current hypothesis, which says that unless a sensation is perceived it does not exist—it is a mere *impression*. The hypothesis here combated says, explicitly or implicitly, that a nerve-centre may receive a stimulus, but this stimulus will not awaken a sensation, unless it be transmitted from that centre to some *other* centre. Though why one centre should have the property of Sensibility which another centre, identical in substance, has not, no physiologist has attempted to explain.

I request the reader particularly to observe that it is no merely verbal difference which is established, when what is usually called an *impression* is here called a *sensation*, and when a *sensation* is distinguished from a *perception*. More than once has this objection been urged, the objector thinking that my dissent from established opinions amounted to no more than calling an impression by another name. My dissent reaches to the very foundations. A verbal difference, carrying with it no important consequences, would not be worth so much labour as has been bestowed on elucidating this point. Those who say that an impression which is not perceived has no element of sensation in it, say that when the preoccupied mind does not hear the roll of carriages in the street, the effect of the vibrations of the air upon the auditory centre has been no more than the effect on the window-pane: a mere physical impulse has been given. Whereas I aver that just as the window-pane reacts according to the properties of glass, so does the auditory centre react according to the properties of nerve-centres—and something more than an impression is the result: the impression is *sensitive*.

It is surely much more than a verbal difference when one school asserts that some animals have Minds, and other animals only Instincts, and another school asserts that all animals have Minds, but the minds of some are strikingly inferior in range, complexity, and energy, to those of others. It is surely more than a verbal difference when a philosophic naturalist, detecting the uniformities which underlie the diversities noticeable in the actions of animals, declares that they are all guided by sensations, and these are all due to a nervous system—in opposition to those who call the actions of some animals mental, and the *same* actions in other animals mechanical. And such is the difference between the view I advocate, and the view I combat. My view may be erroneous; at any rate it is not limited merely to a change of terms. If it be said, that since I admit a difference between sensation and perception—that is, between one sensation and another—this amounts pretty much to the ordinary distinction between an impression and a sensation; the answer is very simple: a naturalist admits the difference between two animals, admits that a monkey is very unlike an oyster, and that the two should never be confounded; but while they differ, they also agree: he classes them according to their differences, but their fundamental agreements make him range both under the general conception of an Animal. Various as animals are, he knows that underlying all diversities there are certain uniformities; all animals belong to the grand division of *Animalia*, and obey the same biological laws. Now it is precisely this reform that I desire to see introduced into our physiology of the Nervous System—that we should recognise the uniformities underlying the diversities, and recognise that all nerve-centres, *as* such, have properties in common, laws in common. The sensations derived through the organic processes are very unlike those derived through the five Senses; and Pain is unlike both; yet they are all sensations. Hunger is unlike Thirst; Thirst is unlike Fatigue; Light is unlike Hearing. But these are all elements of the general Consciousness: all active states of nerve-centres. Instead of admitting this simple principle, physiologists, implicitly or explicitly, assert that one nerve-centre or group of centres has a property of Sensibility not possessed at all by similar centres. Even those who seem to have felt some scruple in denying sensibility to all the centres, have been forced, by their adherence to the notion of a sensation being impossible without the Brain, to establish two distinct classes—namely, the Conscious Sensibility, and the Unconscious Sensibility ; in other terms, Animal Sensibility and Organic Sensibility. The latest upholder of this view thus expresses himself:—

" All the secretions take place under the influence of a reflex action, and sometimes the sensation, which is the point of departure of this action, is a conscious sensation, at other times unconscious. Thus the salivary secretion, which takes place when vinegar is introduced into the mouth, is a secretion which succeeds a conscious sensation (*une sensation avec conscience*). The secretions of gastric juice, pancreatic juice or bile, which take place when food passes along the digestive tube, on the contrary, succeed to sensations without consciousness." *

Now, physiologically, the process in each of these cases is identical : a stimulus is given to the Neurility of a nerve, which excites the Sensibility of a centre, and that, in turn, stimulating the Neurility of the nerve distributed to the gland, produces thereby a Secretion. There is a difference between the organ of Taste, stimulated by the vinegar, and the stomach or liver, stimulated by the food ; there is a difference also in the secretions : but in each case the stimulus has first awakened Sensation, and then Secretion. If the sensation of the one is to be called conscious, and the other unconscious, this use of language, though justifiable, will be excessively misleading. It would be far better to say that the sensation produced by the vinegar, being derived from a special sense (Taste), could be distinctly recognised as special; whereas the sensations excited by the food, being derived from no special sense, but from the Systemic Sensibility,* must necessarily be unlike the taste of vinegar, and not being a special sensation, is less readily recognisable.

---

* CLAUDE BERNARD : *Système Nerveux*, i. 357.

The conclusion, then, which we have reached is briefly this: In muscular tissue there is an inherent *property* named Contractility; this serves various *functions* in various muscles. All the movements of our different organs depend on this property; and the complex mechanism which performs the many movements, say of the hand, is a complex of simple muscular contractions. In like manner ganglionic tissue has an inherent property, Sensibility, which serves various functions in various organs: however complex any one of these functions, we should find, could we analyse it, that it was a complex of simple sensations. There is an incessant action and interaction of the various parts of the sensitive mechanism: sensations cross and recross, exciting and modifying each other; and the sum total is a feeling of existence. In the lower animals, with a simple nervous system, the sensitive phenomena are simple: as the organisation increases in complexity, the sensitive phenomena necessarily become more complex; the elements of general Consciousness become more numerous.

---

* This distinction between special and systemic sensations will be elucidated hereafter.

VIII. VARIOUS FORMS OF CONSCIOUSNESS.—A basis has been laid, in the foregoing paragraphs, for a true zoological classification of mental phenomena. The unity of the nervous system throughout the Animal Kingdom has been generally recognised; but, strangely enough, the unity of Consciousness has not been deduced from it. In ascending the scale we pass from animals destitute of real *movements*, and manifesting only *contractions;* as we note in the higher animals the ever-increasing variety and complexity of movement, we find that no new property has been acquired; but that the same primordial tissue, with its Contractility, suffices for all the new complex phenomena. Yet we speak, and justly, of simple contractions, of voluntary and involuntary movements, of flexion and extension, and so on. I think we should do the same with Sensibility, or Consciousness, which may be conveniently grouped under three heads: 1. *Systemic-Consciousness;* 2. *Sense-Consciousness;* 3. *Thought-Consciousness.* A few words will explain these more fully.

Systemic-Consciousness includes all those sensations which arise in the system at large, chiefly in the organic processes, and form the more massive and diffusive elements of the sense of existence. This must belong to the simplest animals, unless we adopt the hypothesis of Descartes, that all animals are living machines, whose actions are wholly without consciousness. Many who reject this hypothesis feel great difficulty in crediting *all* animals with consciousness or sensation; because few writers can rid themselves of the embarrassing equivoques of our ordinary language. The equivoque here leads them to suppose some element of Thought is included in Consciousness, and even in Sensation. But although every animal must *feel,* it does not follow that it should *think.* Every animal must *contract,* but it does not follow that it should *move.* Thinking results from a more complex organisation than is given to every animal; but Consciousness is the attribute of every animal organism. Simple as the forms of Consciousness may be in the lower animals, it must be nevertheless essentially akin to the more complex forms in the higher animals. Unless we admit this, it will be impossible to say where sensation first begins. If the Mollusc is without sensation, the Crustacean is without it. If the crab is a machine, so is the bee; if the bee, then the beaver, the elephant, the dog, the monkey. It is true that when we pass from the monkey to the man, a wide interval is obvious: the greater fulness and splendour of conscious life which man possesses above all other created beings may account for the disinclination of philosophers to admit that animals can share even a modest claim to similar prerogatives. But the time for such exclusiveness is passed: science has shown that amid the infinite varieties of animal life, certain constant uniformities exist; and that in the magnificent drama of creation, the humble scene-shifter is of the same nature as the more highly-endowed tragedian, who plays the noblest parts. Unless we are to throw science to the winds, we must admit that *all* animals are conscious (have sensations), though all have not every form of consciousness. There are those that have systemic sensations, but no senses, such as sight, hearing, smell. There are others that have systemic sensations, and sense-sensations, but little or nothing of what we mean by Thought. All depends on the complexity of the organism.

The Systemic sensations in man are massive in effect, and constant; and because they are constant and familiar, seldom rise into distinct recognition. This has led to their being denied the character of sensations. It is said, for example, that we have no sensations in the alimentary canal, unless pain be produced there. And the following proof is offered: If we swallow iced-water, there is a sensation of cold excited in the mouth, throat, and gullet; but this sensation disappears as the water reaches the stomach: if any vestige of sensation remain, it will be only at the upper part of the stomach. This is, however, no proof at all. The sensation of cold is a *special* sensation, of which, as we shall see hereafter, only certain organs are susceptible—the same nerve which is susceptible of cold when the cold substance reaches it through the skin, is *not* susceptible of cold when the trunk of the nerve is directly touched; and there is nothing remarkable in the stomach not having this special form of sensibility. But if the iced-water, on reaching the stomach, ceases to produce a sensation of cold, it does not cease to affect the sensitive nerves; it therefore produces some other sensation—one of comfort or discomfort, perhaps. We are assuredly conscious of some difference in our general feeling—a diffusive sensation radiates from the stomach. It may escape recognition, like a rivulet mingling with a river, and adding to its general momentum ; but though *unrecognised,* it is not *unfelt.*

If instead of iced-water we swallow brandy-and-water, the sensations of taste excited by the brandy, being special sensations, will cease in the stomach. In the mouth, the brandy and the water excited sensations of taste, and of cold; in the gullet, of cold without taste; in the stomach, of neither cold nor taste, but something else—a diffusive sensation quite familiar to all readers, but not easily to be described.

Hereafter we shall have to consider more in detail the various Systemic-sensations; enough for the present if we have indicated the class. The second class forms.

The Sense-Consciousness, and includes all those sensations which are derived from the organs of the five Senses. As a rule, it is only these, and Pain, which are admitted to be sensations at all, by those who fail to perceive the distinction between Sensibility and particular forms of Sensibility.

Thought-Consciousness is the third and last class, and includes all those phenomena of Thought and Emotion with which the psychologist concerns himself; all that the physiologist can do with them is to indicate the relation in which they stand to the lower forms of consciousness; and the special organs of the nervous system which serve them. I have adopted the rather clumsy term Thought-consciousness, in order to mark out distinctly the fundamental union of this with the other forms of Sensibility, and at the same time to mark the *speciality* of its phenomena. What Thought is we do not know, perhaps we never shall. We do not know what Life is. But the realm of mystery may be reduced to one of "orderly mystery;" we may learn what are the laws of Life, and what are the laws of Thought. The first is the province of the physiologist, the second is the province of the psychologist. Let us keep to our own:

> "Musicians think our souls are harmonies;
> Physicians hold that they complexions be;
> Epicures make them swarms of atomies
> Which do by chance into our bodies flee.
>
> One thinks the soul is air; another fire;
> Another blood diffused about the heart:
> Another saith the elements conspire,
> And to her essence each doth yield a part.
>
> Some think one gen'ral soul fills every brain,
> As the bright sun sheds light in every star;
> And others think the name of soul is vain,
> And that we only well-mixed bodies are.
>
> Thus these great clerks their little wisdom show,
> While with their doctrines they at hazard play." *

We will take the warning. Our task is difficult enough without extra complications. We have still to point out what light Physiology can at present furnish, in the attempt to determine the various organs for the various mental functions—in other words, what is the relation between the MIND and the BRAIN.

---

* SIR JOHN DAVIES: *On the Immortality of the Soul.*

NOTE TO CHAPTER VIII.

THE experiments of Claude Bernard and Kölliker have proved that the *woorara* poison affects the muscle-nerves, and leaves the skin-nerves unaffected, paralysing the limbs, but not destroying sensibility. At the reading of my paper on the Sensory and Motor Nerves at the "British Association," this fact was referred to as proof that a distinction existed between the properties of the two nerves. I had already satisfied myself that such a conclusion was erroneous; but it may be desirable here to refute the conclusion by one decisive argument. We are completely in the dark as to the *conditions* which determine the effect of a poison on the nerves; but that the resistance of the skin-nerves to an influence to which the muscle-nerves succumb, is no proof of a difference in the properties of the nerves, is demonstrated by the fact that only *some* of the motor-nerves are affected by the poison—and of these nerves, only their terminations in the muscles are affected, not the fibres of their trunks; whereas *other* nerves distributed to muscles resist the poison, no less decisively than the skin-nerves. An animal poisoned by woorara has its limbs paralysed; but its tail is as vigorous as ever. The nerves which move the muscles of the limbs are not different in properties from those which move the muscles of the tail; yet the one class succumbs, the other resists; evidently because there is some set of conditions which favours the action of the poison in the one case, and prevents it in the other. The nerves which move the heart and intestines are likewise unaffected by the poison. Moreover, the fact that the nerve-fibre is unaffected everywhere except at its termination, proves that there must be some other cause at work than the property of the nerve-fibre itself. A similar peculiarity is noticeable of the muscles, there being poisons which destroy the contractility of the muscles of the heart, and have no appreciable effect on the muscles of the trunk; yet no one ventures to assert that the muscles of the heart are essentially different from all other muscles.

CHAPTER IX.

THE MIND AND THE BRAIN.

SECTION I. — THE CEREBRUM.

Recapitulation—Prejudice respecting the Brain as the sole organ of the Mind—Three sources of our knowledge: Experiment, Disease, Comparative Anatomy—Description of the Brain—Evidence of Comparative Anatomy against the idea of the Brain as the sole seat of sensation and will—Nervous systems of Mollusc and Beetle compared—Weight of the Brain—The convolutions; are they the seat of intelligence?—Functions of the Cerebrum—Effect of pressure on the Brain: curious case—The researches of Flourens—State of Animals when the Cerebrum is removed—Persistence of sensation and volition after removal of the Cerebrum—Experiments of Flourens, Bouillaud, Longet, and Dalton—Relation of the Cerebrum to the nerves—Forms of Sensibility peculiar to the Cerebrum—Intellect and Emotion—How can identical convolutions have different functions?—Phrenology—Explanation of the difficulty—Relation of the Intellect to the Senses, and of the Emotions to the Viscera—Other functions of the Cerebrum.

IN the preceding chapter I have endeavoured to establish the following propositions, as the basis of a new doctrine of nervous action:—

1°. All nerves have one common property—Neurility—by means of which they excite contraction in a muscle, secretion in a gland, and sensation in a nerve-centre.

2°. The property of nerves depends on their structure. The functions, or uses, of nerves are determined by their anatomical distribution—*i.e.*, their connection with other parts of the organism.

3°. All nerve-centres have one common property—Sensibility—which is excited by, and in turn excites, the Neurility of nerves; and thus produces either reflex-movements, or reflex-feelings, according as the stimulus to the centre is reflected on muscles, or on other centres.

4°. The property of nerve-centres depends on their structure. The functions, or uses, of each centre are determined by its anatomical connections; the optic-centre being in connection with a very different apparatus from that of the auditory-centre, and so of the rest.

5°. Every stimulus which affects a centre awakens its Sensibility; but the *kind* and *degree* of sensation thus awakened are necessarily determined by the kind and degree of the exciting cause, and the structure of the organs on which that cause first acts: the sensation of sound is very different from the sensation of light; a visceral sensation is unlike a muscular sensation; pressure is different from pain, and both are different from the sensation of temperature.[*]

---

[*] The reader is requested to consider what is said at page 70. The word Sensibility includes very various sensations, as the word Animal includes very various organisms. When I say that every nerve-centre has Sensibility, I no more mean that every sensation will resemble the sensations of pain, or of sight, or of taste, than that every animal must resemble a fish, a reptile, or a mammal.

Although in the establishment of these propositions I have attempted something like a systematisation of our knowledge on this great subject, led thereto by the necessity of clearing the ground of certain prejudices and misconceptions, and by the desire of drawing the student's attention to problems which may profitably employ his energy ; the reader will not expect that in the following pages we can do more than ascertain the general relations of the Mind and the Brain. Before we enter on this interesting, but extremely complicated, inquiry, we must resolve, in a loving, seeking, earnest spirit, to keep our minds open to the reception of Fact ; and calmly to acquiesce in it, when presented, instead of opposing it by preconceptions of our own. It is idle to interrogate Nature in the spirit so often animating the disputes of men, when each antagonist is less desirous of hearing the arguments of the other, than of thrusting forward his own. If we ask Nature a question, we must listen patiently to her reply; should that reply perplex us, we must ask again, putting the question in another form ; and should again, and again, the same reply be elicited, we must accept it, be it never so destructive of our theories and anticipations. We must not dictate an answer to the Oracle we consult.

A deeply-rooted preconception respecting the Brain has for many years prevented men from duly acknowledging facts which, otherwise, would have been clear enough. Biassed thus, they have never thought of admitting the evidence of these facts to be destructive of their conception of the Brain; but have had recourse to various theories, sometimes ingenious, often absurd, to explain how these facts were really *not* what they seemed to be. The conception alluded to is thus expressed (I take the first book of authority at hand) by Todd and Bowman: "The brain, or some part of it, is the sensorium commune; or, in other words, mental nervous actions (*acts of volition and sensation*) *cannot take place without a brain.*"*

This is the doctrine of the schools. Now it should be observed that all physiologists are aware of the fact that animals without a brain (and animals which never had a brain) manifest some of the *same* actions which are attributed to sensation and volition, when the brain is present; yet these actions are said *not* to have that character when the brain is absent, "*because* the brain is the exclusive organ of sensation and volition." I confess that the simpler dictate of logic is to me the easier conception; and that I can more readily admit the facts to prove the brain *not* to be the exclusive seat of sensation and volition, than admit that actions, precisely similar, are in one case sensitive, in the other mechanical; and this inclination gains strength, when close examination reveals that the reason why the actions of brainless animals are said to be mechanical (reflex, and without sensation), is solely because theory declares the Brain to be the only sensorium. If you pinch a dog's tail, he cries out. His cry is supposed to indicate a sensation of pain. But the physiologist who would reprove you for having hurt his yelping puppy, would quietly assure you that this puppy's cries were no evidence of pain or sensation after its brain had been removed: "Merely reflex, my dear sir!" and he would smile at your supposition that an animal without a brain could feel any sensation.* Nay, even when the brainless animal performs complicated actions to rid itself of some irritating object, and exhibits a *choice* of means for this purpose, men find it easier to consider these as "instinctive" (whatever *that* may be), "reflex," or the "effect of habit," than simply to acknowledge that the Brain is not the sole sensorium.

---

* TODD and BOWMAN: *Physiol. Anat.*, i. 322.

Science is the endeavour to make the order of our ideas correspond with the order of phenomena—to make our conceptions of things accord with the order of the things themselves; not to make out a scheme *for* Nature, which shall correspond with our ideas. Let us try, therefore, to ascertain what is the order of nervous phenomena.

There are three great sources of knowledge on this subject, all of which, however, are very imperfect: these are Experiment, Pathology, and Anatomy, especially Comparative Anatomy. It has been a fertile theme for declaimers, who knew nothing of the subject they were declaiming on, to brand with odium or ridicule the attempt of anatomists to discover anything respecting mental actions. "What! with your scalpel, fit only to deal with Death, do you hope to penetrate the mysteries of Thought? When you have taken the mechanism to pieces, do you expect the watch to reveal its action; or do you fancy the Soul is less mysterious than a watch?" Feeble nonsense of this kind has imposed on men, because they have been unacquainted with the real methods of science. That we shall ever penetrate the mystery of the Soul, is improbable; equally improbable that we shall ever penetrate the mystery of Life; yet it is certain that the methods of science have enabled us to discover some of the laws of Life, and to assign to the various organs the part each plays; and this in spite of the fact that "the scalpel is fit only to deal with death." I would ask these declaimers whether they think that the action of the watch will be better understood by taking the mechanism to pieces, and studying each part, and its relations to the rest; or by assuming certain speculative postulates respecting it, and making a system out of deductions from those postulates? The Anatomist attempts to discover the mechanism; the physiologist attempts to detect the influence which each part exercises. Were our knowledge of the structure and properties complete, we should soon know all that could be known respecting the vital mechanism.

---

* Indeed, some physiologists have incurred ridicule for having been misled by the cries and struggles of a brainless animal into the belief that the animal manifested sensibility. See, for example, SCHRŒDER VAN DER KOLK: *Bau und Functionen der Medulla Spinalis*, p. 26, on the "Error shared by so many that the cries of an animal are a proof of Sensibility." The argument relied on by writers who attribute such manifestations to reflex-action, unaccompanied by sensation, rests on the fact that patients with spinal disease are often unconscious of the movements in their limbs produced by external stimulus; we shall hereafter see that this fact in no way contradicts our views, in no way warrants the reflex theory.

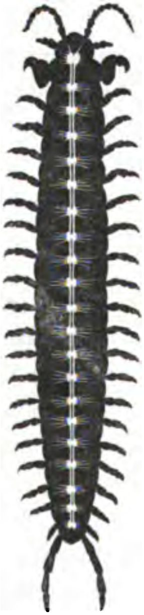
Unhappily our knowledge is very far from complete; and hence it is that Experiment, though valuable, nay, indispensable, is often very misleading. We shall see evidence of this in the course of our inquiry. But there is one direction in which Experiment may always be relied on—the direction of *exclusion*. An experiment may fail to tell us which is the organ of a particular function; but it will never fail to tell us which is *not* the organ of the particular function, if we see the function persisting after the organ has been removed; and to know what parts are *not* necessary, is to arrive, by exclusion, at those which *are*.

Observation of the effects of disease, accident, or deformity has a similar value to that of Experiment; but extreme caution is necessary in interpreting these cases. Anatomy is, of course, indispensable; and comparative anatomy becomes a sort of final standard, because it displays the nervous mechanism in all degrees of complexity, from the simplest to the highest, and thus reveals what parts are essential. Here also great caution is requisite, since we never can be quite certain in our interpretation of the actions of animals.

The BRAIN, or ENCEPHALON, includes several masses of grey and white matter. Anatomists are not agreed as to the exact limits of the brain, some including portions which others exclude; but all limitations are arbitrary, since the cerebro-spinal axis is really, a continuous whole—a series of ganglia connected together by nerve-fibres. The easiest way of arriving at an intelligible conception of this whole, is to begin by considering the somewhat similar axis in the insect, Fig. 45.

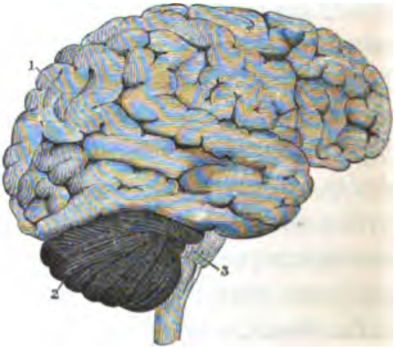
These numerous ganglia are separate; imagine them closely fused together, and enclosed by columns of fibres, and you have the Spinal Chord and Medulla Oblongata; then imagine other ganglia, and, superposed on these, the large ganglionic masses of Cerebellum and Cerebrum. The two latter are represented in Fig. 46. This is their external appearance, in profile. The *convolutions* of grey vesicular matter are outside the white fibrous matter, instead of inside, as in the Medulla Oblongata and Spinal Chord. Fig.

Fig. 45.



Nervous System of Centipede.
(After Dalton.)

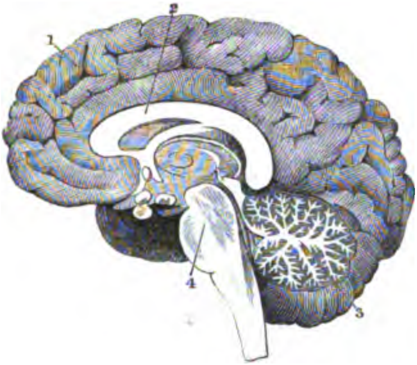
Fig. 46.



The Human Brain in Profile
1, Cerebrum; 2, Cerebellum; 3, Medulla Oblongata

47 gives the aspect of the brain when divided longitudinally.

Fig. 47.



Half of the Human Brain in Profile.

1, Convolutions of Cerebrum; 2, the Corpus Callosum, or White Band, which unites the two Hemispheres; 3, Arbor vitæ, or tree-like disposition of grey and white matter in Cerebellum; 4, Pons Varolii and Medulla Oblongata.

In the diagram on the following page we find the general arrangement and connection of the parts lucidly presented, the black masses representing the grey vesicular matter, and the white representing the white fibrous matter.

The Cerebrum, with which we must first occupy ourselves, is composed of two halves, called cerebral hemispheres, united at their base by the *corpus callosum* (Fig. 47, 2). It is in these hemispheres, or, more strictly speaking, in the grey matter of the convolutions, that most physiologists have agreed to place the seat of Thought, Emotion, Volition, Sensation—in one word, Consciousness—to the exclusion of every other ganglionic mass. Here, and nowhere else, they aver, impressions become sensations. Some physiologists, indeed, are less exclusive, and extend to the ganglia at the base of the Cerebrum—*i. e.*, to the *corpora striata, optic thalami*, and *corpora quadrigemina* (Fig. 48 — 3, 4, and 5), and to the Medulla Oblongata the privilege of sensation. The reader knows that neither of these opinions can be adopted in our pages: we hold that if the Cerebrum is the organ of what is usually named Thought, it is not the sole seat of Sensation, or Volition.

Fig. 48.

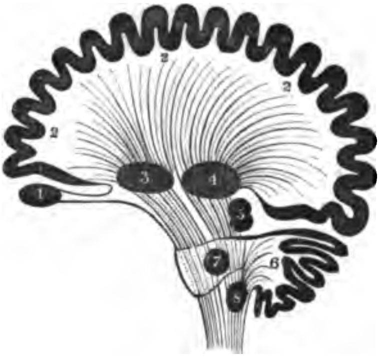


DIAGRAM OF VERTICAL SECTION OF THE BRAIN.

1, Olfactory Ganglion; 2, the Cerebral Hemisphere; 3, Corpus Striatum; 4, Optic Thalamus; 5, Tubercula quadrigemina; 6, Cerebellum; 7, Ganglion of the Pons Varolii; 8, Olivary body, or Ganglion in the Medulla Oblongata. The lines show the direction of the fibres.

(After Dalton.)

Comparative Anatomy is freely invoked when it can sustain the argument in favour of the Brain, or the Cerebrum, being the sole seat of Intelligence; but it is quietly disregarded when it flatly contradicts the idea of the Brain being the exclusive seat of Consciousness, or Sensation. We cannot allow two weights and two measures. If the evidence furnished by animals is good in one case, it is good in another. Now, what says evidence?

A survey of the vertebrate classes discloses a remarkable correspondence between the size and development of the Cerebrum, and the energy and variety of mental manifestations. As we pass from fishes and reptiles to birds, and from birds to mammals, and in mammals from the less intelligent to the more intelligent, we notice a decided increase in cerebral development. It is a legitimate inference that the one is in some correspondence with the other, and that Intelligence is one of the functions of the Cerebrum.

Let this be admitted without reservation, although the well-informed anatomist may have many difficulties to propound. I now ask, what we are to make of the fact, that multitudes of animals have *no* cerebrum at all; and that even among fishes there is at least *one* known to be without a vestige of it (and zoologists may yet discover many more); so that, unless we pronounce the Amphioxus, and all the invertebrata, to be mere machines, without sensation, or consciousness, of any kind, we are forced to admit consciousness in the absence of the very organ which is said to be its exclusive seat. There are two answers open. First, it may be said, as it commonly is, that these animals have no Intelligence, only Instinct. This may be true; but to make it of the slightest use, we must be taught what Instinct is; and that teaching is yet to seek. Instinct, like Chance, is one of the words under which men conceal their ignorance from themselves. That the actions of a Bee, or a Crab, which manifest sensation, memory, invention of new methods under new circumstances, not to mention anger, desire, and some unexplained mode of communicating with each other—that these are the actions of "blind instinct," might not be inconceivable, if we knew what instinct really was; but we shall find it difficult to conceive how precisely similar phenomena are attributed to intelligence, when displayed by the Dog or the Monkey. It is probable that the Bee and Crab have no power of forming abstract propositions. It is probable that they are unable to carry on *trains of thought, remote from the sensations which are immediately affecting them.* And if we limit the term intelligence to those processes by which one idea calls up another, without the intervention of an external stimulus—to *trains* of ideas, in lieu of single ideas excited by Reflex-Feeling—there may be little objection to saying that Bees have not intelligence.

As our desire must be to come to a distinct understanding on this important point, and not to quarrel over phrases, let us, for a moment, grant that no invertebrate animal has intelligence, in any sense in which it may have pleased men to employ the term. Let Instinct explain everything, without itself needing explanation. It will not remove an iota of the objection against the assumption that the Cerebrum is the exclusive seat of sensation and volition. The Bee may have no vestige of intelligence, but you cannot deny that it has sensibility and volition. The brainless Amphioxus may be a very stupid fish, indeed; but you will hardly assert that he wants the consciousness, the sensibility, of other fishes. If you grant me this, dispute is at an end. You merely say that the Cerebrum has certain special functions, among which Intelligence is one; you do not thereby exclude from other ganglia other forms of Sensibility.

So much for the first of the two answers which might be made to our original difficulty. The second answer will run thus: "It is true that the Bee manifests a certain degree of consciousness, perhaps even of intelligence; but it does so because the ganglia encircling the œsophagus, although not the same as the brain, *represent* the brain in the insect. The two organs are *analogous*, not *homologous;* just as the wings of the bee are analogous to the wings of the bird, but are not the same organs."

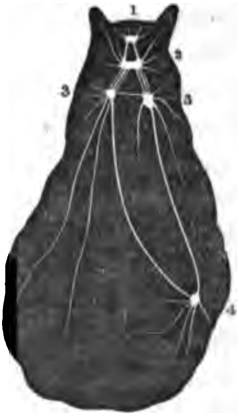
It will scarcely be denied that I have placed the strongest argument which the case admits into the mouth of my antagonist. To answer it we have only to consider steadily the relation which function bears to property and structure. Function, as we formerly saw (vol. ii. p. 21), is the *use* to which certain organs are applied, in virtue of the properties inherent in the structure of those organs. The wings of the bee and the bird are organs moved by muscles, under the stimulus of nerves, so as to beat with a light, expanded surface on the air, and thus make a lever to raise and sustain the body. There are several fundamental resemblances in the two wings; and as these imply similarity in properties, there need only be similarity in the anatomical distribution, to produce a similarity in the uses of the wings. The point to which I wish attention directed is that analogy of structure and connection which determines the analogy of function. Without the moving muscles, the wing would be no organ of flight; without the Contractility of the muscle-fibre, the muscles could not move; without the stimulus and co-ordination of the nervous system, the muscle-fibres could not contract harmoniously, so as to produce this flying movement. It is because the two wings resemble each other in such particulars, that, in spite of their many differences, the wing of the bee and the wing of the bird are both organs of flight.

In the same way the brain of a bee is analogous to the brain of a bird; there are many and important differences, but there are fundamental resemblances of structure and connection, of property and function. It is because they are both formed of ganglionic substance that they have both the property of Sensibility. It is because they are both connected with the organs of special Sense, and are the chief centres with which directly, or indirectly, all the nerves are connected, that they both have the supreme function of what has been aptly termed *Cerebration*.

This is the teaching of Comparative Anatomy, and its lesson is valuable. If it shows us how the cephalic ganglia of an insect may represent the Brain of a vertebrate animal, and thus *seem* to justify the doctrine of the brain being the exclusive seat of consciousness; it also, and by the same evidence, shows that Sensibility must belong to *all* ganglia, as ganglia, and not to any special group. It shows that the Brain of a vertebrate animal does not derive its functions from any arrangement of its tissues, nor from the possession of cerebral hemispheres, nor from any other morphological character: since these functions are performed (the chief of them at least) by ganglia having no other resemblance to the Brain than fundamental similarity of tissue, and similarity of organic connection. The Brain derives its Sensibility from its ganglionic structure, *in which it resembles all other ganglia.* It derives its functions from the various organs to which this Sensibility is made subservient by anatomical connection.

This point may be illustrated in a comparison of the organisation of a mollusc and an insect. Fig. 49 shows the nervous system of a Sea-Hare (*Aplysia*) in its organic relations. The ganglia of this mollusc are large enough to serve many functions, if they were connected with various organs. But the body of the Sea-Hare is not divided into segments, each segment having different organs: it is one large contractile mass, having no legs to walk with, no wings to fly with, no claws to grasp with; it crawls slowly along by means of its large fleshy under-surface, called "the foot." Having so few means of acting on the external world, there can be little *combination* of actions, little of the multifarious instinct shown by the garden-beetle, (Fig. 50.)

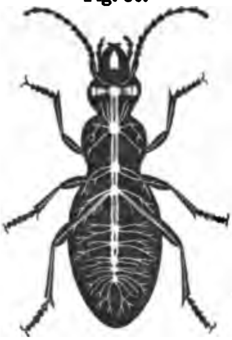
Fig. 49.



NERVOUS SYSTEM OF APLYSIA.
(After Dalton.)

Here we see an organism made up of a number of *different* organs, each organ having different uses; consequently its nervous system has more various functions. Could we measure the nerve-force developed by the shapeless Sea-Hare, and the tiny but complex Beetle, we should doubtless find that in *amount* the superiority of the former was manifest, although its *functional* character was lower.

Fig. 50.



NERVOUS SYSTEM OF GARDEN BEETLE.
(After Milne-Edwards.)

I conceive, therefore, that Comparative Anatomy irresistibly disproves the notion of the Brain, or any other ganglionic mass, being the sole and exclusive seat of Sensibility or Consciousness; and we shall hereafter see still stronger evidence in the observations of experimentalists and pathologists. No one will misunderstand these remarks to mean that the Brain is not one great centre of Sensation and Volition—the chief and dominant organ of the whole psychical mechanism. I have said before that it has the noblest functions; but it does not exclude the other ganglia from their share in the general Consciousness. In it all the sensations derived through the Senses and Viscera are summed up, combined, modified, and in some profoundly mysterious manner elaborated into ideas. It is generalissimo of the whole army, controlling, directing, and inspiring the actions of all subordinate officers. But to suppose that these subordinates have not also their independent functions, is a mistake. The generals, colonels, captains, sergeants, corporals, and common soldiers, are individual men, like their commander-in-chief, with inferior power, and with different functions, according to their respective positions. But if the commander-in-chief be killed, the army has still its generals. If the generals be killed, the regiments have still their colonels. Nay, even a corporal's company may be kept together by an energetic corporal. And this we shall see to be the case with animals when their brain has been removed; each separate part of the organism has its general, colonel, or corporal.

The Brain, as generalissimo, needs an enormous supply of blood. Haller estimates the quantity at one-fifth of the amount sent to the whole body. In mere mass, also, the brain predominates; but this is only true in the higher animals: in frogs, reptiles, and fish, it is *less* than the Spinal Chord.

In man the *average* weight of the whole brain is fifty ounces. In woman the average is five ounces less. Taking 50 oz. 3½ drachms as the weight of the whole encephalon, the following are the respective weights of the three principal masses:—

	oz.	drs.
Cerebrum, ...	43	15¾
Cerebellum, ...	5	4
Pons, and Med. Oblong., .	0	15¾
	50	3½[*]

---

[*] QUAIN'S *Anatomy*. By SHARPEY and ELLIS, 6th edit., ii. 434.

In the Cerebrum and Cerebellum, or big and little brain (Fig. 46—1, 2), the grey cellular matter is outside, and the white fibrous matter is inside. The outside presents a series of irregular folds or *convolutions;* and it is in the convolutions of the Cerebrum that physiologists have imagined all Sensibility and Thought exclusively reside. The degree of intelligence possessed by a man, or animal, has been confidently attributed to the number and depth of these convolutions—*i.e.* to the amount of grey matter. This idea has become so popular that I shall probably excite surprise when I suggest that it is altogether without a scientific basis. In the first place, intellectual action, which is surely a *function*, is made on this hypothesis a *property* of the grey matter, irrespective of any anatomical connection; and this is very unscientific. In the second place, although the amount of grey matter may, other things being equal, determine the amount of force developed, yet, as we saw in the comparison of the Sea-Hare with the Beetle, it will not determine *functional* superiority. In the third place, accurate measurements show that there is no solid ground for the opinion to rest on. M. Baillarger instituted an elaborate series of measurements, from which it appeared that the increase of the grey matter was *not* accompanied by an increase of intelligence; whereas increase of the white matter was accompanied by increase of intelligence.* Further, M. Camille Dareste has shown that the number and depth of the convolutions bear *no constant* relation to the amount of intelligence in animals.* Once imagine that intelligence has its seat in the grey matter of the convolutions, and these facts become inexplicable. But conceive intellectual action as *functional,* and the difficulty vanishes. No development of the pectoral muscles will enable an ostrich to fly, because the ostrich has not wings adapted for flight. In like manner no development of the grey convolutions will enable an animal to manifest intellectual actions, unless these convolutions are in direct connection with the other necessary parts of the mechanism. We can understand how particular aptitudes—say for drawing or music—may exist in men of small brains; or be entirely deficient in men of large brains. The requisite adjustment of the nervous mechanism is present in the one case, and absent in the other. Now what is true of particular forms of intellectual action, must be true of all. I conclude, therefore, that a larger amount of grey matter will produce a larger amount of Sensibility; but the uses which this Sensibility will subserve in the animal economy, will depend on the anatomical connections—on the complexity of the nervous mechanism.

---

* BAILLARGER: *Gazette Médicale*, 19th April 1845

II. Functions of the Cerebrum.—These preliminaries may help us to some understanding of the cerebral functions, by directing attention to its organic connections, native or acquired. Unhappily, as every one knows, Anatomy will not of itself disclose function; but it will help us to *explain* function, when discovered. The mere inspection of a knife would never disclose to a child the various uses of the knife; but having seen the knife in use, he would learn that it was the blade, and not the handle, which had the property of cutting; and after a little experience, he would ascertain that this property of cutting depended on the fineness of the edge. From this he would rapidly generalise, and conclude that all hard substances having a fine edge might be employed to cut with; and that steel, by reason of its hardness, was pre-eminently suited to that purpose. It is the same with anatomy. When observation has shown us the use of an organ, we can explain the action of its separate parts.

---

* Dareste: *Annales des Sciences,* 3e *série,* xvii. 30, and 4e *série,* i. 73. Compare also Leuret, i. 577 *et seq.*

Observation has, in various ways, remarked that the cerebral hemispheres are pre-eminently, if not exclusively, devoted to the intellectual actions. Whenever we hear of an idiot, we conclude that his cerebrum is very small, or malformed; whenever we see a man with an unusually small cerebrum, we conclude him to be an idiot.

There can be no doubt of the all-important participation of the Cerebrum in mental manifestation, when we learn that, if accident have produced a hole in the skull, we can, by a slight pressure on the Cerebrum, throw the patient into a state of stupor, seeing nothing, hearing nothing, thinking nothing. Pressure on the Cerebellum produces no such results.

Sir Astley Cooper relates in his *Lectures on Surgery* the following remarkable case: A sailor fell from the yardarm on the deck, and was taken up insensible. For some months he remained in the hospital at Gibraltar perfectly insensible, yet otherwise healthy. He was then conveyed to Deptford, where Mr Davy visited him, having been informed by the surgeon of the curious case. "It is a man who has been insensible for many months: he lies on his back with very few signs of life; he breathes indeed, has a pulse, and some motion in his fingers; but in all other respects he is deprived of all powers of mind, volition, or sensation." Mr Davy, on examining the patient, found a slight depression on one part of the head. Being informed of the accident which had occasioned it, he had the man removed to St Thomas's Hospital under the care of Mr Cline. It was here Sir Astley Cooper saw him "lying on his back, breathing without any great difficulty, his pulse regular, his arms extended, and his fingers moving to and fro to the motion of his heart, so that you could count his pulse by the motion of his fingers. *If he wanted food, he had the power of moving his lips and tongue; and this action of his mouth was the signal to his attendants for supplying this want.*"

I interrupt the narrative at this point to request the reader's attention to the passage in italics, which has apparently escaped the notice of the numerous writers who have referred to this case. The man's "consciousness" is said to have been obliterated; he had, we are told, lost all sensation and all volition; yet we see that the sensation of Hunger was not gone, nor the volition which indicated by signs that food was wanted. The current doctrine is incompetent to explain such facts; but according to the doctrine propounded in these pages, the facts are such as *a priori* might have been announced.

"Mr Cline," continues Sir Astley, "on examining his head, found an obvious depression; and thirteen months and a few days after the accident, he was carried into the operating theatre and there trephined. The depressed portion of the bone was elevated from the skull. While he was lying on the table the motion of his fingers went on during the operation, but no sooner was the portion of bone raised than it ceased. The operation was performed at one o'clock in the afternoon; and at four o'clock, as I was walking through the wards, I went up to the man's bedside, and was surprised to see him sitting up in bed. He had raised himself on his pillow. I asked him if he felt any pain, and he immediately put his hand to his head. This showed that volition and sensation were returning. In four days from that time the man was able to get out of bed, and began to converse; and in a few days more he was able to tell us where he came from. He recollected the circumstance of his having been pressed and carried down to Plymouth or Falmouth; but from that moment up to the time the operation was performed, his mind had remained in a perfect state of oblivion. He had drunk, as it were, of the cup of Lethe; he had suffered a complete death as far as regarded his bodily and mental powers; but by removing a small portion of bone with the saw, he was at once restored to all the functions of his mind, and almost all the powers of his body."

This is in many respects an interesting case. The psychologist should note that the man's memory was incompetent to recall not only everything that had passed from the moment of the accident, but also the whole series of events and sensations which occurred from the time of his impressment till that of his accident, which occurred in the Mediterranean.

It would be easy to multiply cases which point in this direction, and warrant the suspicion that the Cerebrum is the seat of intellectual action. But they only warrant a suspicion, they do not establish a certitude. Other evidence is needed. It might be asked whether other parts of the encephalon may not have been implicated — whether, when the Cerebrum is pressed upon, some interference with the masses underneath has not been induced—and whether the loss of mental manifestation may not result from this interference. The heart is assuredly not the seat of intelligence; yet if we check the action of the heart, we suspend all mental manifestations. What proof have we that the Cerebrum does not also act in some such indirect manner? Here precise Experiment becomes necessary. Unhappily precise Experiment is here of such excessive difficulty, that we shall find experimenters often arriving at different results. The

Cerebrum is not a mass definitely separated from the other masses, so that its removal can be effected without interfering with other parts. The very complexity of the connections established between it and other centres, renders every experiment dubious, except in that negative direction previously pointed out (p. 87).

The researches of Flourens made an epoch.* They were very striking: the conclusions he drew from them were announced in that systematic, definite, and absolute style, which is characteristic of French writers; and by reason of their being easily understood and easily remembered, they gained European acceptance, in spite of the reservations of Cuvier and Müller. They are really very captivating works. They seem to make the whole matter so clear and simple, that criticism and reservation are regarded as impertinent disturbers of a comfortable conviction. But the serious student learns to distrust all epigrams in science. Whenever a complex question is answered by a neat formula, pretending to embrace all the phenomena, and to clear up all obscurities, it is a wise caution which suspects there is something wrong. I am far from wishing to detract from the merit of Flourens. He made an epoch, and deserves honourable recognition. But I do not think his conclusions tenable in the face of evidence; his facts are not unfrequently in contradiction with what more careful experiment and observation disclose; and his conclusions are occasionally contradicted by the very facts he himself records, although theoretic preconceptions blinded him to their significance.

---

* FLOURENS: *Recherches expérimentales sur les propriétés et les fonctions du Système Nerveux*, 1824; and *Expériences sur le Système Nerveux*, 1825.

Flourens declares that when he removed the whole of the Cerebrum from Pigeons and Fowls, they lost *all* sensation, *all* perception, *all* instinct, and *all* volition. They lived perfectly well for months after the operation, if the food were placed in their mouths; but they never *sought* their food; they never *took* it, even when their beaks were plunged into it: they could swallow, and digest the grains; but they had no instinct to make them seek, no volition to make them pick up the grains. They *saw* nothing, although the iris remained irritable; they heard nothing; they could not smell. A state of stupor came on, resembling that of deep sleep. All voluntary action ceased. If they were thrown into the air, they flew; if irritated, they moved away; but if left to themselves, they remained motionless, with the head under the wing, as in sleep. Now, inasmuch as these effects always ensue when the Cerebrum is removed, and *never* when only the Cerebellum is removed, he concludes that all instincts, volitions, and sensations "belong exclusively to the cerebral lobes."

It should be stated that all experimenters agree in one cardinal point, namely, that removal of the cerebral hemispheres throws the animal into a stupor resembling that of deep sleep, Memory being almost, if not totally, destroyed. In this respect we may compare the case previously cited from Sir Astley Cooper (p. 101).

But all experimenters do not agree in other points named by Flourens; nor in the conclusions he has drawn. On the contrary, it is very certain, and we shall find evidence even in Flourens himself, that all instincts and all sensations are *not* destroyed by the removal of the cerebral lobes.

Let us hear Bouillaud on this subject.* He repeated the experiment of Flourens, removing the whole of the Cerebrum from the Brain of a fowl; and he thus records his observations: "This fowl passes the greater part of her time asleep, but she awakes at intervals, and *spontaneously.* When she goes to sleep, she turns her head on one side and buries it in the feathers of the wing; when she awakes, she shakes herself, flaps her wings, and opens her eyes. In this respect there is no difference observable between the mutilated and the perfect bird. She does not seem to be moved at all by the noise made round about her, but a very slight irritation of the skin suffices to awaken her instantaneously. When the irritation ceases, she relapses into sleep. When awake, she is often seen to cast stupid glances here and there, *to change her place, and walk spontaneously.* If put into a cage, *she tries to escape;* but she comes and goes without any purpose, or rational design. When either foot, wing, or head is pinched, she withdraws it; when she is laid hold of, she *struggles to escape, and screams;* but no sooner is she liberated than she rests motionless. If severely irritated, she screams loudly; but it is not only to express pain that she uses her voice, for it is by no means rare to *hear her cackle and cluck a little spontaneously;* that is to say, when no external irritation affects her. Her stupidity is profound; she knows neither objects nor places, nor persons, and is completely divested of memory in this respect: not only does she not know how to seek or take food, she does not even know how to swallow it when placed in her beak—it must be pushed to the throat. Nevertheless her indocility, her movements, her agitation, attest that she *feels* the presence of a strange body. Inasmuch as external objects excite in her no idea, no desire, she pays no attention to them; but she is not absolutely deprived of the power of attention, for if much irritated her attention is awakened. She knows not how to escape an enemy, nor how to defend herself. All her actions, in a word, are blind, without reflection, without knowledge."

---

* BOUILLAUD: *Recherches Expérimentales sur les Fonctions du Cerveau en général,* 1830, p. 5 *et seq.*

In this recital, the evidence both of *sensation* and *instinct* is incontestable, to any unprejudiced mind. Bouillaud, in commenting on his observations, remarks, that assuredly *all* sensation was not destroyed since the sensibilities of touch and pain were very manifest. Nor is it certain, he says, that the fowl heard nothing, saw nothing. It is true that she stumbled against objects, and knew not how to avoid them; but having lost all *memory*, that is intelligible. She opened her eyes on awaking, looked about, and showed a sensibility in the pupil to light; which, he thinks, is incompatible with the absence of all sensation of sight.

The experiments of Longet[*] seem decisive on this latter point. Having removed the whole of the Cerebrum from a pigeon, he observed that whenever he approached a light brusquely to its eyes, there was contraction of the pupil, and even winking; but, what was still more remarkable, "when I gave a rotatory motion to the candle, and at such a distance that there could be no sensation of heat, the pigeon made a similar movement with its head. These observations, renewed several times in the presence of persons who were at my lectures, left no doubt of the persistence of sensibility to light after removal of the cerebral lobes." We have only to think of the baby following with its eyes the light moved before it, to understand the kind of impression produced by the candle on the pigeon. Longet also declares that his experiments prove the existence of sensations of sound, after removal of the whole cerebrum.

Dr Dalton, giving the results of numerous experiments he performed, says that removal of the Cerebrum plunges the animal in "a profound stupor, in which he is *almost* entirely inattentive to surrounding objects. . . . Occasionally the bird opens its eyes with a vacant stare, stretches his neck, perhaps shakes his bill once or twice, or smooths down the feathers upon his shoulders, and then relapses into his former apathetic condition. This state of immobility, however, is *not* accompanied by the loss of sight, of hearing, or of ordinary sensibility. *All these functions remain, as well as that of voluntary motion.* If a pistol be discharged behind the back of the animal, he at once opens his eyes, moves his head half round, and gives evident signs of having heard the report; but he immediately becomes quiet again, and pays no further attention to it. Sight is also retained, since the bird will sometimes fix its eye on a particular object, and watch it for several seconds together." *

---

[*] LONGET: *Traité de Physiologie*, ii. 240.

While, therefore, Flourens concludes from his experiments that the Cerebrum is the seat of *all* sensation and all volition; and Bouillaud concludes that it is most probably the seat of *none;* Dr Dalton, more philosophical than either, concludes that the functions of the Cerebrum are restricted to those usually classed as intellectual. "The animal," he truly says, "is still capable, after removal of the hemispheres, of receiving sensations from external objects. But these sensations appear to make upon him no lasting impression. He is incapable of connecting with his perceptions any distinct succession of ideas. He hears, for example, the report of a pistol, but he is not alarmed by it; for the sound, although distinctly perceived, does not suggest any idea of danger or injury. The memory is altogether destroyed, and the recollection of sensations is not retained from one moment to another. The limbs and muscles are still under the control of the will; but the will itself is inactive, because apparently it lacks its usual mental stimulus and direction." *

---

* DALTON: *Human Physiology,* Philadelphia, 1859, p. 362. This lucid exposition of the principles of Physiology is—like the similar work of Dr Draper—an honour to American science.

Translated into the language of these pages, the experiments show that sensations are excited in the centres to which the respective sensory nerves belong; these centres, in turn, excite the sensibility of the great centre (Cerebrum) with which they are connected: these reflex, or secondary, excitations call forth others (trains of ideas), and one of these (say the idea of danger) acts by reflex on the motor-organs. By removing the great centre, you remove the possibility of its various reflexes; you do not remove the sensibility of other centres.

Dr Dalton reminds us how disturbance of the cerebral functions in human beings recalls these observations on animals. "In cases of impending apoplexy, or of softening of the cerebral substance, among the earliest and most common phenomena is a loss or impairment of the memory. The patient forgets the names of particular objects, or particular persons; or he is unable to calculate numbers with his usual facility. His mental derangement is often shown in the undue estimate which he forms of passing events. He is no longer able to appreciate the true relation between different objects and different phenomena. Thus he will show an exaggerated degree of solicitude about a trivial occurrence, and will pay no attention to other matters of importance. As the difficulty increases, he

---

* Dalton, p. 362.

becomes careless of the directions and advice of his attendants, and must be watched and managed like a child or an imbecile. After a certain period he no longer appreciates the lapse of time, and even loses the distinction between day and night. Finally, when the injury to the hemispheres is complete, the senses may still remain active and impressible while the patient is completely deprived of intelligence and judgment." *

Having seen how far other experimenters are from confirming the conclusions of Flourens, let us glance at his record of observations, and we shall find there evidence that *all* sensation and *all* volition cannot be localised in the Cerebrum.

Speaking of a fowl whose Cerebrum was removed the day before, he says, "she shakes her head and feathers, sometimes even she cleans and sharpens them with her beak; sometimes she changes the leg on which she sleeps, for, like other birds, she sleeps habitually resting upon one leg. In all these cases she seems like a man, asleep, who, without quite waking, changes his place, and reposes in another, from the *fatigue* occasioned by the previous posture: he selects one more *comfortable*, stretches himself, yawns, shakes himself a little, and falls asleep again. . . . On the third day the fowl is no longer so calm; she comes and goes, but without motive and without an aim; and if she encounters an obstacle on her path, she knows not how to avoid it."† In his second work he remarks of a Duck operated on in the same way: "As I mentioned last year *à propos* of fowls, the duck walks about oftener, and for a longer time together, when it is fasting than when it is fed."*

---

* DALTON, p. 363.

† FLOURENS, p. 89.

Here he observes the unmistakable evidence of feelings of Hunger, Fatigue, and Discomfort in animals which, according to him, have lost *all* sensation. He also observes the operation of instinct (cleaning the feathers), and of spontaneous activity (walking about), in animals said to have lost *all* instinct and *all* volition. We can only repeat the judgment of Johann von Müller: "Flourens concluded from his experiments that the cerebral hemispheres are alone the central organ of sensations. But this is not a legitimate inference from his highly interesting observations, which, as Cuvier remarked, prove directly the *contrary* of this."

If we review the evidence furnished by experiment and observation, we shall perceive that although the cerebral hemispheres play an important part in the psychical mechanism, they are not the exclusive centres of sensation, instinct, or volition, since all these are manifested when the cerebrum is removed. This is one step towards the demonstration of the truth, that neither the whole brain, nor any portion of it, can be the *exclusive* organ of the Mind. This is only a step, however, and we must wait until the whole nervous system has come under review before the demonstration can be complete.

We have still to ascertain what are the functions of the Cerebrum. The first point we notice is, that no nerve whatever issues from it. It is the ganglionic centre with which *all* nerves are indirectly connected, but from which no nerve takes its rise. All its vast array of nerve-fibres forms but a *connecting apparatus*, by means of which it is played upon by all other centres, and can play upon them. We are justified, therefore, in concluding that not one of the sensations excited by external stimuli can be *directly* produced in the Cerebrum, but must reach it through the ganglion of each stimulated nerve. It is on this ground, I presume, that Prochaska, Müller, Schrœder van der Kolk, and others, have maintained that consciousness of sensations is no function of the Cerebrum at all. I am not sure that I perfectly seize their meaning; but every argument seems to be against the idea that the Cerebrum acts as the original seat of *sensations derived through the sensory nerves.* This position must be clearly understood. The Cerebrum I conceive to be a seat of Sensibility, because of its ganglionic nature. But the forms of Sensibility excited in it must necessarily differ from those excited in other ganglia, which have different organic connections. It has no nerves distributed to the skin, or to the organs of special sense; it cannot, therefore, have the peculiar kinds of sensation which are excited through the skin and organs of sense. Its Sensibility can only be excited through the agency of its own nerve-fibres; and these connect its several parts together, and connect it with the other centres, upon which it plays, and through which it is played upon. We have no difficulty in understanding that, because the organic apparatus with which the optic centre is connected, differs from that of the auditory centre, the forms of Sensibility excited in these centres will be different. Nor should we have any difficulty in understanding why a ray of light acting on the optic centre must excite a different form of Sensibility from that which would be excited by its acting on the Cerebrum *through* the optic centre, *i.e.* giving the *sensation* of light in the one case, and the *perception* of light in the other.

---

* FLOURENS: *Expériences*, p. 20.

By this explanation I have endeavoured to make it clear how the *forms* of Sensibility excited in the cerebral hemispheres must necessarily be in many respects unlike those excited in the ganglia, which are *directly* stimulated by sensory nerves. The difference between a *perception* of sound and a *sensation* of sound is as great as the difference between the sensation of pain when a skin-nerve is divided, and the sensation of a flash of light when the optic nerve is divided.

The Sensibilities of the Cerebrum are usually grouped under two general heads—Intellect and Emotion. Hitherto it has been mysterious how an organ so homogeneous as the Cerebrum, wherein each convolution is but a repetition of the others, should nevertheless serve functions so various as those of the Intellect and the Emotions. Even those who reject the phrenological localisation of the separate faculties, admit that the front part of the Cerebrum seems specially devoted to the Intellect, and the middle and back parts to the

Emotions and Instincts. But neither phrenologists, nor their antagonists, to my knowledge, have had any scientific explanation to offer; and as the views advocated in these pages seem to me to present a possible elucidation of the difficulty, I will first state the objection, and then endeavour to remove it.

The convolutions of the Cerebrum are everywhere similar and continuous, like so many folds in a piece of velvet. They are not separate; they are not different; they are identical. Why, then, can we suppose they are the organs of very different functions? We do not imagine that one lobe of the liver, or lobule of the kidney, plays a different part from that of its fellows; why, then, do we imagine that one convolution, or group of convolutions, can be devoted to reasoning, and another to loving, one to the perception of colours, and another to an instinct?

The phrenologists evade this difficulty by saying that "observation teaches them the facts *are* so." Without here disputing this assertion, I must remark that it in no way removes the scientific difficulty; and as Phrenology professes to be the Physiology of the Brain, it is bound to connect its observations with some scientific explanation of the nervous mechanism involved.

We need not evade the difficulty; for if Function be determined by anatomical connection, it is clear that, although the Cerebrum may be one homogeneous structure, it will have various functions if it be connected with various organs. Assuming that every convolution has the same general property of Sensibility, our principles lead to the conclusion that the peculiar *kind* of Sensibility excited in each will depend upon the nature of the stimulus; and the nature of the stimulus will of course be determined by the organic apparatus effecting it. A stimulus through the optic centre must be different from a stimulus through the gustatory centre; and both of these must be different from a stimulus through the viscera. The stimulus received through the centre of Perception, will necessarily differ from that received through the centre of the Appetites; and thus, widely as an Idea differs from an Emotion, they are both peculiar forms of Sensibility.

Although the general relations of the cerebral convolutions are thus indicated, it would require an entirely new investigation of the anatomy of the Brain to carry this into detail. For the present our knowledge is too imperfect to admit even of an attempt at detail. I will, however, venture to suggest the point of view which might direct inquiry. If we ask what are the chief divisions into which all psychical phenomena group themselves, we shall find that the popular classification into Thinking and Feeling, or Mental and Moral, lucidly and broadly indicates the primary groups. Let us, ideally, decompose these into six centres, three for each division. In the first group we may place Sensations, Perceptions, and Ideas—the various combinations of which represent intellectual activity. In the second group we may place Sensations, Instincts, or Appetites and Emotions—the various combinations of which represent moral activity.

Sensation thus forms the point of departure for each series. But we have already seen that there are various *kinds* of sensation, and that these fall into two distinguishable groups—namely, Sense-sensations and Systemic-sensations. The first are derived through the organs of special Sense, *i. e.* Sight, Hearing, Taste, Smell, and Touch. The second are derived from the Viscera, Muscles—in a word, from the system at large. The first may almost be considered as *impersonal*, because they bring us into conscious relation with external objects—with the Not-self. The second are, on the contrary, intensely personal, because they do not bring us into conscious relation with external objects, but only with what is passing within our bodies. Reason has often been considered impersonal, but no one ever considered Emotion to be so. The intellectual operations always imply an externality; even when we are speculating about our own feelings or mental operations, we always view them as if *apart* from ourselves. The Emotions have a deeper root in personality. Every man, unless he be a subtle psychologist, believes that the redness is in the rose, the sweetness in the apple, and the loudness in the thunder. But no man, not even the psychologist, believes that the love or hate, the fear or reverence, the desire or disgust, which moves him, belongs to the object which excites the feeling.

This externality of the Sense-sensations, and internality of the Systemic-sensations, creates a broad line of demarcation between the Perceptions which arise from the one, and the Appetites, or Instincts, which arise from the other; and these, in turn, issue in the very different forms of Sensibility known respectively as Thought and Emotion.

It has never been doubted that our perceptions and ideas have their origin in sensations. The old philosophic adage that nothing is in the intellect which was not previously in the sense, may be equivocal, from a restricted use of the word sense; but it points to the well-authenticated fact, that sensation lies at the basis of all intellectual operation. I feel myself justified, therefore, in considering *Ideation* as the form of cerebral sensibility which is determined by the cerebral connections with the ganglia of special Sense. In like manner, *Emotion* may be considered as the form of cerebral sensibility which is determined by connections with the ganglia of visceral sensation. It was formerly believed that the heart, the liver, and the spleen, were seats of the passions; popular language still preserves this notion; but Bichat was the last great anatomist who countenanced the doctrine. Since that doctrine has fallen into discredit there has been an undue neglect of the important fact which it endeavoured to explain, namely, the immediate influence exercised over the emotions by the condition of the viscera, and the influence exercised over the viscera by the state of the emotions. Both the ancient and the modern doctrine are reconciled in the view I have put forth, which makes the viscera the *main* source of emotions, just as the organs of sense are the main source of ideas.

Although the mental phenomena are thus grouped under two primary and six secondary divisions, to which anatomy may one day assign corresponding parts of the nervous apparatus, we must never lose sight of the fact, that there is perpetual action and interaction between the various centres. But this consideration leads us into the domain of Psychology, and we must therefore pause here. It is not for us to attempt to unravel the tangled mystery of mental operations. We have not been able to do more than indicate in a very general manner the relations existing between the Mind and the Brain ; and having done so, we have now to ask, whether the cerebral hemispheres have any *other* functions? I think they have ; but as physiologists have not directed their attention in this direction, there are no experiments on record to throw light on it. Lallemand conceives that there must be some necessary dependence of the Spinal Chord on the Cerebrum in the higher animals, and it is to this he attributes the fact, certainly significant, that children born without a brain never live more than three or four days : they do not die of starvation, for they swallow food quite well. He also observes that the *movements* of these brainless children are much feebler than those of others at the same age.* One might, indeed, suspect the cause to lie deeper; the same cause which determined the malformation may determine the feeble vitality. But Lallemand's idea is strengthened by the observation of Flourens,† that removal of the cerebrum retards digestion; and by the familiar fact of the intimate sympathy between the Mind and all the organic functions.

---

* Quoted by SOLLY *On the Brain,* p. 455.

† FLOURENS: *Recherches,* p. 129.

It is perhaps on this ground that we may explain the disproportion between the mental activity and the size of brain in certain animals. The elephant has a brain three times heavier than that of man; and that of the whale is nearly twice as heavy as that of man. It is true that in the elephant and whale the cerebrum is not so much greater than the other parts, as is the case in man, in whom, as we saw, it is eight times the size of the other parts; nevertheless the preponderance of the cerebrum is considerable, and there is certainly no preponderance of mental power to correspond with it. When, however, we conceive the cerebrum as the chief nervous centre having dominion over all others—when we assign to it participation in other functions besides those of mental activity, the immense size of the body of the elephant and whale seems to justify an equivalent development of the cerebrum.

On this, however, we must be content with hypotheses. Science has no carefully-collated facts to justify us in anything beyond a general surmise that the cerebral hemispheres have more than an intellectual and volitional part to play: that they are directly connected with the functions of intelligence, no reasonable man will doubt; and having arrived at this conclusion, we will now pass on to the examination of the other centres, by anatomists grouped together under the general term Brain.

CHAPTER IX.

(*Continued.*)

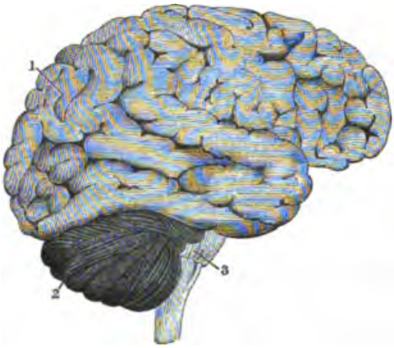
THE MIND AND THE BRAIN.

SECTION II.—THE CEREBELLUM AND MEDULLA OBLONGATA.

Description of the Cerebellum—Gall's hypothesis—Experiments of Flourens to prove that the Cerebellum co-ordinates muscular movements: arguments against such an hypothesis—Flat contradictions—The experiments of Schiff—Evidence furnished by Comparative Anatomy; and by Disease—Absence of the Cerebellum not accompanied by absence of co-ordination: Combette's case—The Cerebellum as a seat of sensation and volition—Has the Cerebellum any share in intellectual operations?—Description of the Medulla oblongata—Is the Medulla distinct from the Spinal Chord?—The Medulla a seat of volition—Why it is considered a seat of sensation—Is there a "vital point?"—Experiments of Flourens contradicted by the experiments of Schiff and Brown-Séquard—Influence of the Medulla on the heart's action—Why a "shock" of grief or joy causes death—Pain the cause of death—Summary of the functions of the Brain.

AFTER the Cerebrum, or big brain, our attention must be claimed by the Cerebellum, or little brain, which, in man, lies underneath the greater mass, as seen in Fig. 51, 2, p. 122. The two organs present some differences in structure, but they agree in principal points: they are both composed of grey matter externally, and internally of white matter. The convolutions of the Cerebellum are more com-pressed, and have the appearance of leaflets. Like the Cerebrum, it is totally insensible to pricks or pinches. Its relation to the other centres is roughly indicated in this diagram (Fig. 52).

Fig. 51.



THE HUMAN BRAIN IN PROFILE.
1, Cerebrum; 2, Cerebellum; 3, Medulla Oblongata.

Fig. 52.

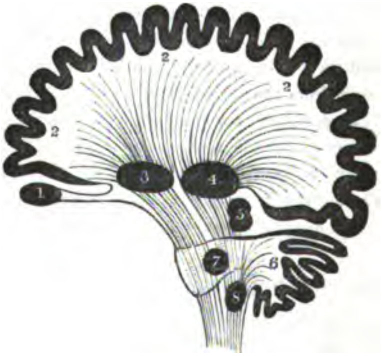


DIAGRAM OF VERTICAL SECTION OF THE BRAIN.
1, Olfactory Ganglion; 2, the Cerebral Hemisphere; 3, Corpus Striatum; 4, Optic Thalamus; 5, Tubercula quadrigemina; 6, Cerebellum; 7, Ganglion of the Pons Varolii; 8, Olivary body, or Ganglion in the Medulla Oblongata. The lines show the direction of the fibres.
(After Dalton.)

Gall, to whom we owe so great a debt for the impulse he gave to the study of the nervous system, assigned the sexual instinct to the Cerebellum. This hypothesis, after having been extensively discussed, is now, I believe, rejected by all good physiologists; and we must declare with Longet that a calm survey of the evidence forces the conclusion that neither Anatomy nor Pathology, neither Experiment nor Comparative Anatomy, justifies our assigning the sexual instinct to the Cerebellum.*

It is extremely probable that an antagonism to Phrenology, and a desire to find it in the wrong, has considerably sharpened the wits of physiologists, in the investigation of this point, making them at once more patient in research, and more circumspect in interpretation, than they are wont to be when no doctrine is at stake. If a little of this animus had moved them to scrutinise more closely the hypothesis first propounded by Flourens, it would never have obtained the too facile acquiescence of Europe; and our science would have been burdened with one error less. But there was nothing at stake in the hypothesis that the Cerebellum was the organ of muscular co-ordination—nothing but Truth; and, unhappily, the desire for Truth does not move the passions of men with the same energy as a desire to attack or defend a system.

If there is a position which our text-books represent as impregnable, it is that the co-ordination, or regulation, of muscular movements is the peculiar function of the Cerebellum. It seems looked on as a first truth. The experiments of Flourens and Bouillaud are considered as having established the point beyond dispute.† With what justice we will now inquire.

---

* LONGET: *Traité de Physiol.* ii.; LELUT: *Rejet de la doctrine organologique;* LEURET et GRATIOLET: *Anat. Comp. du Système Nerveux,* i. 427, 547. Dr CARPENTER has also collected evidence on this point in his *Human Physiology.*

† FLOURENS: *Recherches,* &c. BOUILLAUD: *Recherches tendant à réfuter l'opinion de M. Gall sur les fonctions du Cervelet,* 1827. Although

Flourens removed, by successive slices, the Cerebellum of a goose. With the first slices he noted hesitation and want of harmony in the movements of the bird; with the second slices the walk became a stagger; with the final slices all power of locomotion, all power of retaining the equilibrium of the body, disappeared. Throughout these operations the intellectual and sensitive faculties were undisturbed. "The Cerebellum, therefore," concludes Flourens, "is the seat of the co-ordination of muscular movement; but not the seat of intelligence, sensation, volition. The Cerebrum *feels* and *wills;* the Medulla oblongata and spinal chord *execute;* the Cerebellum *co-ordinates.*" This is one of those neat epigrams which captivate assent, and rest in the memory; and as regards the Cerebellum, it receives further assistance from the following statement made by Longet: "Take two pigeons—from the one remove the whole Cerebrum, from the other only a part of the Cerebellum; on the morrow, the first will be firm on its feet, the second will have the gait of a drunken man."*

The fact is indisputable; but does it carry the conclusion? It seems to me that there is a misconception of the bearing of experimental evidence, in the supposition that injury to an organ, followed by disturbance of a particular function, *proves* that the function

---

BOUILLAUD controverts FLOURENS on some points, showing that it is only over the muscles of the extremities that the co-ordinating power extends, yet he agrees with him on the main point.

* LONGET, ii. 265.

in question has its seat in that organ: nothing more than a suspicion can be warranted by such evidence; and in the present case the suspicion is proved to be erroneous, by the fact that the function can in some cases be exercised when the organ is absent. Experiment and Pathology, if sufficiently examined, distinctly pronounce against the hypothesis.

The idea of there being one particular seat of muscular co-ordination appears to me illusory. Each centre co-ordinates the muscles to which its nerves are distributed; and the co-ordination of the whole depends on the integrity of the whole mechanism. The Spinal Chord is the general centre from which spring the nerves distributed to the limbs and trunk. If this chord be divided, lengthwise, into two halves, each half will co-ordinate the muscles of each side of the body; but the two sides will not move harmoniously, because the integrity of the mechanism has been disturbed; and as regards the movements of the limbs, the two halves might be two animals. It is the same when the chord is divided across: this cuts the body into two halves; but the fore-legs are co-ordinated by the anterior half, the hind-legs by the posterior half. But if the division has not been complete, if we leave the grey matter to be on one side continuous, then Stilling assures us, the continuity enables both sides to be co-ordinated.*

The experiments of Flourens and Bouillaud certainly prove that the Cerebellum exercises some marked influence on muscular movements, at least those implied in sustaining equilibrium. But this is true only of birds and mammals. It is unequivocally *not* true of lizards, frogs, tritons, &c. Flourens indeed says: "I removed the Cerebellum of a frog; walking, leaping, even standing, were suddenly abolished. I removed the Cerebellum, bit by bit, from another frog; walking, leaping, and standing were gradually abolished."* The facts were doubtless as he describes them; but I venture to say that if he had allowed half an hour to elapse, so as to allow the shock of the operation to pass away, he would have seen the frogs leaping and swimming as vigorously as ever. Every one who has experience in these matters will remember that unless the frogs so operated on (or even after the entire removal of the brain) were carefully secured, they were soon hopping about the house—to the horror of maid-servants, and amusement of the experimenter.

---

* STILLING: *Untersuchungen über die Functionen des Rückenmarks*, 145, 146, 155.

Here then is a patent contradiction: The Cerebellum, which has such marked influence over the movements of birds, has none at all, that is appreciable, over the movements of frogs. Those who suffer phrases to do duty for ideas, may get over this contradiction by saying that birds are "differently constituted" from frogs, and therefore the Cerebellum may have a function in the one which it has not in the other. But no biologist, who pretended to philosophy, would be satisfied with a phrase so vague; he would demand in what this said difference of constitution consisted. He would know that function is determined by anatomical connection, and would inquire what were the anatomical differences in these cases.

---

* FLOURENS, p. 51.

I was groping my way through this obscurity, when the experiments of Schiff seemed to flash a sudden streak of light. He showed that the strands of nerve-fibres which pass into the Cerebellum (called the *crura cerebelli*) are instrumental in fixing the vertebral column in the region of the neck; and that when an injury to the Cerebellum disturbs the harmony of movements, it is by acting on these neck-fixing parts. If the Cerebellum, he says, be cut through on one side, the movements become irregular; but if it be cut through on both sides, in a perfectly symmetrical manner, the movements do not at all become irregular.* No one need be told that the equilibrium of the body is maintained by the adjustment of the vertebral column. This must be notably the case with a long-necked bird; it is not, however, the case with the short-necked frog, or triton, which requires no such adjustment of its column to preserve equilibrium.†

After this refutation, it is interesting and instructive to read the array of facts which can be brought from

---

* SCHIFF: *Lehrbuch der Physiol.*, i. 355.

† Since this was written, the German translation of SCHRŒDER VAN DER KOLK'S work has appeared, in which he declares it has always been incomprehensible to him how men could believe the cerebellum to be the seat of co-ordination, which he places in the spinal chord, and he adduces the movements of decapitated frogs as proof.—*Bau und Functionen der Med. Spin.*, 1859, p. 68.

Comparative Anatomy to give countenance to the hypothesis: "On ascending the scale of mammiferous animals, we cannot but be struck with the rapid advance in the proportional size of the Cerebellum which we observe as we rise from the lowest (which are surpassed in this respect by many birds) towards man, in whom it attains a development which appears enormous even when contrasted with that of the Quadrumana. In proportion, in fact, as the extremities acquire the power of prehension, and together with this a power of application to a great variety of purposes—still more in proportion as the animal becomes capable of maintaining the erect posture, in which a constant muscular exertion, consisting of a number of most elaborately combined actions, is required—do we find the size of the Cerebellum and the complexity of its structure undergoing a rapid increase. Thus, even between the dog and the bear there is a marked difference; the latter being capable of maintaining for some time the erect posture, and often spontaneously assuming it; whilst to the former it is anything but natural." *

This evidence is valuable as indicating some connection between the Cerebellum and muscular movements, but it cannot weigh against the positive evidence just adduced. Nor can much countenance be gained from the evidence of Disease, although that also is frequently invoked. Irregularity of co-ordination is indeed sometimes found accompanying disease of the

---

* CARPENTER: *Human Physiology*, p. 521.

Cerebellum; but more frequently not; and it sometimes accompanies disease of the Cerebrum. Andral has collected ninety-three cases of diseased Cerebellum, and of these "there is *only one* which tends to favour the opinion of physiologists." Longet, who quotes this, remarks that inasmuch as the diseases were for the most part chronic, it is possible that the unaffected portions may have performed the co-ordinating function; and he considers the evidence of Experiment to be more conclusive. On the other hand, Ludwig curtly declares that all experiments on birds are valueless for the physiology of man, since no one has ever observed these phenomena of imperfect co-ordination when the human Cerebellum has been injured.* I agree with Longet that experiment is more conclusive; but it concludes *against* the hypothesis. Pathology, however, records one very striking case, which of itself would suffice to settle the question, because it shows us the *absence* of a Cerebellum in a human subject. It was first published by Combette, in the *Revue Médicale*, 1831, and is constantly alluded to. Mr Solly reports it thus, in his work on the Brain:—

"This was a case in which the Cerebellum was entirely wanting, nothing being found in its place but a *quantity of serous fluid* contained in the membranes: on each side a pedunculated body, not larger than a pea, was attached to the *corpora restiformia;* all the rest seemed replaced by a nervous sac. The pons Varolii, as well as the Cerebellum, were absent, and the individual in whom this remarkable lesion was observed had attained the age of eleven years.

---

* LUDWIG: *Lehrbuch der Physiol.*, i. 210.

"This case shows that *agenesia cerebelli*, or total want of the Cerebellum, does not necessarily render existence impossible, provided the other parts of the nervous centres be well conformed. The individual may even live for a considerable time. The child here was nearly twelve years of age, and we have reason for considering the disease as congenital; for had it been acquired—had this absence of the whole Cerebellum depended on an actual destruction of the nervous substance by an organic lesion, it is not probable the child would have survived so long.

"What were the phenomena observed during life? What effect did this complete atrophy produce on the intellect? What modification in the function of motion? The intellectual faculties were obtuse, though not to a remarkable degree; the answers slow and difficult; the whole countenance expressive of stupidity: in a word, the child, though not exactly idiotic, still showed a deviation of the mental powers. The motility was also modified; the power of motion was considerably weakened in the lower limbs, which did not possess their natural force and vigour; hence the child was unable to support itself with any firmness; it fell down frequently; the legs crossed each other during walking, and the gait was irregular and unsteady. At length the child was compelled to confine itself altogether to bed, and after some time was unable to stir, even when lying in a horizontal position.

Thus the modification of motility consisted in the gradual abolition of motion: to this were joined epileptiform convulsions, which continued for some time, and finally carried off the patient. The sensation of the integumental covering was not modified in any way whatever. There was no increase of sensibility in the commencement—no obtuseness or diminution of feeling, even when paralysis was most complete; the senses also remained intact. The child could see, hear, and taste, in a perfect manner. The functions of nutrition, circulation, and respiration were carried on without any notable disturbance."

This remarkable case has been quoted to prove the truth of the opinion advanced by Flourens; it is alluded to by Longet as one subversive of *all* the hypotheses that have been advanced. If we examine it by the light of the experimental canon previously laid down—namely, that "the disappearance of a function, on the removal of an organ, is no proof whatever that the function had its seat in that organ; but the *persistence* of a function, after the removal of an organ, is rigorous proof that the function had *not* its seat in that organ"—we shall clearly see that the opinion of Flourens is refuted by this case. The co-ordinating power was weak, indeed, at least in the lower extremities (nothing is said of the upper); but the whole motility was weak, and at last destroyed. Now the hypothesis which assigns co-ordination as the function of the Cerebellum, expressly denies that organ to be the seat of motility,—that being assigned to the Spinal Chord.

It is co-ordination alone which the Cerebellum is said to effect; and when the whole of that organ is removed, all co-ordination, at least of the extremities, is said to disappear. But this was assuredly not the case in the instance just quoted; the whole of the Cerebellum was absent, and only a slight *part* of the co-ordinating power was affected. This persistence of the function in the absence of the organ is a demonstration of the error of Flourens. Whatever were the causes which affected the motility of the child, who was half an idiot, they also necessarily affected her power of co-ordinating her movements.*

Two other cases are referred to by Brown-Séquard, wherein total destruction of the Cerebellum by disease was unaccompanied by paralysis, or any other trouble in the functions of animal life. "I have ascertained," he says, further, "that it is by the irritation they produce on various parts of the basis of the brain that the diseases of the Cerebellum, or its extirpation in animals, cause the disorder of movements which has been considered as depending upon the absence of a guiding power. In fact, the least irritation of several parts of the brain, with only the point of a needle, may generate very nearly the same disorder of movements that follows the extirpation of the Cerebellum." †

---

* Dr Robert Willis, in the valuable notes to his translation of *Wagner's Physiology*, takes a similar view: "The poor child was almost an idiot; but as she spoke, and walked, and used her hands, it is obvious that the Cerebellum could have had nothing to do with the highly complicated movements requisite in all these acts" (p. 637).

† Brown-Séquard: *Lectures* in *Lancet*, 27th August 1858.

Lockhart Clarke is also indisposed to accept the current hypothesis. "That the Cerebellum," he says, "is in some way concerned in the regulation and co-ordination of muscular movements, appears to follow from experiment and pathological investigation; but I think there are many reasons for believing that the mode of action usually assigned to it is unsatisfactory, and at variance with many established facts."*

It is an interesting illustration of the powerful bias which may be given to the mind by a striking experiment, or a captivating formula, that a multitude of facts which contradicted Flourens have either been disregarded, or have been only admitted as suggestive of doubts. But the reader will, I hope, now see that the hypothesis of the Cerebellum being the organ of muscular co-ordination is not doubtful — it is utterly erroneous.

II. THE CEREBELLUM A SEAT OF SENSATION AND VOLITION.—What, then, is the function of the Cerebellum? Unhappily no clear answer can be given to that question; and mainly because the answer has not been sought in the right way. All I can attempt at present is to show that the Cerebellum must be a seat of Sensation and Volition, because it is a great ganglionic centre, or group of centres, having the same tissues as those other cranial centres which every one admits to be endowed with Sensibility.

It has been held by very eminent investigators that the Cerebellum is *the* seat of Sensibility.* This is an exaggeration; but it suffices, even as an exaggeration, to show how many facts speak in favour of Sensibility in this organ. Mr Solly has quoted the following passage from Foville, which bears upon the point: "The opinion advanced by some physiologists that the Cerebellum is the regulator of the voluntary movements, if we attentively consider the reasoning on which it rests, seems to me to strengthen the idea which places the central seat of sensibility in the Cerebellum. After having injured the structure of the Cerebellum extensively, we have observed that animals preserved their power of moving their limbs, but had lost that of coordinating the movements of these in a manner convenient to station, progression, flight, &c. But when we *will* to perform, and actually perform, certain movements, do we not distinctly feel that we execute them? The man who, with his eyes shut, moves his hand or his arm, does he not also distinctly feel that he moves these parts as if he followed them with his eyes? While the paralysed man who, with his eyes shut, is desired to move his paralysed limbs, may be very willing to do so, though incapable and perfectly aware of his incapability of obeying. If this be true, how can we expect that an animal deprived of the faculty of perceiving the sensation of the movements which it executes, should execute them in the *ensemble* with harmony, and in accordance with a proposed end? How can we expect it to walk deliberately and to keep its equilibrium, if it does not feel the ground upon which it stands, if it is ignorant of the position in which its limbs are placed? I remember conversing with Sir Astley Cooper on this subject towards the end of 1830. Sir Astley cited to me the case of a man completely deprived of the faculty of sensation in one arm and hand, the muscular power of which was, however, preserved. When this man was desired to take hold of and to lift anything, he did so quite well; but if, whilst holding the object, his attention was taken away from the hand, irregular contractions of the limb commenced, and very soon the object fell to the ground: as soon as the patient ceased to follow the contractions of his fingers with his eyes, nothing remained to inform him that he held the object, when, of course, it escaped from his grasp. This, and other cases of a similar description, seem conclusive as to the fact of sensation being the true regulator of the muscular motions: it is by means of sensations that we are aware of the mode or degree of action in our muscles, that we have the power of co-ordinating their contractions in a suitable manner, and of executing a succession of voluntary movements in harmony with one another."

---

* LOCKHART CLARKE: *Philosophical Transactions*, 1853, p. 531.

---

* Compare LONGET, *Traité*, ii. 263, for authorities. He himself thinks that it participates in sensation but is positive, that it is not the *exclusive* seat.

Indeed there will be few physiologists now to dispute the position, that the facts which indicate the Cerebellum to have a considerable share in muscular coordination, also prove that it must have a share in

Sensation, since without Sensation no co-ordination is possible. What that share precisely is, we cannot say. It is clear that the Senses are not dependent on the Cerebellum; for the girl in Combette's case was in perfect possession of Sight, Hearing, and Taste. It is equally clear that Pain has not its centre there. But nothing can be said very decisively as to the other sensations.*

Has the Cerebellum any share in the intellectual operations? Flourens and Bouillaud declare not; but Longet properly remarks, that inasmuch as animals deprived of that organ survive but a little time, and generally in a state of great agitation, it is difficult to form an opinion as to the state of their intelligence. Nor can much reliance be placed on pathological cases. Very frequently no trouble whatever of the intellectual faculties seems to accompany disease of the Cerebellum; but inasmuch as disease of the Cerebrum is also frequently unaccompanied by disturbance of the intelligence, no conclusion can be founded on that. Disease sometimes notably disturbs the intelligence; and in Combette's case the girl was nearly idiotic.

When, therefore, we consider the great similarities in structure and position between the Cerebrum and Cerebellum; and when, further, we reflect that the Cerebellum in the various stages of the animal scale is always found to be developed in proportion to the intellectual development of the animal, being beyond all comparison greatest in man, and smallest in fishes, there is, it must be confessed, reason to suspect that the Cerebellum also *participates* in some degree in the production of mental phenomena. At the same time we must admit that there is no direct evidence on the point, either for or against it.

---

* Dr NOBLE, in his work, *The Human Mind in its Relations with the Brain and Nervous System*, p. 49 *seq.*, advances the hypothesis that the ganglionic masses of the Cerebellum named *corpora dentata* are the seat of tactile sensibility.

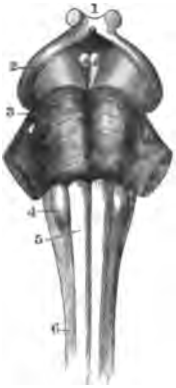
III. FUNCTIONS OF THE MEDULLA OBLONGATA.—Besides the Cerebrum and Cerebellum, there are other smaller ganglionic masses at the base, the most important of which are the *corpora striata* and *optic thalami* (Fig. 52, 3, 4, p. 122), and each of which must play some important part in the nervous mechanism. But what that part is, can in no case be stated with even an approach to certainty. The hypotheses are numerous and contradictory;* but except the share which the *corpora quadrigemina* have in vision, there seems to be no solid ground of fact yet ascertained. We will therefore pass over these ganglia, and direct attention to the third important organ of the encephalon—the *Medulla Oblongata.*

Although included among the parts of the Brain, or encephalon, because it is included within the cavity of the skull, and because its columns pass into both Cerebellum and Cerebrum, the *Medulla Oblongata* is

---

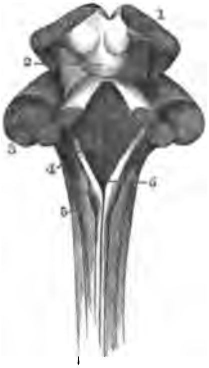
* The latest I have met with is that of Dr NOBLE, who regards the optic thalami and corpora striata as the seat of emotional sensibility. —*The Human Mind*, p. 60.

Fig. 53.



Front or under view of the Medulla Oblongata.
1, The Optic Nerves; 2, *Crura Cerebri*, or strands of white matter running into the hemispheres; 3, *Pons Varolii*; 4, Olivary Bodies; 5, Anterior Pyramids; 6, Spinal Chord.

Fig. 54.



Back view of the Medulla.
1, Section of the Optic Thalamus; 2, Corpora quadrigemina;[*] 3, Section of the *Crura Cerebelli*; 4, Fourth Ventricle; 5, Restiform Bodies; 6, *Calamus Scriptorius*.

really and truly the upper part of the Spinal Chord, and bears more resemblance to the Spinal Chord than to any portion of the Brain. This, which is obvious on a superficial examination, becomes still clearer on a microscopic investigation; and although the point is of little importance as a matter of naming, it will presently be seen to have considerable importance when we come to the interpretation of functions. In Figs. 53 and 54 we see the Medulla in its connections with the Brain and Chord.

Such is the external aspect. Respecting the internal structure little can here be said, since there is probably no anatomical question which can surpass it in complexity and obscurity. The student desirous of pursuing the subject will do well to prepare himself by studying the important works noticed below.[*] It will suffice for our purpose if we can bear in mind that the Medulla is really a prolongation of the Spinal Chord, to which there are some additions both of grey and white matter.[†] It has ganglionic masses peculiar to it, named the *Olivary bodies* (Fig. 53, 4). It has also a great variety of commissural fibres, by means of which an immense variety of actions are co-ordinated. Sensory nerves and motor nerves issue from it; but these do not display the symmetry of arrangement noticed in the nervous pairs issuing from the Chord. It is also the ganglionic centre for the nerves of Hearing and Taste.

---

[*] The two lower bodies are imperfectly distinguished in the engraving.

If we now ask what are the functions of this complex part of the Encephalon, we shall find that the ever-recurring question as to whether the Brain is or is not the exclusive sensorium, may here receive considerable illustration. We shall also find that there was something more than a mere question of naming involved in the doubt as to whether or not the Medulla belonged to the Brain. For observe, the Medulla is now almost unanimously declared to be a seat of sensation and volition. If, therefore, it be *not* included in the term Brain, there can be no longer any question as to the Brain being the exclusive sensorium; and if the upper part of the Spinal Chord can be a seat of sensation and volition, there can be no valid argument against the evidence which proves that the rest of the Spinal Chord is also a sensorial seat.

---

[*] The two works of STILLING (*Ueber die Medulla*, 1843, and *Ueber den Bau des Hirnknotens*, 1846), form the storehouse whence succeeding writers have gratefully drawn their chief materials, the only considerable additions being due to SCHRŒDER VAN DER KOLK (*Bau und Functionen der Medulla Spinalis und Oblongata*); and LOCKHART CLARKE (*Philosophical Transactions*, 1858), whose admirable researches have been duly appreciated by the few persons in Europe competent to estimate them. In citing the last-named anatomist, I cannot here forbear acknowledging how much I owe to the readiness with which he initiated me into his method of making microscopic preparations.

[†] "It is well known that in the spinal chord the columns of white and grey substance are parallel to each other, and preserve the same relative position throughout their entire course; but in the Medulla Oblongata these parts not only assume a different arrangement, by becoming more or less blended with each other, and with new structures, but frequently pursue a curvilinear direction in different planes, inclined at varying angles."—LOCKHART CLARKE.

Müller says: "*It is the seat of volition.* The experiments of Flourens show that animals in which the cerebral hemispheres have been removed, though in a state of stupor, are still capable of executing voluntary movements; they have still this power after the removal of the cerebellum also."* I am not sure that I rightly understand this; but we may suppose him to mean that the Medulla is *a* seat of volition; that it is not *the* exclusive seat, will be evident when I narrate my own experiments, which show that animals manifest volition after removal of the whole Brain, including the Medulla. Müller thus continues:—

"*It is the seat of the faculty of sensation.* This is not merely shown by the anatomical fact that all the cerebral nerves, with the exception of the first and second (olfactory and optic), are connected with it, or with its prolongations into the Brain; it is proved also by experiment."

---

* MÜLLER: *Physiology*, i. 828.

The reader is requested to pay particular attention to the passage just quoted, and to the assertion that the Medulla must be the seat of sensation because nerves are connected with it. Schrœder van der Kolk, who also maintains that the Medulla and the ganglia at the base of the Cerebrum constitute the sole sensorium,* is more specific than Müller, for he says that the Medulla must be "the seat of Perception, the spot where sensory *impressions* become *sensations*," because it is there (as he supposes) that the sensory nerve-fibres terminate in ganglionic cells. Now I ask, if the fact that nerves are connected with ganglionic centres is a proof that these centres must be the seat of sensation; how can we deny that *similar* ganglionic centres lower down the Spinal Chord, which are *similarly* connected with sensory nerves, must also be the seat of sensation? And this question becomes still more pressing when we learn, as we shall presently, that the same kind of experiments as those which indicate the Medulla Oblongata to be a seat of sensation, no less decisively indicate the Spinal Chord to be one. The only *possible* answer which occurs to me is this: The nerves which issue from the Medulla Oblongata, and the ganglia above it, are nerves of *special* sense—namely, the auditory, gustatory, olfactory, and optic; each of these nerves, being in connection with a special apparatus, is capable of producing a different effect from that of all other nerves. It is only the nerves of special sense which can create sensation.

---

* SCHRŒDER VAN DER KOLK: *Bau und Functionen*, p. 88.

Easy as it is to refute such an answer, I have thought it worth while to adduce it, because Logic irresistibly forces us to conclude, that unless sensation be the product of a *particular* group of nerves and their ganglia, it must be the product of *all* nerves and their ganglia. Unless some reason can be shown why the apparatus of Sight, Smell, Taste, and Hearing, has the peculiar privilege of awakening sensation, the mere connection of a nerve with its ganglion must suffice for sensation. Now, that these special organs of sense have no such peculiar privilege is very evident, when we consider the facts. Without here insisting on muscular sensations and visceral sensations, on the multitudinous forms of sensation I have grouped under the head of Systemic consciousness, it is enough to refer to the sensations of Touch, Temperature, and Pain. No one will be hardy enough to deny that these are veritable sensations; yet these are not capable of being produced at all by the nerves of Sight, Smell, or Hearing. They have no special apparatus. There is no one tactile nerve and tactile ganglion: all the skin-nerves are nerves of touch, all produce sensations; and these sensations, there is ample evidence to believe, are produced when the whole of the brain, including the Medulla, is removed.*

This view of the relation of nerves to centres, and the consequent extension of sensation to all parts of the cerebro-spinal axis, is not so entirely novel as it may at first appear. The only real novelty is in its anatomical precision. Sir Charles Bell, to cite only one example, had a strong though dim conviction that it was an error to limit sensation to the action of the special senses. "It appears to me," he says, "that the frame of the body, exclusive of the special organs of seeing, hearing, &c., is a complex organ, I shall not say of sense, *but which ministers, like the external senses, to the mind;* that is to say, as the organs of the five senses serve to furnish ideas of matter, the framework of the body contributes, in certain conditions, to develop various states of the mind."[*] Although this is very vague, and, because of its vagueness, has found little acceptance, we see that the phenomena had not escaped Bell's attention. As he proceeds, he becomes still more vague; and thinks that because a blow on the eye will produce the sensation of a flash of light, which proves that the organs of the senses can be exercised when there is no *corresponding* outward impression, we can comprehend how other organs of the body may have a relation established with the mind without reference to outward impressions. As an anatomist, it behoved him to indicate by what kind of mechanism this relation was established. I have done so, by referring to the one mechanism which, in the case of the external senses, we know to be operative—namely, the connection of nerves with their ganglia: wherever a nerve excites a ganglion, there will be sensibility. The visceral-nerves excite their ganglia, the muscle-nerves excite their ganglia, the skin-nerves excite their ganglia, and the nerves of special sense excite their ganglia: the physiological process is in each case similar, although the sensations produced are necessarily various. Bell seemed to be very near the truth at one time, as may be gathered from the following passage:—"It is a singular fact in the history of physiological opinions, that the heart, an organ the most susceptible of being excited by the agitations or derangements of the body, should have been considered at one time as insensible. And yet in one sense it is true that it is so. To actual touch the heart is insensible, as was exhibited to the illustrious Harvey, in the person of a young nobleman who had the heart exposed by disease. This single circumstance, had there been no other evidence, should have earlier directed physiologists to a correct view of the matter; from its proving that the *internal organs are affected and united by sensibilities altogether different in kind from those bestowed upon the skin.* The sensibility of the external surface of the body is a special endowment adapted to the elements around, and calculated to protect the interior parts from injury. But though the heart has not this common sense of touch, yet it has an appropriate sensibility, by which it is held united in the closest connection and sympathy with the other vital organs."* Had Bell not shared the universal prejudice respecting the Brain as the exclusive seat of sensation, such considerations would have led him, I think, to the truth.

---

* The attempt to explain away such evidence, on the Reflex Theory, will be refuted by-and-by.

---

[*] BELL: *The Anatomy and Philosophy of Expression,* 4th edit. p. 83.

---

* BELL: *Anatomy, and Philosophy of Expression*, p. 86.

In the course of our demonstration that every ganglionic centre must be a seat of Sensibility, we have found that the majority of physiologists now regard the Medulla Oblongata as a sensational centre; and their reasons for so regarding it, both anatomical and experimental, we have said, apply with equal force to the whole of the Spinal Chord, of which, indeed, the Medulla is but the upper part. Having elicited this, we will now call attention to another interesting hypothesis which has been advanced respecting the Medulla.

IV. Is there a Vital Point?—It has been demonstrated that the Medulla Oblongata is the centre for the nerves of Respiration. The various actions involved in speech, in swallowing, sneezing, &c., are also co-ordinated here. It is because the actions concerned in Respiration have their centre here, that any other part of the spinal axis may be destroyed without destroying life; but destruction of the Medulla in mammals has been known, since the time of Galen, to bring with it instantaneous destruction of life. This fact was lighted up with sudden interest when Flourens announced that he had discovered a particular spot, a mere point, the extirpation of which caused instant death. He called this the *nœud vital*, or vital point, because every other part of the organism might be removed without death.

In that part of the grey substance of the Medulla called the "floor of the fourth ventricle," and, because it is shaped like a V, called the *calamus scriptorius* (nib of the pen), there is a smaller v to be seen (Fig. 54, *6*, p. 138). The point of this second v, not larger than a pin's head, is the "vital point," according to Flourens. When experiment had verified the assertion that death instantly followed the destruction of this point, physiologists quickly perceived that the explanation of the fact was not that here life had its special centre, but that here was the special centre for the co-ordination of the breathing mechanism. Indeed, the fact that reptiles will live for months without their heads, sufficed to disprove this notion of a "vital point."

Since this passage was originally written, I have learned that the very fact upon which Flourens rests his hypothesis, is not correctly conceived by him. Schiff and Brown-Séquard have both removed this "vital point" without destroying life—the animal continuing to breathe and live for five or six days. Their conclusion is, that when extirpation of the point causes death, it is because of the irritation of the neighbouring parts in the Medulla suddenly checking the action of the heart.*

V. INFLUENCE OF ONE CENTRE ON ANOTHER.—The reader will perhaps have felt some surprise in learning that the irritation of a nerve-centre arrests, instead of exciting, the action of the heart. The fact is indubitable, whatever may be the explanation. The heart, as we formerly learned (vol. i. p. 332), possesses its own independent ganglia, in virtue of which it continues pulsating long after death, and even after removal from the body. But if the pneumogastric nerves be galvanised, these nerves, although they send filaments to the heart, will not cause the heart to beat faster—they will arrest its beating. This was first shown in an experiment devised by Weber. He transmitted an electric current through the pneumogastric, and the heart suddenly stopped; after allowing it to resume its beats till they reached the normal rapidity, he again stopped them by another current.*

---

* SCHIFF: *Lehrbuch der Physiol.*, i. 323. BROWN-SÉQUARD: *Journal de la Physiol.*, i. 217. *et seq.*

That which is effected by the electric current may be effected by irritation of the Medulla Oblongata. The galvanic stimulus acts in the same way as the stimulus of the nerve-centre. There is one point, however, to which especial attention should be called. Although the electric current arrested the beating of the heart, in the experiments just noticed, it would have *quickened* the pulsation had the current been slighter. The same phenomena will follow a powerful and a moderate excitement of the Medulla: the powerful excitement arresting, and the moderate accelerating, the pulsations.

---

* This experiment has since been repeated in various ways. See ECKHARD, *Beiträge zur Anatomie und Physiologie*, p. 147. LONGET failed when he tried the experiment, owing to an incorrect application of the method.

Hence it is that sudden death may result from the "shock" of grief or of joy. Hence it is that a strong emotion makes the heart palpitate. Every one knows the effects of a burst of strong emotion: there is a momentary cessation of the pulse, followed by an agitation: at first our "breath is taken away," and then we shout, or cry, or tremble. Some faint under a powerful shock; others die.

While noticing this influence of the Medulla on the heart, we must not omit to mention the important fact that *pain* diminishes the heart's action. Indeed, pain will sometimes cause death in this way. When patients die under an operation, it is seldom from the loss of blood; it is from the effect of pain on the heart's action, combined with the previous weakening effect of the hemorrhage. This has been proved by experiment. On the other hand, Wilson Philip found that when a rabbit had been stunned by a blow on the head, its Medulla might be destroyed without an arrest of the heart ensuing.* Claude Bernard also found that if rabbits were rendered insensible by etherisation, their Medulla could be destroyed without destroying life; whereas, if the Medulla be even irritated without previous etherisation, death inevitably ensues from cessation of the heart's action.† The immense importance of chloroform, and other anæsthetic agents, in severe operations, is thus made physiologically apparent.

---

* WILSON PHILIP: *Experimental Inquiry into the Laws of the Vital Functions*, 1817, p. 72.

† CLAUDE BERNARD: *Système Nerveux*, i. 384.

It is a consequence of the wonderful complexity of our organism, in which each part plays on another, that remote and unsuspected influences produce important results. Mental agitation will suddenly arrest or increase the secretions; imperfect, or too abundant, secretion will depress, or confuse the mind. An idea will agitate the heart, and disturb the liver; a languid liver will disturb the serenity of the mind; a worm in the intestine will produce melancholy, and even madness.—So indissolubly is our mental life bound up with our bodily life.

VI. SUMMARY OF THE FUNCTIONS OF THE BRAIN.—We have now gone over, one by one, the cardinal facts which have been ascertained respecting the functions of the various parts of the Brain; and these are the chief results to which the inquiry has led:—

1. All the ganglionic masses have one property in common, which is Sensibility; and they differ only in their functional manifestation of this property, according to their connections with different organs.

2. The Cerebral hemispheres have the functions of Intelligence and Emotion. Probably also some other functions.

3. The Cerebellum has functions which remain at present undetermined.

4. The Medulla Oblongata has the functions of ministering to the respiratory and many other reflex actions. The special senses, Hearing and Taste, have their centres in this organ; as Sight and Smell have

their centres in the corpora quadrigemina and olfactory lobes.*

5. Inasmuch as all these centres have Sensibility, they must all be organs of the Mind, because they must all minister to the general Consciousness.

6. The facts and arguments which establish the conclusion that the Mind cannot be localised in any *one* part of the Brain, such as the Cerebrum, Cerebellum, the ganglia at the base, or the Medulla Oblongata, but that each of these parts has its own special functions, and contributes its own special forms of consciousness, lead to the suspicion that other parts of the nervous system may also contribute other special forms of consciousness.

It is this suspicion we shall examine minutely in the ensuing section.

---

* There are grave doubts respecting the olfactory lobes, which will be discussed in the next chapter (OUR SENSES AND SENSATIONS), but for the present we may follow the doctrine of the schools.

CHAPTER IX.

(Continued.)

THE MIND AND THE BRAIN.

SECTION III.—THE SPINAL CHORD AND ITS FUNCTIONS.

Structure of the Spinal Chord—Imperfection of our microscopical knowledge—Difference between an organism and a mechanism: the organic mechanism is vital and sensitive—Theory of Reflex-Action: Marshall Hall, Müller, and Prochaska—Error of excluding sensation—Grainger's advocacy of the theory—Schrœder's reflex-fibres—Sensation without the brain: experiments on insects—Marshall Hall's experiments—Spontaneous motion in headless animals—Reflex-action in the Brain—Hydrophobia—Voluntary and involuntary actions—What is volitional control?—Volition dependent on sensation—Growth of a volition—Control over our thoughts—Involuntary actions becoming voluntary—Spinal Chord a sensational centre: views of earlier writers; the deductive evidence; the inductive evidence—Evidences of spontaneity and choice in decapitated animals—Two centres of sensibility established in one animal—Are walking and swimming reflex-actions?—Experiments on puppies and rabbits—Examination of the evidence against the sensibility of the chord—What share has the Spinal Chord in our actions?

I. STRUCTURE OF THE SPINAL CHORD.—Formerly the Spinal Chord (or Spinal Marrow, as it is still sometimes called) was held to be a large nerve-trunk issuing from the brain, and giving off numerous branches which supplied the body. Although this idea is now completely abandoned, and the Chord is known to be a great centre, or series of centres, intimately connected, as the several vertebræ are connected into one spinal column, yet men still cling unconsciously to some of the old conceptions, and are disposed to regard the Chord as a *conductor*, rather than as a *centre* of Sensibility. It is this conception we must irrevocably dismiss from our minds.* If the Chord seems to act sometimes as a conductor only, it is merely because nerve-centres once excited play upon each other ; and in this sense the brain is also a conductor. Every segment of the Spinal Chord *may* act separately as an independent centre ; every segment is a little brain.

The Chord commences at that imaginary line where the Medulla Oblongata is supposed to end. It terminates in man about the second lumbar vertebra ; in most animals it extends along the whole length of the spinal column ; but there are great varieties in this respect. It is composed of six *external* columns of white fibrous matter—namely, two anterior, two posterior, and two lateral ; and of four *internal* columns of grey, ganglionic matter, called respectively the two anterior, and the two posterior, horns (*cornua*). A minute canal runs through the centre, which may be regarded as a prolongation of the fourth ventricle of the brain.

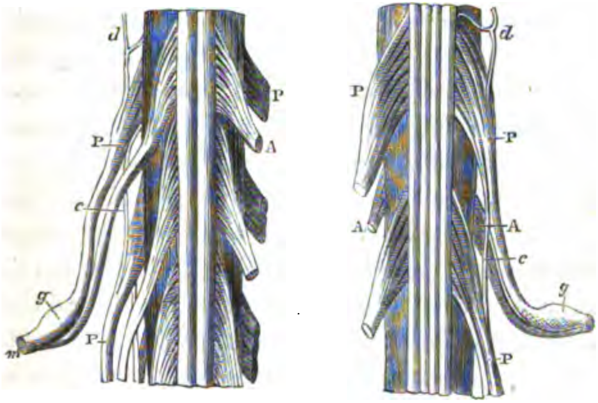
---

* It seems to me that the recent experiments of SCHIFF, BROWN-SÉQUARD, and others, which have excited so much discussion, and which attempt to determine the conductibility of different parts of the chord, are all vitiated by this initial mistake.

Fig. 55 represents the anterior and posterior aspects of the Chord with the double origin of its nerves.

As we are not here called upon to understand the whole anatomy of this organ, further details would be superfluous. The student will find ample information in the works named below.*

Fig. 55.



Spinal Nerves and Spinal Chord.—(After Bernard.)

1, Anterior view; 2, Posterior view. A is the anterior root of the nerve, and P the posterior root. One pair in each figure is represented with the division made below the ganglion (*g*). The other pairs are divided above the ganglion. At *c* and *d* are filaments connecting two posterior roots.

Of late years there has been an immense expenditure of labour in the attempt to unravel the microscopic structure of the Spinal Chord; but so excessively difficult is this task, that one may fear it will be a long while before Physiology will be in a condition to base safe conclusions on the results ascertained. This remark is especially addressed to students, as a warning against their being misled by the confident and plausible statements and deductions to be met with in modern works. I have been led to pay great attention to this subject; and have not only made a vast quantity of preparations of the chord of man, oxen, sheep, pigs (adult and newly born), cats, kittens, puppies, moles, rats, lizards, frogs, and fishes, many of which have been prepared according to the various methods employed by Stilling, Kölliker, Schrœder van der Kolk, Gratiolet, and Lockhart Clarke, so as to test the various statements of these authorities; but I have also had the advantage of seeing some of the best English and German preparations: the result of all this has been the conviction, that whoever propounds an hypothesis on the basis of the facts hitherto ascertained, is in danger of building on a sandbank.*

---

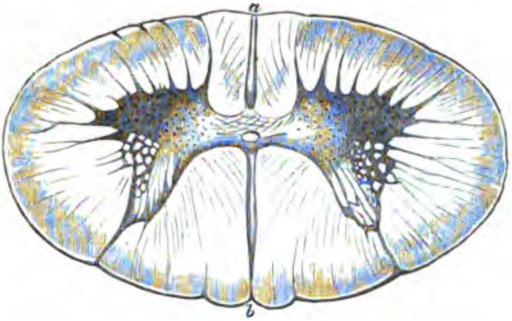
* Rolando: *Ricerche anatomiche sulla struttura del Midollo Spinale.* Grainger: *Observations on the Structure and Functions of the Spinal Chord.* Todd and Bowman's *Physiological Anatomy.* Todd: *Anat. of the Brain, Spinal Chord, and Ganglions.* Leuret et Gratiolet: *Anat. Comparée du Système Nerveux,* tome ii. Hirschfeld and Leveille: *Névrologie.* Quain's *Anatomy,* edited by Sharpey and Ellis.

A transverse section of the Chord, if sufficiently thin, and rendered properly transparent by any of the accepted modes of preparation, presents the appearance indicated in Figs. 56, 57, 58.

---

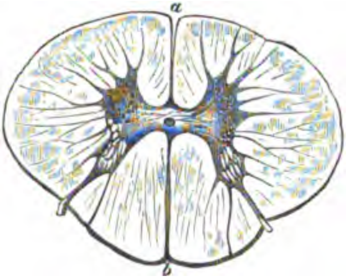
* KÖLLIKER, in his last edition, expresses the same scepticism. His words read like a sarcasm against SCHRŒDER VAN DER KOLK, whose confidence is astonishing, and whose published figures represent, I venture to say, what he *imagines*, not what can be *seen*. This will be evident to any one who adopts the method prescribed by SCHRŒDER; still more so, when we find that anatomist giving up his method in favour of LOCKHART CLARKE'S, which most certainly does *not* show what SCHRŒDER represents.

Fig. 56.



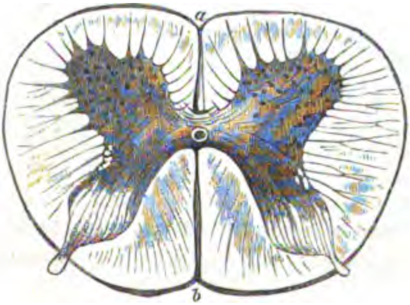
Transverse Section of Spinal Chord in Man (Cervical region). *a*, Fissure of the anterior columns; *b*, Fissure of the posterior columns. The central masses are the anterior and posterior horns of grey matter.

Fig. 57.



Transverse Section of the Spinal Chord (Dorsal region).

Fig. 58.

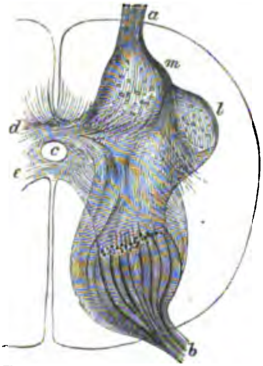


Transverse Section of the Spinal Chord (Lumbar region.)

These figures are copied from Gratiolet's Atlas. By way of comparison, we may consider Fig. 59, which is copied from Kölliker, and represents one-half of a section made in the lumbar enlargement of a child's chord.

Having made ourselves acquainted with these aspects of the internal structure, we may look a little closer at the groups of large ganglionic cells to be found in the anterior horns of grey matter. They vary considerably in shape and size. A general idea may be gained from Fig. 60.

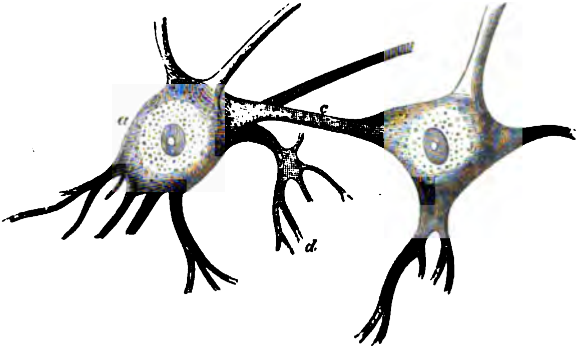
Fig. 59.



Transverse Section of Spinal Chord in a Child (Lumbar region).

a, Root of anterior nerve entering the columns of ganglionic cells *m* and *l*; *c*, Central canal; *d* and *e*, Anterior and posterior commissures; *b*, Root of posterior nerve entering the posterior horn of grey matter.

Fig. 60.



Nerve-Cells from Spinal Chord.—(After Gratiolet.)

These ganglionic cells are identical with those found in the brain; but because they are larger than the majority of those found in the posterior horn, they have been supposed to be motor-cells, by a logical process not very admirable, as it seems to me. The anterior roots of the nerves are, as you know, held to be motor only; the posterior roots sensory. We have already seen that this is a misconception; but even on the supposition that it is correct, we shall need some other evidence for believing that the anterior horn of grey matter is *therefore* motor. The notion seems to me entirely baseless. Still less can I accept the notion of Jacubowitch that the largest cells are motor, those of the second size sensory, and the smallest sympathetic. What may be seen, or imagined, in preparations which are imperfectly transparent, must yield to what is seen in preparations made on a better method,* and so transparent that one may read such characters as in a well-printed book. In such sections I affirm that so far from there being any necessary connection between large cells and the motor function, there is no constancy whatever as to size. In the ganglia of two indubitable motor-nerves—the *spinal accessory* and the *hypoglossus*—the size varies greatly. In the posterior horn there are not only large cells† (less abundant, indeed, than in the anterior), but the largest cell I have ever seen, was one with seven processes in the posterior horn of an ox. And while there are large cells in the posterior, there are abundance of small cells in the anterior horn. Indeed, the idea of properties so widely separated, as Sensibility and Motility, being dependent on slight differences in the size of identical cells, is eminently unphilosophical.

---

* See note at the conclusion of this chapter.

† BIDDER and KUPFFER deny the existence of these cells in the posterior horn. The same denial is made by OWSJANNIKOW: *De Medullæ spinalis textura*, p. 37; and by METZLER: *De Medullæ spinalis avium textura*, p. 30, who declares that what have been mistaken for nerve-cells are nothing but lacunæ in the web of connective tissue. In LOCKHART CLARKE'S preparations, and in my own, the cells are

But we must pause no longer on this microscopic structure;* we have ascertained that the spinal chord is a series of centres, having ganglionic substance in its interior, which is in connection with the roots of the motor and sensory nerves. In a word, it has the essential characters of a series of ganglia; and minute investigation discloses that these ganglia are essentially similar to those of the Brain. We shall never under-

---

unmistakable, and their well-defined nucleus prevents any possibility of doubt. From this one statement it is certain that the preparations of the Dorpat School must be very inferior in distinctness to ours; yet these writers profess to see, and they give drawings of, appearances which can by no means be made out in our preparations; whence it is easy to conclude that they describe what they imagine to be the real structure, not what can positively be seen.

* The student is referred to BIDDER *und* KUPFFER: *Untersuchungen über die Textur des Rückenmarks*, and KÖLLIKER'S reply in the *Zeitschrift für Wissenschaftliche Zoologie*, ix. 1; also KÖLLIKER: *Gewebelehre*, 3d edit.; LEYDIG: *Histologie;* LOCKHART CLARKE: *Philos. Transactions*, 1851 and 1853; LENHOSSEK: *Annales des Sc. Nat.*, 1857, vii.; LEURET *et* GRATIOLET: *Anat. Comp.*, ii.; SCHRŒDER VAN DER KOLK: *Bau und Functionen der Med. Spinalis und Med. Oblongata;* OWSJANNIKOW: *De Med. spin. textura;* METZLER: *De Med. spin. avium textura.* STILLING'S last work I have not seen; but it is said to be the most encyclopædic treatise yet produced.

stand the functions of the nervous system unless we fix firmly in our minds the conception of the cerebrospinal axis as one whole, the functions of the various parts differing from each other, only as they are differently related to the rest of the organism. Bergmann and Leuckart do not hesitate to define the "brain as the anterior enlargement of the spinal chord;" and this definition, they remark, makes it less paradoxical to find the brain so slightly developed in the lower vertebrate animals, and not existing at all in the Amphioxus.* In the invertebrate animals, there can be no shadow of doubt that the ganglia which represent the brain of higher animals are absolutely, and in all respects, identical with the other ganglia, which represent the spinal chord.

But our attention is now demanded by the important questions raised respecting the functions of the Spinal Chord. The chief of these functions (by most writers regarded as the sole function) is the production of movements in the trunk and limbs. These movements may either be produced in obedience to the mandates of the Brain—as in voluntary motions; or independently of the Brain—as in involuntary motions, which take place by what is called the Reflex-Action of the chord.

II. THE VITAL MECHANISM.—What is meant by Reflex-Action, and what is the Reflex-Theory, so continually invoked by modern writers? Several times already we have been forced to use these terms;[*] and now we have reached a stage in our inquiry when a distinct understanding of them becomes indispensable. But inasmuch as the Reflex-Theory implies the conception of the animal *organism* being to a great extent a *mechanism;* and further, inasmuch as our ordinary conception of a mechanism carries with it the idea of actions performed unconsciously, without sensation; it is absolutely necessary that we should accurately determine the sense in which we are to use, and understand, such a phrase as "the vital mechanism."

---

* BERGMANN *und* LEUCKART: *Vergleichende Anatomie*, p. 515. Compare also TODD: *Anat. of Brain and Spinal Chord*, pp. 2, 77.

The results of scientific investigation during many centuries have been gradually, and of late rapidly, tending to the conclusion that the animal organism is constructed on principles analogous to those which determine the construction of a mechanism. It acts like a mechanism; and differs from one, only in the greater *complication* of its parts and its effects. On the other hand, many persons feel a very natural repugnance to such a conception. Life seems to them the antithesis to mechanical action; the very notion of a mechanism is not that of life, but of a simulacrum of life. Without life, the organism is without action. We cannot imagine the watch performing its actions prompted by any sensation; we cannot imagine the organism performing its actions *without* life. This is the source of the repugnance, often felt, against conceiving the organism as a mechanism; but this repugnance will subside, if we reflect on the wonderful adjustment of part to part, in a complicated whole, and compare it with the somewhat similar adjustments of a mechanism; bearing in mind that this *similarity* is not *sameness*, and that the organism, although a mechanism, is a *vital* mechanism, this vitality being the source of profound differences. Some theorists have overlooked the important element of vitality; and have imagined that a vital, *sensitive* machine, can act like an *ordinary* piece of mechanism, which is not sensitive; but this error should not force us to give up a very useful analogy. We may still cling to the fact that the *adjustments* of the complex organism determine its activities: the muscles set moving by a peculiar stimulus, are as necessarily determined by their organic adjustments, as the wheels of a watch are set moving in a particular way, and are determined in their particular action by the peculiarity of *their* adjustments. When a stimulus to the respiratory nerves brings into play the group of muscles which expand the chest, and these, in turn, bring into play the group which contracts the chest—when the stimulus of food to the nerves of the alimentary canal determines the action of the digestive mechanism, muscular and secretory—when the stimulus of pain sets going the actions of flight, struggle, or shrieking,—we have a series of actions which are *mechanical*, inasmuch as they depend on the adjustment of parts, like the adjustment of cogged wheels; and are *vital*, inasmuch as they depend on vitality for their performance. With this qualification, the idea of a mechanical construction becomes important. We perceive how the various actions of an animal are dependent on the adjustment of its organs. Once set this mechanism going, and it can only act in the determinate direction permitted to its peculiar adjustments; and in so far it acts like a watch, or a steam-engine. The organism of the chick is so adjusted that it can peck up the grains; and it does so immediately the stimulus given to its sensational centre, through the optic apparatus, sets going the first of the necessary movements. It will do this as soon as it leaves the shell, or even before it leaves the shell. In like manner the Snapping Turtle, according to Agassiz, snaps as fiercely when it is still a colourless and undeveloped embryo, wrapped up in the fœtal membranes, as it does in after-life. This surprises us at first, because we are accustomed to attribute such actions to "a conscious and voluntary" effort. On getting over our surprise, and learning to regard these actions not as prompted by any distinct ideas, but simply as the results of an organism peculiarly constructed to act in this way—what we have to guard against is the error of those who, fixing their attention on the *mechanical adjustment*, overlook the fact of the *sensational guidance.* It is true that the animal mechanism, when set going, acts like the mechanism of a watch; but to *set* it going, and to *keep* it going, there is needed the constant presence of sensation. This has been frequently forgotten, and even denied. The mechanism has been credited with the whole phenomenon. That sensation is a necessary part of the mechanism—the mainspring of the watch—the fuel of the steam-engine—seems, singularly enough, to have been overlooked. The marvellous way in which the sensational element is bound up with the mechanical element, so that our very sensations may be said to be organised in us, is illustrated in a thousand examples of what are called "automatic actions." A piece of music with which we are familiar may be performed "mechanically," without any "effort" or "attention;" indeed, while we are talking to some one, or thinking of something else. This we are said to do "unconsciously;" and if Consciousness be restricted to Thought-Consciousness, there is no objection to calling these automatic actions unconscious; but there is every objection to calling them actions independent of sensation. The sensations excited by each note, and determining each movement of the muscles, are indispensable to the performance of music; the presence of *guiding sensations* suffices; but without this presence, the action is impossible. Although the mechanism of the fowl, whose cerebrum has been removed, is uninjured except as regards one of its sensational centres, this mechanism will not suffice to keep the bird from starvation amidst heaps of grain; because to set going the necessary train of actions which would result in pecking, there is needed the stimulus from the cerebral centre. If the grains be placed within the beak of this bird, they will not be swallowed, unless they roll near the sensitive part of the throat, and *then*, the stimulus of the sensation being given, the swallowing actions are set going, and on these follow the digestive actions. The theorists whom I have chiefly in view at present, deny that swallowing depends upon sensation—they call it simple reflex-action; an opinion which we shall presently examine. Enough, in this place, if we have made clear to ourselves the following positions:—

---

[*] See the explanation given at p. 55-59.

An organism is a mechanism, and acts mechanically, inasmuch as its actions are *necessarily determined by the adjustment of its organs.* But the organism differs from the mechanism in being vital, in having Sensibility as its mainspring; and its actions, which are called mechanical or automatic, are all *determined by the impulse of guiding sensations.* It further differs from a mechanism in its development of new adjustments and new capabilities. No machine has any self-development. What its parts could perform last week, they perform this week, and no more. But the organism develops. New adjustments are formed, so that a combination of actions, which to-day is impossible, will become easy, perhaps, at the end of a month. I do not, however, conceive it to be rational to suppose that a group of muscles which originally required the stimulus of a nerve-centre, as a guiding sensation, to move them, will at any time move without that stimulus; although it is very conceivable that the *secondary* sensations which the movements, when they were difficult, necessarily excited, should frequently be absent, now the movements are so easy that they claim no attention. I can conceive the possibility of *having* a sensation without *thinking* of it, and I can conceive automatic actions to be determined by guiding sensations, although the mind may not be "conscious" of them.

III. THE THEORY OF REFLEX-ACTION.—The muscular movements have from time immemorial been grouped under two heads: the voluntary, and involuntary. We shall have to consider the nature of this distinction presently, but must now inquire into that principle of Reflex-Action which, I conceive, underlies all the nervous phenomena, but which Marshall Hall conceived was characteristic of only a particular class. There were, he thought, four distinct classes: the *voluntary*, dependent on the brain; the *involuntary*, dependent on the irritability of the muscular fibre; the *respiratory*, wherein "the motive influence passes in a *direct line* from one point of the nervous system to certain muscles;" and the *reflex*, dependent on the "true spinal system" of *incident-excitor* nerves, and of *reflex-motor* nerves. These last-named actions are produced when an *impression* on the sensitive surface is conveyed, by an excitor-nerve, to the spinal chord, and is there *reflected* back on the muscles by a corresponding motor-nerve. In this process *no* sensation whatever occurs. The action is purely reflex, purely *excito-motor*—like the action of an ordinary mechanism.*

---

* MARSHALL HALL in *Philos. Trans.*, 1833. *Lectures on the Nervous System and its Diseases*, 1836. *New Memoir on the Nervous System*, 1843.

Müller, who shares with Marshall Hall the glory of having established this classification, thinks that although the absence of sensation is a characteristic of the reflex-actions, these actions may be, and are at times, accompanied by sensation. "The view I take of the matter is the following: Irritation of sensitive fibres of a spinal nerve excites primarily a centripetal action of the nervous principle, conveying the impression to the spinal chord; if the centripetal action can then be continued to the *sensorium commune*, a true sensation is the result; if, on account of division of the chord, it cannot be communicated to the sensorium, it still exerts its whole influence upon the chord; in both cases a reflex motor-action may be the result." *

This is, in brief, the Reflex Theory, which Marshall Hall and Müller have succeeded in making an European doctrine. There has been much discussion, often exploding on both sides into anger and injustice, respecting Marshall Hall's claims to originality. He has been accused of plagiarism from Prochaska; but I think that whoever attentively traces the history of the doctrine, will find that although Prochaska undoubtedly described the reflex-actions, and called them reflex, long prior to Marshall Hall—and, as it seems to me, conceived the whole phenomenon with far greater accuracy than was ever attained either by Marshall Hall or by Müller—yet it is very probable that Marshall Hall was unacquainted with his predecessor, who had long been dead, and whose theory had excited no attention; and it is certain that the theory propounded by Marshall Hall was *not* that propounded by Prochaska, but differed in essential points from it. This, however, is what neither Hall nor his adversaries seem to have known. So entirely had Prochaska's theory escaped attention, that even Müller, in announcing Hall's theory, and giving him the credit of priority over his own nearly contemporaneous discovery, never once alludes to Prochaska; and Arnold, in the historical sketch in which he enumerates those who before Hall had knowledge of the facts of reflex-action, is evidently unaware of Prochaska's claims.* When, therefore, Hall and Müller took up this subject, it was new to the scientific world; they succeeded in making the world accept their theory.

---

* MÜLLER: *Physiology*, i. 721.

The one fundamental point in this theory which seems to me wholly inadmissible, is the supposition that the reflex-mechanism is independent of Sensibility —that reflex-actions take place *without* sensation. And, unless I have wholly misunderstood Prochaska's general views, this is the point which he also would have considered erroneous. The reader may be glad to have Prochaska's exposition of the reflex-actions. We must premise that he always means to indicate the whole of the Spinal Chord, Medulla Oblongata, Optic Thalami, and the *crura Cerebelli et Cerebri*, when he speaks of the *sensorium commune:* "The reflexion of sensorial into motor impressions which takes place in the sensorium commune, is not performed according to mere physical laws, where the angle of reflexion is equal to the angle of incidence, and where the reaction is equal to the action; but that reflexion follows according to certain laws, writ, as it were, by Nature on the medullary pulp of the sensorium, which laws we are able to know from their effects only, and in nowise to find out by our reason. The general law, however, by which the sensorium commune reflects sensorial into motor impressions, is the preservation of the individual; so that certain motor impressions follow certain external impressions calculated to injure our body, and give rise to movements having this object, namely, that the annoying cause be averted and removed from our body; and *vice versâ* internal, or motor impressions follow external or sensorial impressions beneficial to us, giving rise to motions tending to the end that the agreeable condition shall be maintained.

---

* J. W. Arnold: *Die Lehre von der Reflex Function*, 1842.

"Very many instances which might be adduced, undoubtedly prove this general law of the reflexions of the sensorium commune, of which it may be sufficient to mention a few. Irritation being made on the internal membrane of the nostrils excites sneezing, because the impression made on the olfactory nerves by the irritation is conducted along them to the sensorium commune, there by a definite law is reflected upon motor nerves going to muscles employed in respiration, and through these produces a strong expiration through the nostrils, whereby the air passing with force the cause of irritation, is removed and rejected. In like manner it happens that when irritation is caused in the windpipe by the descent of a particle of food or a drop of fluid, the irritation excited is conducted to the sensorium commune, and there reflected on the nerves devoted to the movement of respiration, so that a violent cough is excited; a most suitable means for expelling the cause of the irritation, which does not cease till the irritant be ejected. If a friend brings his finger near to our eye, although we may be persuaded that no injury is about to be done to us, nevertheless the impression carried along the optic nerve to the sensorium commune, is there so reflected upon the nerves devoted to the motion of the eyelids, that the eyelids are involuntarily closed, and prevent the offensive contact of the finger with the eye. These, and innumerable other examples which might be brought forward, manifestly show how much the reflexion of sensorial impressions into motorial, effected through the sensorium commune, has reference to maintaining the conservation of the body.

"Since the *principal function* of the sensorium commune thus consists in the reflexion of sensorial impressions into motor, it is to be noted that this reflexion may take place either with consciousness or without consciousness."

We must interrupt the quotation here, to remark, that when Prochaska says reflexes may take place with or without consciousness,—or, to use his own phrase, *vel animâ insciâ, vel verò animâ consciâ*—he means "with, or without, *Thought*-consciousness," and not, as so often misinterpreted, "with, or without, *sensation.*" This distinction entirely accords with the view I hold, but is different from that generally held.

"The movements of the heart, stomach, and intestines," he continues, "are certainly in nowise dependent on the consciousness of the soul, for whilst no muscular movement can be excited unless a stimulus applied to the sensorial nerves passes by a peculiar reflexion to the motor nerves, and excites contractions of the muscle, it is at the same time certain that the reflexion of the impressions suitable for exciting those movements, if it takes place in the sensorium commune, is affected without consciousness [Thought-consciousness]. But it is a question whether these impressions, in order that they may be reflected, do really travel so far as the sensorium commune, or, without taking this long circuit, are reflected nearer in the ganglia from whence these parts derive many nerves? This matter is to be considered afterwards. But that reflexions of sensorial impressions into motor are effected in the sensorium commune itself, while the mind is altogether unconscious, is shown by certain acts remaining in apoplectics deprived entirely of consciousness; for they have a very strong pulse, breathe strongly, and also raise the hand, and very often apply it unconsciously to the part affected. The sensorium commune also acts independently of consciousness in producing the convulsive movements of epileptics, and also those which are sometimes observed in persons buried in profound sleep, namely, the retractions of pricked or irritated limbs, to say nothing of the motion of the heart and the respiratory acts. To this category also belong all those motions which remain some time in the body of a decapitated man, or other animal, and are excited when the trunk, and particularly the medulla spinalis, are irritated, which motions certainly take place without consciousness, and are regulated by the remaining portion of the sensorium commune existing in the medulla spinalis."*

It is clear, from this passage, that Prochaska, in explaining the actions which take place "without consciousness," did not mean that they took place without *sensation;* he meant that they were neither determined by, nor followed by, *thought.* He uses the word Consciousness as equivalent to Thinking; and this is a very general use of the term, nowadays, as we find in writers who declare that we cannot have sensations unless we *know* that we have them. In strict physiological language, this is tantamount to saying that we cannot have spinal Sensibility unless we have at the same time cerebral Sensibility—that the spinal chord cannot act independently of the Brain; whereas we know that it *can,* and does, act independently of the Brain; and it is to distinguish such actions that the term "reflex" has been invented. To suppose, as is commonly supposed, that the cerebrum *endows* the spinal chord with sensibility—that an impression only becomes a sensation when it reaches the Brain—is to suppose what the whole course of modern investigation tends to refute, namely, that the spinal chord is simply a conductor, and not itself a centre; and it is also to suppose that the Brain is *essentially different* from the Chord. Prochaska seems to have understood that the sensorium must first be called into activity before a motion can be produced; the sensations excited in this sensorium are quite independent of any thought which they may, or may not, excite in the Brain.

---

* UNZER and PROCHASKA *On the Nervous System.* Translated by Dr LAYCOCK, p. 430-2. The original may be seen in PROCHASKA: *Opera Minora,* pars ii., cap. 2.

It is here that Marshall Hall diverges from Prochaska, and, as I think, it is here his error begins. Yet, in common with his antagonists, he imagined Prochaska held reflex-action to be wholly divorced from sensation. His distinguished ally, Mr Grainger, whose work* is far more philosophical than anything Marshall Hall ever wrote, also insists on this absence of sensation as the distinguishing peculiarity of reflex-action.

Mr Grainger seems fully impressed with the philosophical canon to which we have constantly referred, that similarity of structure necessarily implies similarity of properties; but instead of concluding that the brain and spinal chord, being similar in structure, must have sensibility in common, he assumes a diversity of structure to correspond with an assumed diversity of function. The Reflex-Theory, he truly remarks, wants an anatomical basis: "unless a set of nerves distinct from those of sensation and volition be capable of demonstration, it is impossible to establish the correctness of what, without such evidence, is merely an hypothesis." Accordingly he set himself the delicate task of demonstrating this distinct class of nerves. Sharing the prejudices of that day against the use of the microscope, and refusing to use a lens for fear of deception, his task became impossible. In the twenty years which have since elapsed, men have learnt to use the microscope with more security, and in the last ten years they have learnt how to make preparations, which, if they have not taught us the whole structure of the spinal chord, must in the course of time lead to accurate knowledge. Anatomists have long rejected the supposed system of excito-motory fibres.

---

* GRAINGER: *Observations on the Structure and Functions of the Spinal Chord,* 1837.

[While these sheets are passing through the press, I find Schroeder van der Kolk, whose statements, as I have before hinted, are to be received with extreme caution, describes a system of reflex-fibres, with a confidence rather surprising when we scrutinise his evidence. He maintains that each posterior root, on entering the white columns of the chord, divides into two bundles; the one bundle, containing purely sensory-fibres, runs straight up to the brain; the other bundle, containing purely reflex-fibres, runs into the grey matter of the chord, where the fibres terminate in the ganglionic cells. It is, he says, because the posterior root contains these two kinds of fibres, that it is larger than the corresponding anterior root. On this I have to remark, first, that the facts on which this hypothesis is based are excessively questionable; secondly, that the supposed confirmation drawn from the larger size of the posterior root is worthless, inasmuch as the fact is only true with regard to some animals—in the frog, for instance, the posterior root is *smaller* than the anterior; and finally, that if the anatomical facts were as he states them, his conclusion would not be less rash and hypothetical. For let us grant that one fibre enters the grey substance of the chord, and there terminates in a ganglionic cell, whereas another fibre runs up to the Medulla Oblongata, and there terminates in a ganglionic cell (which is the real statement of his position, stripped of equivocal language), what difference, of an essential kind, is here established, such as could justify the distinction of sensory and reflex fibres?]

The anatomical basis for the Reflex-Theory is still to seek. Indeed, were a distinct set of nerves to be discovered, they must be in connection with nerve-centres; and inasmuch as the reaction of a nerve-centre is Sensibility—inasmuch as *all* nerve-centres must have the *same property,* in spite of any difference in their *functions,*—it is clear that the reflex-fibres would excite Sensibility, if they excited the centre.*

---

* A friendly critic objects: "How, in this case, do you explain one nerve-centre appreciating only sound, and another light, and another flavour? Rigorously interpreted, your position would involve that all sensations are the same." By no means; my position is, that all sensations are forms of Sensibility, and their differences result from variations in the nature or degree of the stimulus. The same skin-nerve yields to different stimuli the very different sensations of touch, warmth, cold, tickling, pressure, pain, &c. The subject will be treated more fully in the next chapter.

Let us, however, see what physiological observations can be adduced in favour of the reflex-theory.

"We have lastly to inquire," says Müller, "how far true sensation is engaged in the production of reflex motions. Volkmann inclines to the opinion of Whytt, that the motions consequent on impressions are the result of sensations conveyed to the sensorium, giving rise to spontaneous reactions. That this is in many instances the case, appears to me indubitable, particularly with those reflex phenomena which occur in an unimpaired state of the brain and spinal chord. Thus I regard, for example, the closure of the eyes under the stimulus of a strong light, and the action of the respiratory muscles induced by irritation of the mucous membranes of the respiratory organs, or intestinal canal." Note this admission, and then consider the counter-argument he brings forward: "But when we remember that, if we divide the trunk of a salamander into several portions, each part, if it contain a fragment of the spinal chord, will still evidence the reflex motions, we can scarcely maintain the applicability of this view to all cases." *

Here the question naturally rises: Why can we *not* suppose the segments of the chord to be sensitive, and its actions to be determined by sensations, as well when the brain is absent, as when the brain is present? The only obstacle is in the prejudice respecting the brain being the sole seat of sensation (compare page 84). But look at the facts. We etherise a frog,

---

* MÜLLER, 720.

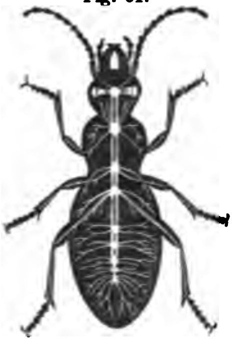
and cut off its head; after which we divide its spinal chord, in the region of the back. We have then two segments of a headless animal. When the effect of the ether has disappeared, we pinch, or prick, the fore-leg, and it is at once withdrawn; we do the same to the hind-leg, and it is withdrawn; we irritate the posteriors, and the hind-legs vehemently push the instrument away. Another frog, also decapitated, hops away when thus irritated; but this one, with the divided chord, cannot hop. All these actions of the decapitated animal are, we are told, purely reflex, totally without sensation; "because sensation is only possible when the brain is present." In the uninjured frog these actions result from sensations, because the impressions on the skin are *conducted* to the brain. All this seems clear enough until we extend our observations. Let us decapitate a frog, and see whether the head, separated from the body, will not exhibit the same signs of Sensibility which are exhibited by the body separated from the head. We touch the eye, and the eyelid closes; we throw a strong light on the eye, and the pupil contracts and the lid closes; we touch the tongue, or the inside of the mouth, and the larynx makes a motion of swallowing. All these are precisely similar to what occurs in the uninjured frog, and are clearly allied to the actions of the headless trunk; yet Physiologists would have us believe that these actions, both of head and trunk, which indicate sensation when the animal is entire, do *not* indicate sensation when the head is separated from the body; they would have us believe that the motions of the eyelid, pupil, and larynx, in the separated head, are purely reflex, wholly destitute of sensation. I confess this seems to me very arbitrary. The headless trunk is first said to be incapable of sensation, because the brain cannot be reached; and then the head itself is said to be incapable of sensation, although impressions on it *do* reach the brain. Does, then, sensation depend on the union of the spinal chord with the brain?

But however arbitrary it may seem to deny sensation to the separated head, and to consider its actions as purely reflex, there is absolute necessity for such a position, if the Reflex-Theory is to be upheld; because, if we grant sensation to the *separated head*, when we see it responding in the usual manner to stimuli, we can have no grounds for denying sensation to the *separated body*, when we see it responding in the usual manner to stimuli.

In the numerous experiments I have made with a view to a right understanding of this subject, those on insects have furnished decisive examples. When a wasp is cut in two, both halves live, and manifest Sensibility (if we assume that a wasp *ever* manifests it, and is not a mere machine) during three or four days. If you irritate one half—the head—it will *bite;* if you irritate the other half—the tail—it will *sting.* Take a beetle, such as the large water-beetle, *Dytiscus marginalis*, and remove its head. Here is one, which was decapitated eleven hours ago. At first the head twittered its antennæ, and moved its mandibles, just as it had done before decapitation: this was evidence of *spontaneous* action. It also frequently moved the larynx. Sometimes it spun round on the table by means of its long antennæ. When quiet, if I touched it, these motions were resumed. It seized any object with its mandibles, and bit fiercely. About ten hours ago all spontaneous action ceased; but any irritation produced the reactions of sensibility. All this while the *headless trunk* was swimming rapidly, and, if irritated, behaved precisely as the trunk of an *uninjured* insect behaves.

Once more, I ask, How can we refuse this evidence of sensibility, unless we boldly deny that the insect has sensibility at all? If we accord sensibility to the entire beetle, we must accord it to the separated head; and if to the separated head, then, on the same grounds, to the separated body. Cast a glance at the nervous system of the beetle, and you observe that the ganglia in the head are similar to those in the trunk, and are only united with them by two threads of nerve-fibres. Is it rational to suppose that the mere fact of union of such threads can endow actions with sensation, when we know that the actions themselves are the same whether the union exist or not; and the only difference created by a division is, that the actions of the fore-part are no

Fig. 61.



NERVOUS SYSTEM OF GARDEN BEETLE.—(After Milne-Edwards.)

longer consentaneous with the actions of the hind-part? The nervous communication enables the two parts of the insect to *act together;* it does not, and it cannot, give sensation where no Sensibility existed. According to the view I take, Sensibility is the property of every ganglion, just as Contractility is the property of every muscle. And carrying this idea with us, we shall find experimental evidence in abundance; for, suppose we cut off one segment of an animal's body, the loss of this segment will not entail a loss of sensibility in the *rest* of the body; a dog, or a lizard, may lose its tail without losing its susceptibility to impressions. Let us now remove another segment. The insect continues to fly, walk, bite, &c., as before. From this sensitive, though truncated, insect, we continue to remove segment after segment, till we reach the last segment, or head. In the course of these operations we find the insect more and more crippled, but at no point ceasing to manifest sensibility: the head gives unequivocal signs, just as the whole body, *minus* one segment, gave unequivocal signs. There is but this alternative: either we must deny that the insect at any time has sensibility; or we must assert that only in the ganglia of the head does sensibility reside. The first of these positions would render Physiology silent altogether, and indeed would render it impossible for us to draw any conclusions respecting man; since it is only by the similarity of their actions with our own that we can conclude men to have feelings similar to our own: each man can only have direct knowledge of his *own* consciousness. The second position of the alternative, which makes Sensibility the exclusive property of the cephalic ganglia, is an assumption contradicted on the one hand by the *anatomical identity* of all ganglia, and on the other hand by the *physiological identity* of the actions of head and trunk, when separated.

That I am not doing the upholders of the Reflex-Theory any injustice in asserting that they call the actions of the separated head purely reflex, without sensation, will be apparent from the following passage from Marshall Hall's Memoir:—He decapitated a turtle. "The head being placed upon the table for observation, it was first remarked that the mouth opened and shut, and that the submaxillary integuments descended and ascended alternately from time to time, replacing the acts of respiration. I now touched the eye or eyelid with a probe. It was immediately closed: the other eye closed simultaneously. I then touched the nostril with the probe. The mouth was immediately opened widely, and the submaxillary membranes descended. This effect was especially induced on touching the nasal fringes situated just within the anterior part of the maxilla. Having made these observations, I gently withdrew the medulla and brain. All the phenomena ceased from that moment. The next observations were made upon the other parts of the animal. The limbs, the tail, were stimulated by a pointed instrument or a lighted taper. They were immediately moved with rapidity. The sphincter was perfectly circular and closed: it was contracted still more forcibly on the application of a stimulus. On withdrawing the spinal marrow gently out of its canal all these phenomena ceased." The conclusions drawn from these observations are not a little surprising to any one who has a sense of logical proof. "This experiment," he continues, "affords evidence of many important facts in physiology. It proves that the presence of the medulla oblongata and spinalis is necessary to the contractile function of the eyelids, the submaxillary textures, the larynx, the sphincters, the limbs and the tail, on the application of stimuli to the cutaneous surfaces or mucus membranes [which no one doubted]. It proves (!) the reflex character of this property of the medulla oblongata and spinalis, and the dependence of these motions upon the reflex function. It proves that the tone of the limbs, and the contractile property of the sphincter, depend upon the same reflex function of the medulla spinalis—effects not hitherto suspected by physiologists." *

Now what is the proof here afforded? The facts are, that the limbs and tail contract, when stimulated, although the head be removed from the body. This is assumed to be a proof that such contractions take place without sensation, because the Brain is assumed to be the sole seat of sensation. "In decapitated animals the *very organ* of sensation and volition, and the other mental faculties, is removed." † But unhappily for this argument, the facts cited by him show, that when the "very organ" itself is present—in the severed head—precisely similar phenomena are observed. If the brain is the very organ of sensation, it will manifest sensibility as long as any vitality remains, whether joined to the body or severed from it: just as muscles manifest contractility, and the heart beats, when separated from the body: it is but a feeble manifestation, I admit, but as long as there is *any* manifestation, it will be of the *organic function peculiar to the organ.* In most animals, the loss of blood, and the shock, destroy at once all manifestation of sensibility in the severed head;* but in reptiles, batrachians, and insects, the property still endures for a short period.

---

* Marshall Hall: *Philos. Trans.* 1833, p. 644.

† Marshall Hall: *New Memoir on the Nervous System*, p. 32.

Does not the reader perceive that if the head, containing the "organ of sensation," after being severed from the body, manifests any of those signs of sensibility which it manifests when joined to the body, we can have no right whatever to say these *identical effects* are due to *different causes?* Does he not further see that if the phenomena observed in the *body separated from the head,* are precisely similar to those observed in the *uninjured animal,* and to those observed in the *severed head,* these identical phenomena cannot be due to different causes? Deny that the separated body manifests sensibility, and you must deny that the separated head manifests it; but you cannot stop here, you must also deny that the uninjured animal manifests it.

---

* Were it not so, the sufferings of a man beheaded would be frightful. But we know, from experiments on mammals, that the loss of blood causes death before the shock passes away.

That is the interpretation I put upon the facts. The one suggested by Marshall Hall rests on the double assumption that the Brain is the exclusive seat of Sensibility (an assumption of the very point in question), and that unless the Brain be in vital connection with the rest of the body, all Sensibility is impossible. Of *proof* there is not a shadow. Indeed, I cannot help an expression of surprise at the weakness of the evidence on which this celebrated Reflex-Theory is based. Except the striking facts noted of human beings, with injured spines, who were insensible to prickings and burnings—facts which we shall hereafter see do not in any way contradict the views put forth in these pages, and do not, when properly considered, give the Reflex-Theory any countenance—there is not a single fact, or experiment, which can make us pause. As to Marshall Hall's few experiments, the facts are so inaccurately stated by him, or, when accurate, are so illogically interpreted, that a feeling of the ludicrous is irresistible when we hear him declare: "*Nothing can affect my view* of the subject, which is, as I have stated, the *simple expression of the facts.*"

Let us give Marshall Hall due honour for his ingenuity and his steady pertinacity against all opposition, but let us also rigidly question the claims even of the most honoured. I do not think that accuracy of observation, or of reasoning, were his strong points; and in the course of the following pages the reader will see grounds for this judgment. Meanwhile two important misstatements call for notice. One of the cardinal "facts" in Hall's theory is, that decapitated animals never move unless *excited* to motion by an *external* stimulus. This fact I positively deny. Had he kept frogs and tritons for days after decapitation, or weeks after division of the spinal chord, he would have observed frequent refutations of his hasty conclusion. Had he even remembered Redi's account of the tortoise, which, after the whole cranial cavity had been completely emptied, *walked* about as usual, and lived for five months,* he would never have ventured on such a statement.

Some time ago I removed the brain from a frog, and left it on a plate to recover from the effects of ether. The next morning the servant came to me, with suppressed alarm, assuring me my frog would escape. "No, there's no danger. It can't escape, its head is off." "But I assure you, sir, it's quite lively; I thought it would jump off the table." On going upstairs, I found the animal in the middle of the room. Such things are of frequent occurrence. Dr Inman, of Liverpool, writes me word that he has witnessed it on several occasions. He completely emptied the cranial cavity of a frog, yet found the animal quite vivacious: "There is no lack of spontaneous movement, and the reflex-actions are distinct enough in the eye, eyelid, and other places." Indeed, no one has investigated this subject without seeing abundant evidence of spontaneous movements after removal of the brain. These movements are, it is true, less evident when the Medulla Oblongata has also been removed ; but that is simply because lung-respiration depending on the Medulla, and vital energy on respiration, the removal of the Medulla is too serious an interference with the relations of the organism to permit more than feeble evidences of spontaneity. Nevertheless, when the spinal chord is divided, the *direct* influence of the Medulla is removed from the nervous centres behind the section, but its *indirect* influence, in keeping up an active nutrition of those centres, enables the parts behind the section to move spontaneously. A Triton, or Lizard, with a divided chord, will be seen to move its tail and hind-legs spontaneously ; *i. e.*, without any *external* stimulus being applied.

---

* REDI: *Observ. circa Animal. Vivent.*, p. 209.

When I use the phrase external stimulus, it is in the sense implied by Marshall Hall and Mr Grainger, meaning some irritation from the contact of a hard substance, an acid, or the flame of a taper. But as a change in the temperature of the room, a current of cold air, or the effect of exposure on the wound, may equally well range under the category of external stimuli, I think it important to show that a decapitated frog will move spontaneously when there is no external stimulus possible. The frog is first etherised ; and then, its brain having been carefully removed, the whole of its skin is stripped off. The effect of the skinning is to render it totally insensible to any external impressions : you may pinch, prick, tear, burn the flesh, or cut off the limbs bit by bit, without producing the slightest sign of sensation. Yet this frog will hop spontaneously; and if you draw its hind-legs from under it, and leave them outstretched, it will draw them up again in a few minutes; thus proving that motions may take place in a brainless animal when all external impressions are rendered powerless. Moreover, I found that a triton, *divided in two*, had spontaneous movement in *both* halves: I placed the tail-half under a glass, and kept the air within moist, to prevent evaporation from the skin, yet even here this fragment of an animal occasionally moved.

So much for the cardinal fact in the Reflex-Theory—a fact which is to prove that the motions of a decapitated animal are *excited*, but not *felt*. I conceive that *all* actions, in all animals, are excited, and all are felt: an unexcited motion is inconceivable; but some motions are excited by *external*, and others by *internal* stimuli.

The second point to be noticed here, illustrates Hall's somewhat loose manner of conducting experiments which require rigid caution; and the equally loose manner in which he drew conclusions. "I divided," he says, "the spinal marrow of a very lively snake between the second and third vertebræ. The movements of the animal were immediately before extremely vigorous and unintermitted. From the moment of the division of the spinal marrow, it lay perfectly tranquil and motionless, with the exception of occasional gaspings and slight movements of the head.

It became quite obvious that this state of quiescence would continue indefinitely, were the animal secured from all external impressions." So far from this being "obvious," it is directly counter to fact. The mere division of the spinal chord gives a "shock," which for a time keeps the animal motionless, or convulsive, but never prevents subsequent spontaneity of movement. But let us continue his account: "Being now stimulated, the body began to move with great activity, and continued to do so for a considerable time, each change of position or situation bringing some fresh part of the surface of the animal into contact with the table or other objects, and renewing the application of the stimulus. At length the animal became again quiescent; and being carefully protected from all external impressions, it moved no more, but died in the precise position and form which it had assumed."

A closer reasoner would have seen that the snake's movements — if originated by a *sensation* derived from an irritation — would subside with the subsidence of the sensation; but they could never subside —while vitality remained—if the mere *excitation*, caused by contact of the table, originated each movement; since this excitation must be *renewed* at each movement of the snake, and thus one movement would necessarily bring on the next. But Marshall Hall set logic at defiance. On this very page he declares, that when a decapitated animal has relapsed into the repose from which it will never rouse itself, unless stimulated externally, "the slightest touch with a hard substance, the slightest stimulus, will renew the movements;" and he thinks that these movements *cannot* depend upon sensation, because the position into which the animal falls may be one which would be attended with extreme pain if sensibility remained—*e. g.* when the animal remains suspended over the acute edges of the table. Had he stopped here, one might have attached some weight to the argument; but he proceeds—"the infliction of punctures and the application of a lighted taper did not prevent the animal, still possessed (?) of active powers of motion, from passing into a state of complete and permanent quiescence."

As the "observation" is one on which Hall lays great stress, the reader is requested to notice the remarkable confusion in the statement. First we are told that the slightest touch with a hard substance suffices to renew the movements; and then we are told that the proof of these movements not being due to sensation, is that punctures and burnings produced *no* such renewal of movements—as if punctures were effected without "the slightest touch of a hard substance!" The truth of the case, so ill observed by him, is this, that the snake, when thoroughly exhausted, did not move, if punctured or burned; but before that exhaustion had taken place, punctures and burnings could have made it writhe, and any other stimulus would have made it move. On the whole, we cannot but agree with Arnold, when he says that Hall one moment relies on the fact that decapitated animals move, as a proof that movements may take place without sensation; and the next moment proves the absence of sensation by the fact that the animals do not move.*

These examples will suffice to show how little Marshall Hall's experiments support his theory. Let us glance at another example which is adduced in favour of the theory: "Mr Grainger found," says Dr Carpenter, "that he could remove the entire hind-leg of a salamander with the scissors without the creature moving, or giving any expression of suffering, if the Spinal Chord had been first divided; yet that by irritation of the foot, especially by heat, in an animal similarly circumstanced, violent convulsive actions were excited in the legs and tail. This fact is important, not only as showing the comparatively powerful effect of impressions on the cutaneous surface, but also as proving how little relation the amount of reflex-action has to the intensity of sensation."

On this we must remark that the fact is altogether misstated, owing to theoretic preconceptions. When Mr Grainger states that the creature did not move, or give any expression of suffering, he means, I presume, that the fore-part of the creature remained quiet; which is no more than would be expected, since the division of the spinal chord had divided the creature into two distinct parts—as regards sensibility. The fore-part was quiet, because the fore-part was not touched. But I venture to say, the hind-part was not quiet; *that* always writhes when the leg is cut off. And although the leg may be cut off from this hind-

---

* ARNOLD: *Die Lehre von der Reflex Function*, p. 54.

part without producing any considerable writhings, such as are produced by heat or other irritation, yet this by no means warrants the conclusion drawn by Dr Carpenter; and for these reasons: the sensibility of a Salamander to external stimuli is almost exclusively cutaneous; stripped of its skin, *no* amount of cutting, or pricking, produces the slightest sensation: when therefore the leg is cut off, if the scissors be very sharp, and the movement rapid, the animal scarcely responds to the stimulus; but if the scissors be not very sharp, and the cut be made slowly, the animal—or a segment of the animal—writhes energetically. An irritation of the skin, by heat or other stimulus, will, of course, produce greater writhings than cutting off a leg: but this will be the case with the uninjured animal, no less than with the animal whose chord has been divided. Thus we see that the fact in no way supports the hypothesis of reflex-actions being produced without sensation.

Indeed, I know of no facts whatever which justify the hypothesis, but abundant facts irreconcilable with it. These latter facts we shall have to consider presently, in treating of the sensational function of the spinal chord. If we are to limit sensation to the brain, it is obvious that actions which take place without a brain, must be actions without sensation. But this is the point which has to be proved, not assumed; and it cannot be proved by such facts or arguments as have hitherto been advanced. Actions may be determined by cerebral, or by spinal, sensibility; to deny sensibility to the spinal chord, because its sensations may in many respects be unlike those of the brain, is as unphilosophical as to deny sensibility to the cerebrum, because its sensations are unlike those of the optic or auditory centres: *and this has actually been maintained.* Various writers have denied consciousness to the cerebral hemispheres. Nor, until we adopt language having scientific precision, is there any ground for objecting to such a conclusion; since it is perfectly clear that the same principle of reflex-action takes place in the cerebrum, as that which we have noted in the spinal chord.

IV. **Reflex-action in the Brain.**—It was suspected by Prochaska, and has since become quite ascertained, that reflex-actions would take place from the Sympathetic Ganglia, without the participation of the Spinal Chord. The proof is simple. The whole spinal chord and brain may be destroyed, yet the heart will continue to beat, and the intestines to move.

But a more important step was to be taken: the Brain was to be included among reflex centres, and some at least of its actions reduced under the general law of reflexion. This step was taken by Dr Laycock in 1840. In a striking paper* read by him at the British Association in 1844, he brought together the evidence on which his view was founded. The idea has been adopted and illustrated in the writings of Dr Carpenter, who now calls the action "unconscious cerebration." If the modern-reflex theory be adopted, this extension of it to the brain is inevitable. But it seems to me that unconscious cerebration is nothing more than this: we can have a sensation without thinking of that sensation; and in like manner we can think without thinking that we think—a train of ideas may go on without exciting a particular direction of the consciousness.

---

* Printed in the *British and Foreign Medical Review*, Jan. 1845.

"I was led to this opinion," Dr Laycock says in announcing his view, "by the general principle that the ganglia within the cranium being a continuation of the spinal chord, must necessarily be regulated as to their reaction on external agencies by laws identical with those governing the spinal ganglia and their analogues in the lower animals." If, therefore, the spinal chord is a centre of reflexion, the brain must also be one. It is a matter of regret that Dr Laycock did not extend this principle, and declare that whatever was true of the *properties* of the cranial centres, must also be true of the spinal centres; if the brain have Sensibility, the spinal chord must also have it.

Dr Laycock refers to the curious phenomena of Hydrophobia in proof that reflex-actions may be excited by the optic nerves, or by a mere idea of water. When a mirror was presented to a patient, the reflection of the light acting on his retina, in the manner of a reflection from the surface of water, produced a convulsive sobbing, as in the attempt to swallow water, and the patient turned aside his head with expressions of terror. Money was given him to induce him to look a second time, but before he had looked in it a minute, the same effect was produced.

The *idea* of water excited similar convulsions. No sooner was it suggested that the patient should swallow a little water than he seemed frightened, and began to cry out. By kindly encouragements, he was brought to express his willingness to drink, but the *sound* of the water, as it was poured out again, brought on convulsions. In another case, " on our proposing to him to drink, he started up, and recovered his breath by a deep convulsive inspiration. On being urged to try, he took a cup of water in one hand, and a spoon in the other. With an expression of terror, yet with great resolution, he filled the spoon, and proceeded to carry it to his lips; but before it reached his mouth, his courage forsook him, and he was forced to desist. He repeatedly renewed the attempt, but with no more success. His arm became rigid and immovable whenever he tried to raise it to his mouth, and he struggled in vain against this spasmodic resistance."

The facts of reflex cerebral action are important illustrations of the law of nervous phenomena, but they do not in any way establish the peculiar assumption of the Reflex-Theory, which is, that actions can take place without the excitations of Sensibility.

Dr Carpenter, adopting the opinions of Marshall Hall and Dr Laycock, thus classifies all reflex-actions: 1. Those determined by the Spinal Chord; they are " excito-motor." 2. Those determined by the Sensory Ganglia; they are " sensori-motor." 3. Those determined by the Cerebrum; they are "ideo-motor." In all these consciousness is said to be absent.

Here we must once again call attention to the ambiguity of ordinary language. Consciousness is a word which, although used as equivalent to sensation, is often, and more frequently, used as equivalent to thought. To have a sensation, and to be conscious, are held as convertible terms: there can be no sensation without consciousness. But there is frequent use of phrases implying that sensations *may* exist without consciousness, as when we say, "I heard the clock strike, but was unconscious of it; I wound up my watch, but did so unconsciously." This is the meaning which Dr Carpenter seems to attach to the word; and with that meaning, there is no impropriety in his speaking of "unconscious cerebration," which some have ridiculed, as if it were tantamount to "unthinking thinking." There is no doubt that we go through many mental processes without any of that reflex-feeling which is characterised by the phrase "being conscious." The train of ideas may never diverge from the direct path: a problem may be solved, and the mind will be so intent on the solution as to be wholly "unaware" of anything else. During reverie we are not only "unconscious" of the presence of external objects, but of our own state. The intellectual mechanism acts without interruption from sensation. When the word consciousness is restricted thus, we may properly say that there can be unconscious thinking, and unconscious sensation. It is only saying that some centres of our complex organism may act, without at the same time calling some other centres into play.

The perplexing ambiguities which necessarily arise when different writers use the words sensation and consciousness, without rigorously defining their terms, I have endeavoured to avoid, by uniformly employing the word Sensibility as a general term indicating the property of *all* nerve-centres; and the word Sensation as a general term including every reaction of Sensibility to a stimulus, external or internal. By so doing, all nervous phenomena are brought under the same law; as all muscular phenomena are brought together when Contractility is assigned to muscular fibre, and Contraction to the activity of that property. A muscle may be flexor, or extensor, or sphincter; its function may be to serve expression, respiration, locomotion, voluntary or involuntary movements; but in all cases the actions depend on Contractility. The muscular mechanism, like the nervous mechanism, is so related in its parts, that some of them cannot be called into action without also playing upon others; but there are parts which can act with a certain independence. The arm may be moved without any alteration of the muscles of expression, yet it cannot be moved without affecting the muscles of respiration. But whether the parts act separately, or in combination, the necessary condition of all muscular action is Contractility. In like manner, whether the various nerve-centres act separately or in combination, the necessary condition of all ganglionic action is Sensibility.

I have thought it desirable, on many grounds, to make Consciousness an equivalent term for Sensibility, and have grouped the various forms of Sensibility under the heads of Thought-consciousness, Sense-consciousness, and Systemic-consciousness. But this is a point which may be left to the pleasure of the reader. The thesis maintained in these pages does not require assent to such a classification; but it asserts, as a scientific proposition, that Sensibility is the property of all nerve-centres; and that no action can possibly take place in the organism which has not a sensation, *of some kind*, as a basis. The Reflex-Theory, which considers the organism as a mechanism, errs in supposing that this mechanism can dispense with Sensibility. I also hold that the organism is a mechanism—but a vital and *sensitive* mechanism. It was no discovery of Marshall Hall's that many actions could take place, and did take place, without our being "conscious" of them—meaning thereby, without our *attending to*, or *thinking* of them. Every one knew that. His discovery purported to be, that whereas *some* actions have a sensational origin, and can only be performed in virtue of a sensitive mechanism; there are *other* actions which have *no* such origin, but which, like the actions of a steam-engine, depend on the reflexion of impressions into motions, one cogged wheel moving another. In this he made the immense mistake of forgetting that in the *vital mechanism each cog is a sensation*. Unless his purely mechanical view be adopted, the Reflex-Theory is merely a new name; and the reader has already seen how little evidence there is in favour of such a view, how much against it. The evidence against it will become more and more coercive as we proceed. But it is necessary for a right understanding of the claims of the Spinal Chord to the title of a sensational and *volitional* centre, that we should examine with some attention what constitutes—in a physiological sense—a volition.

V. VOLUNTARY AND INVOLUNTARY ACTIONS.—It seems an easy thing to distinguish a voluntary from an involuntary action; and yet this seemingly easy thing sorely perplexes the cunning of philosophy. It seems also an easy thing to distinguish between an animal and a plant; yet when we come to seek for the one distinctive characteristic which marks the animal world, and separates it decisively from the vegetable world, we are sorely puzzled. There is no difficulty in saying that a cow is an animal, and a cabbage is a plant: but when we descend to the simpler forms of animal life, we find them so nearly allied to plants that our classification is troubled. Still greater is our perplexity when the simpler actions are presented for analysis; positive as we may be that some actions have a volitional element, we are at a loss to mark out *what* that element is.

In popular language, those actions are called voluntary over which we can exercise control, either in the way of restraining, or of prompting them. I can move my arm, or keep it motionless, if I will to do so. But there are other actions which are beyond control; no effort of Will suffices to prompt, or to restrain them. The heart beats without my control. The eyelid winks, the wounded muscle quivers, the stomach digests, involuntarily. I can control the movement of my arm, unless a sharp pain forces me to withdraw it; and when I withdraw it under sudden pain, the action is said to be involuntary.

This is a rough classification, which suffices for our daily needs. We want a term to mark a certain group, and the term voluntary satisfies that want. But the severer exigencies of Science are not satisfied so easily. A rigorous examination shows that in most, if not in all, the so-called involuntary actions (as we shall see presently) this very volitional element of control may find a place. Although breathing is an involuntary act, it can be, and often is, restrained or accelerated by the will; but the controlling power soon comes to an end—we cannot voluntarily suspend our breathing for many seconds; the urgency of the sensation at last bears down the control. In like manner, we can partially, but not wholly, restrain the shrinking and trembling which accompany pain and terror. It has been said that these partial influences of control are due to the fact that the apparatus involves some of the voluntary muscles, and these are, of course, under the control of the will; but that inasmuch as the apparatus is not wholly constituted by voluntary muscles, it is not wholly under control. Yet this is only a restatement of the fact in different terms. The muscles are styled voluntary, because they are under control. Nevertheless, it is easy to prove that an apparatus of purely voluntary muscles will furnish an involuntary act—an act quite beyond all influence of the Will. The act of winking is an example. It is performed by voluntary muscles, and may be a purely voluntary act—as when we wish telegraphically to warn one of our hearers that we are jesting, Yet this act, which is as purely voluntary as any we perform, is habitually an involuntary act; the contact of the air with the eye causes a loss of temperature by evaporation, and the sensation caused by this dryness urgently insisting on being remedied, we wink. Not only is winking one of the typical examples of involuntary action, but we find that it occurs in spite of the most obstinate effort to restrain it: no resolution on our part *not* to wink will prevent our winking, after a certain time, or if a hand be passed rapidly before the eye.

This example shows that the partial control which the will exercises over what are called involuntary acts, does not depend on the nature of the muscles involved. The *same* action which is voluntary at one moment will be involuntary at another, according to the urgency, or intensity, of the stimulus. We laugh because we are tickled, or because some ludicrous image presents itself: both of these are involuntary actions, although both are capable, within certain limits, of control; but we may also laugh because we pretend to be tickled at the great man's joke—secretly felt to be a very feeble effort of humour. We cough because there is a tickling in the throat; and we also cough because we desire to drown the too buoyant platitudes of a remorseless orator. We yawn because we are weary, and we yawn because we determine to set others yawning. It seems clear, therefore, that the *volitional* element we are in search of, cannot lie *in the act* itself, but in something which *precedes or accompanies* the act. According to the popular opinion, an act is called voluntary if the mind has determined it by a conscious conception of the object to be attained; and if we were to say that volition is an action determined by a distinct idea, we should express the current opinion pretty accurately. Is that opinion tenable?

It is not tenable, because on the one hand actions may be determined by distinct ideas, and yet be "involuntary;" and because on the other hand actions may be voluntary, yet not determined by distinct ideas, but determined simply by sensations. Let a friend pass a finger rapidly before your eye, and although he has solemnly assured you that he will not touch you, and you have profound confidence in his word, yet no effort of Will prevents your winking. It is in vain you resolve to be firm—the eyelid drops as the finger approaches. This winking is, according to ordinary conceptions, an involuntary act, since it is performed in spite of the will; yet it is an act determined by an idea, the idea of *danger;* and the proof of this is seen when you approach a finger to the eye of an animal, or infant, in whom no such idea of danger is excited: it does not wink. Nor do you wink when you approach your own finger to your eye, because then the idea of danger is absent. We have here an action eminently *controllable,* and obviously determined by an *ideal stimulus,* having therefore the two cardinal characters of a voluntary act, yet being unmistakably involuntary. To reconcile such a contradiction we must suppose that the Will oscillated — one instant it resolved that winking should not take place, and the next instant resolved that it should. This explanation would, however, force the admission that the act of winking was not involuntary; after which, it would be puzzling to say *what* acts are involuntary. If the will can thus oscillate, and thus rescind its orders, why may it not in all the assumed cases of involuntary action be in a state of oscillation?

What is the process of control? Every action is a response to a sensitive stimulus. Muscles are moved by motor-nerves which issue from nerve-centres; these nerve-centres are excited by impressions carried there, either by sensory nerves going from a sensitive surface, or by impressions communicated from some other centre. A stimulus applied to the skin excites a *sensation,* which being reflected on a muscle excites a *contraction.* But instead of the sensation exciting a muscle, it may be reflected on some nerve-centre, and excite a reflex-feeling. This secondary, or reflex-sensation, may either play upon a muscle, or upon some other centre, and *this* will excite an action. Thus it is that the same external stimulus may issue in very different actions. We decapitate a frog, and half an hour after prick or pinch its leg: the frog hops, or suddenly draws up its leg. We now prick, or pinch, an uninjured frog in the same way, and we mostly (not always) observe that its leg is motionless; it does not hop away, it only lowers its head, and perhaps closes its eyes; a second pinch makes it hop away. In the decapitated frog, the action was reflex; the stimulus transmitted from the skin to the spinal chord was directly answered by a contraction of the leg. In the uninjured frog, the stimulus was also transmitted to the Spinal Chord; but from thence it ran upwards to the brain, exciting a reflex-feeling of alarm; but though alarmed, the animal was not forced into any definite course of action to secure escape; and while thus hesitating, a second prick came, and the urgency of the sensation then caused it to hop away. This hopping was reflex, but it was indirectly so: it was prompted by the reflex-feeling, which in turn had been excited by the original sensation. In like manner, if a dog's tail be pinched by a stranger, the dog cries out, and turns suddenly round to bite his tormentor. If the tormentor happen to be the dog's master, or friend, the dog will cry out, start away, or perhaps even turn round to bite—but he will not bite; should he get so far as to seize the hand with his teeth, he checks himself in time. This control is often touchingly seen in removing a thorn from a dog's foot: the pain causes a reflex-action, which brings the dog's head down upon the operator's hand; but instead of biting, the grateful animal licks that hand.

These are cases of control. They are possible only because reflex-feelings are excited; one sensation being rapidly followed by another, so that before one action, directly reflex, can occur, *another action* is set going, which interferes with it, controls it. An examination of the Nervous System discloses a number of centres, all capable of independent action, yet all connected with each other, and thus brought into some dependence on each other; it is through this dependence that control becomes possible. A sensation, instead of issuing in the action which usually follows it, sometimes issues in another sensation; this in turn may issue in a third sensation, instead of in an action; just as, when a row of billiard-balls is struck, the impetus is transmitted from one ball to the other, the *last* in the row flying off, and all the others remaining in their original position. At some point or other, could we follow its course, we should observe that the original sensation issued in an action, although, because the final stimulus to this said action is a reflex-*feeling*, the action itself is very unlike what it would have been if *directly* reflex. Tickle the face of a sleeping man, and by a reflex-action his hand is raised to rub the spot; tickle the face of that man when awake, and instead of this reflex-action, there will be one of vocal remonstrance, or perhaps one guiding a pillow in its descent upon your head.

Inasmuch as all actions whatever are the products of stimulated nerve-centres, it is obvious that all actions are reflex—reflected from those centres. It matters not whether I wink because a sensation of dryness, or because an idea of danger, causes the eyelid to close: the act is equally reflex. The nerve-centre which supplies the eyelid with its nerve has been stimulated; the stimuli may be various, the act is uniform. At one time the stimulus is a sensation of dryness, at another an idea of danger, at another the idea of communicating by means of a wink with some one present; in each case the stimulus is reflected in a muscular contraction. Sensations excite other sensations; ideas excite other ideas; and one of these ideas may issue in an action of control. But the restraining power is limited, and cannot resist a certain degree of urgency in the original stimulus. I can, for a time, restrain the act of winking, in spite of the sensation of dryness; but the reflex-feeling which sets going this restraining action will only last a few seconds; after which the urgency of the external stimulus is stronger than that of the reflex-feeling—the sensation of dryness is more imperious than the idea of resistance—and the eyelid drops.

If a knife be brought near the arm of a man who has little confidence in the friendly intentions of him that holds it, he will shrink, and the shrinking will be "involuntary"—in spite of his will. Let him have confidence, and he will not shrink, even when the knife touches his skin. The idea of danger is not excited in the second case, or if excited, is at once banished by another idea. Yet this very man, who can thus repress the involuntary shrinking when the knife approaches his arm, cannot repress the involuntary winking when the same friend approaches a finger to his eye. In vain he prepares himself to resist that reflex-action; in vain he resolves to resist the impulse; no sooner does the finger approach, than down flashes the eyelid. Many men, and most women, would be equally unable to resist shrinking on the approach of a knife: the association of the idea of danger with the knife, would bear down any previous resolution not to shrink. It is from this cause that timorous women tremble at the approach of firearms. An association is established in their minds which no idea is powerful enough to loosen. You may assure them the gun is not loaded; "*that* makes very little difference," said a *naïve* old lady to a friend of ours. They tremble, as the child trembles when he sees you put on the mask. These illustrations show that the urgency of any one idea may, like the urgency of a sensation, bear down the resistance offered by some other idea; as the previous illustrations showed that an idea could restrain or control the action which a sensation or idea would otherwise have produced. According to the doctrines current, the Will is said to be operative when an idea determines an action; and yet all would agree that the winking which was involuntary when the idea of danger determined it, was voluntary when the idea of communicating with an accomplice in some mystification determined it.

The reader will have gathered already that we admit no real and essential distinction between voluntary and involuntary actions. They are all voluntary. They all spring from Consciousness. They are all determined by feeling. It is convenient, for common purposes, to designate some actions as voluntary; but this is merely a convenience; no psychological, nor physiological, insight is gained by it; an analysis of the process discloses no element in a voluntary action which is not to be found in an involuntary action. In ordinary language it is convenient to mark a distinction between my raising my arm because I will to raise it for some definite purpose, and my raising it because a bee has stung me; it is convenient to say, "I *will* to write this letter," and "this letter is written against my will—I have no will in the matter." But Science is more exacting when it aims at being exact; and the philosopher, analysing these complex actions, will find no element answering to the "will," in one, which is absent from the other: he will find this only, that in each case certain muscular groups have been set in action by certain sensational or ideational stimuli.

It is a very general mistake to suppose that every act of volition implies a distinct idea of its object. Unless such an intellectual element be present, guiding the movement, the voluntary character is said to be wanting. But it is well said by Müller, that "the ultimate source of voluntary motion cannot depend on any conscious conception of its object; for voluntary motions are performed by the fœtus before any object can occur to the mind—before any idea can possibly be conceived of what the voluntary motion effects. . . . The fœtus moves its limbs at first, not for the attainment of any object, but solely because it can move them. Since, however, on this supposition, there can be no particular reason for the movement of any one part, and the fœtus would have equal cause to move all its muscles at the same time, there must be something which determines this, or that, voluntary motion to be performed. The knowledge of the changes of position which are produced by given movements, is gained *gradually, and only by means of the movements themselves.* . . . The voluntary excitation of the origins of the nervous fibres, without objects in view, gives rise to motions, changes of posture, and consequent sensations. Thus a *connection is established* in the yet void mind *between certain sensations and certain motions.* When subsequently a sensation is excited from without, in any one part of the body, the mind will be already aware that the voluntary motion, which is in consequence executed, will manifest itself in the limb which was the seat of sensation; the fœtus *in utero* will move the limb that is pressed upon, and not all the limbs simultaneously. The voluntary movements of animals must be developed in the same manner. The bird which begins to sing is necessitated by an instinct to incite the nerves of its laryngeal muscles to action; tones are thus produced. By the repetition of this blind exertion of volition, the bird at length learns to connect the kind of cause with the character of the effect produced. The instinct of this dream-like and involuntary-acting impulse in the sensorium has some share in the production of certain movements in the human infant, which are in themselves voluntary. In the sensorium of the newly-born child there is a necessitating impulse to the motions of sucking; but the different parts of the act of sucking are themselves voluntary movements."*

In this passage, Müller calls certain actions voluntary which writers usually consider to be reflex (involuntary), and reflex-actions with them mean actions without sensation; but as Mr Bain remarks, "It may be by a reflex-action that a child commences to suck when the nipple is put between its lips; but the continuing to suck so long as the sensation of hunger is felt, and the ceasing when that sensation ceases, are truly volitional acts. All through animal life, down to the very lowest sentient being, this property of consciousness is exhibited, and operates as the instrument for guiding and supporting existence. To whatever lengths the purely reflex instincts, or the movements divorced from consciousness, may be carried on in the inferior tribes, I can with difficulty admit the total absence of feeling in any being we are accustomed to call an animal; and with this feeling I am obliged also to include this property, *which links the state of feeling with the state of present movement.*"† It is this link of feeling with action, which, according to Müller, constitutes Volition.

This idea of the nature of Volition has been adopted and developed by Mr Bain,* with a fulness and suggestiveness of treatment which leaves little to be desired. There are questions he leaves untouched, and points on which I do not find myself able to agree with him; but, on the whole, I have received more light from his work, than from that of any other psychologist, and shall therefore expound his views, in preference to those of any other. He considers the Will as the generalised expression of our power to perform voluntary actions; and by voluntary actions he means those which have an *established* connection with certain sensations. A sensation of pain excites the active organs: the animal struggles till it has escaped the pain; these struggles are involuntary, and they are so because beyond definite control, beyond the guidance of any one feeling; they belong to what Mr Bain calls the "diffusive wave of emotion;" whereas voluntary actions are *isolated*, and directed to a *particular* end. In the course of its struggles, the animal accidentally makes one movement which is followed by an alleviation or cessation of the pain; this makes it discontinue all the other movements, and continue that which alleviates. If any of the other movements are set going, the pain recurs, and warns the animal to cease. The continuance of an alleviating movement, Mr Bain regards as the volitional element.

---

* MÜLLER: *Physiology*, ii. 835.

† BAIN: *The Senses and the Intellect*, p. 296.

"We must in the first instance clearly and broadly separate the diffusive wave, accompanying all emotions as their necessary embodiment, from the active influence now under discussion. This is the more necessary as the two classes of movements are very apt to coexist. A blow with a whip, inflicted on a sentient creature, produces, as a part of the emotional effect, strictly so called, a *general* convulsive start, grimace, and howl; it also produces, in the case of the mature animal, an exertion *in some definite course* to avoid the recurrence of the infliction. The first effect is entirely untaught, primitive, instinctive; being intimately and indissolubly connected with feeling in the very nature of it. The other effect is based likewise on an original property, but brought into the shape that we usually find it in, after some experience and considerable struggles. The element just mentioned, of aim or purpose, in no sense belongs to the movements of the diffused wave, or those constituting the manifestation or expression of the mental state. The ecstatic shout of hilarious excitement, the writhings of pain, are energetic movements, but they belong neither to the class of central spontaneity above described, nor to the voluntary class now under consideration."

---

* BAIN: *The Emotions and the Will.*

When a very young infant is in pain, it struggles and squalls. That is all it can do; it does that lustily. Mr Bain considers it due to the diffused wave of emotion. Suppose the cause of the pain to be a needle pricking its foot; the child will make no effort to remove that needle, because the link between such a pain and such an action has not yet been established, and this voluntary effort cannot be made. Before it can make such an effort it must have learned to *local-*

ise its sensations. Every surgeon knows that the young infant may be allowed to have his hands free, when operated on, because it cannot with its hands interfere with the knife, not as yet knowing *where* the seat of pain is. When, later on, it has learned to localise its sensations, it may learn what actions alleviate them. A baby in discomfort from some itching of the nose is at first simply restless; it learns to rub that nose with its little fist, only after much experience of rubbings.

Mr Bain relies on the primary fact that when pain coexists with an accidental alleviating-movement, or when pleasure coexists with a pleasure-sustaining movement, such movements become subject to the control of the respective feelings which occur in their company. It is a primordial law that we shrink from pain and cling to pleasure; as long as the pain is unalleviated, movements are kept up; as soon as one particular movement brings cessation of pain, that movement is kept up. An infant lying in bed has the painful sensation of chilliness. This feeling produces the usual emotional display, namely, movement, perhaps cries and tears. In the course of a variety of spontaneous movements of arms and legs, there occurs an action that brings the child in contact with the nurse lying beside it; instantly warmth is felt, and this alleviation of painful feeling becomes immediately the stimulus to sustain the movement going on that moment. That movement, when discovered, is kept up, in preference to the others. In this way the child learns to connect certain sensations with certain movements, and at a year old will draw close to its nurse whenever the sensation of cold comes on, even during sleep. "It is an original property of our feelings to prompt the active system one way or another, but there is no original connection between the several feelings and the actions that are relevant to each particular case. To arrive at this goal, we need all the resources of spontaneity, trial and error, and the adhesive growth of the proper couples, when they can once be got together. The first steps of our volitional education are a jumble of spluttering, stumbling, and all but despairing hopelessness. Instead of a clear curriculum, we have to wait upon the accidents, and improve them when they come."

No one will withhold his assent from the proposition that a pain increasing in company with any movement must tend to cause the arrest of that movement; or that pleasure increasing in company with a movement must tend to cause the continuance of that movement.

"The spontaneous action that brings a limb into a painful contact, as when the child kicks its foot upon a pin in its dress, is undoubtedly from the earliest moment of mental life arrested. Without this I see no possible commencement of voluntary power. So a movement that mitigates a pain already in operation is maintained, as long as the creature is conscious of diminished suffering. In this way, the arms, hands, and fingers work for abating sharp agony, provided only the right member has found its way into action. No provision, as I have often said, exists at the dawn of life for getting the right member into play. The infant being must go through many a cycle of annoyance, because, among numerous stimulants to action that have occurred, the right one has been omitted. But the true impetus once arising, the mind is alive to the coincidence of this with decreasing or vanished pain ; just as, on the other hand, we must suppose it alive to the coincidence of some other movement with an aggravation of the evil. The greater the pain, the more strongly is the alleviating movement sustained when once under way. For the next stage of the process, the establishment of a connection between the pain and the special action, we must fall back upon the foundation of all our acquisitions, namely, the force of contiguous association. The concurrence of a particular sensation, as a prick in the arm, with that retracting movement which rids us of the pain, leads to the rise of an adhesive bond between the two, if a sufficient number of repetitions have occurred. We cannot say how many instances of chance conjunction are requisite to generate an association so strong as to take away the uncertainties attending the spontaneous discharge; all the circumstances governing the rapidity of contiguous adhesion would have to be taken into account in this case. The excitement of strong pain on the one hand, or of strong pleasure on the other, is a favourable moment for the growth of an association : and probably not a great number of those occasions would be necessary to convert an inchoate into a full-formed volition. Full-formed, I say, because when the supposed pain can bring into play the proper movement, in the absence of all spontaneous tendency, we have a case of voluntary power complete for all the purposes of the living being. The example that I am now discussing, namely, the retractation of any part of the body from a painful contact, implies a very numerous set of coincidences between local pains and local movements. For all contacts on the back of the hand, there must be an association with the muscles of flexion; for the palm, the extensor muscles must be affected. For the outside of the arm, the tendency to draw it towards the side has to be prompted. And so in like manner for every part of the body, under an irritating smart, there must be a formed connection between painful sensation arising in the locality and the corresponding movements of retractation. This is one department of voluntary acquisition, and consists of a multitude of couples of individual sensations and individual movements, joined by association, after being commenced by spontaneity. For the class of acute pains supposed the acquirement is perfect, owing in a great measure to the simplicity of the case. It is not so with many of those muscular pains, which we are professedly considering at present, although in the foregoing illustration we have departed from them, and somewhat anticipated the subject of sensation at large. The cramps of the limbs do not ordinarily suggest the alleviating action. Owing partly to the rarity of the feeling, we have not usually a full-formed volition which enables the state of suffering to induce the alleviating action, and consequently we are thrown upon the primitive course of trial and error. This instance shows, by contrast with the preceding, how truly our voluntary powers result from education. An established link between a cramp in the ball of the leg, and the proper actions for doing away with the agony, is quite as great a desideratum as drawing up the foot when the toe is pinched or scalded; yet no such link exists, until a melancholy experience has initiated and matured it. The connection in the other case is so well formed from early years, that almost everybody looks upon it as an instinct, yet why should there be an instinct for the lighter forms of pain, and none for the severest? The truth is, that the good education in the one is entirely owing to our being more favourably situated for making the acquisition."

Hitherto we have seen voluntary actions under the guidance of sensations only; let us now observe the ideal guidance. A child is seated at table. He places his hand upon the bright teapot, and the pain of the burn makes him withdraw his hand; again the brightness attracts his curious fingers, and again the pain makes him desist. After a certain number of trials the idea of the pain is so associated with that of the teapot, that the child no longer burns himself. But he has thrust his hand into the biscuit-plate, and finds this action rewarded with a biscuit instead of a burn. On repeating it he is scolded, or slapped, or put into the corner—made to suffer pain; and if this pain be *always* inflicted when he acts thus, he will soon learn to restrain his forays upon the biscuits. This link which is established between an action and a pain, is an ideal link, and finds its place in memory; it is nearly as firm as a sensational link. The supposition, however, that this ideal link makes the action voluntary, as distinguished from an action which is guided by a sensational link, will not withstand criticism.

At first our actions are guided by sensations; then by the ideal representatives of those sensations.

"Instead of an actual movement seen, we have for the guiding antecedent a movement conceived, or in idea. The association now passes to those ideal notions that we are able to form of our various actions, and connects them with the actions themselves. All that is then necessary is a determining motive, putting the action in request. Some pleasure or pain, near or remote, is essential to every volitional effort, or every change from quiescence to movement, or from one movement to another. We feel, for example, a painful state of the digestive system, with the consequent volitional urgency to allay it; experience, direction, and imitation, have connected in our minds all the intermediate steps, and so the train of movements is set on. On the table before us we see a glass of liquid; the infant never so thirsty could not make the movement for bringing it to the mouth. But in the maturity of the will, a link is formed between the appreciated distance and direction of the glass, and the movement of the arm up to that point; and under the stimulus of pain, or of expected pleasure, the movement is executed."

It often happens that we are conscious of "an interval of suspense between the moment of painful urgency and the moment of appeasing action;" because the reflex-feelings are many, and these cross and recross each other, so that no one of them issues in action. This was the case with the frog which we cited just now; instead of hopping away when pinched, it cowered and seemed hesitating as to its escape.

And this leads us to consider how thoughts, no less than actions, can be controlled; how the mind has power over its actions, no less than over the actions of the body. The fact that we can, in some degree, control the thoughts, is indisputable; *how* we do so is not so clear.

The power of keeping up one train of thought, is analogous to that of keeping up one course of muscular action. We cling to certain ideas because they are pleasant, or interest us, or because some remote pain or pleasure stimulates us; and we repress all other thoughts as they arise, just as we should repress movements which disturbed a pleasurable sensation. It is notorious that we cannot call up any one idea at will; but having once got hold of the idea, we can keep it before the mind. What Mr Bain has said when treating of the intellectual process named by him "constructive association," seems to us the true explanation of *all* command over the thought:—

"When Watt invented his 'parallel motion' for the steam-engine, his intellect and observation were kept at work, going out in all directions for the chance of some suitable combination rising to view; his sense of the precise thing to be done was the constant touchstone of every contrivance occurring to him, and all the successive suggestions were arrested, or repelled, as they came near to, or disagreed with, this touchstone. The attraction and repulsion were purely volitional effects; they were the continuance of the very same energy that, in his babyhood, made him keep his mouth to his mother's breast while he felt hunger unappeased, and withdraw it when satisfied, or that made him roll a sugary morsel in his mouth, and let drop or violently eject what was bitter or nauseous. The promptitude that we display in setting aside or ignoring what is seen not to answer our present wants, is volition, pure, perennial, and unmodified; the power seen in our infant struggles for nourishment and warmth, or the riddance of acute pain, and presiding over the last endeavours to ease the agonies of suffering. No formal resolution of the mind, adopted after consideration or debate, no special intervention of the 'ego,' or the personality, is essential to this putting forth of the energy of retaining on the one hand, or repudiating on the other, what is felt to be clearly suitable, or clearly unsuitable, to the feelings or aims of the moment. The inventor sees the incongruity of a proposal, and forthwith it vanishes from his view. There may be extraneous considerations happening to keep it up in spite of the volitional stroke of repudiation, but the genuine tendency of the mind is to withdraw all further consideration, on the mere motive of unsuitability; while some other scheme of an opposite nature is, by the same instinct, embraced and held fast. In all these new constructions, be they mechanical, verbal, scientific, practical, or æsthetical, the outgoings of the mind are necessarily at random; the end alone is the thing that is clear to the view, and with that there is a perception of the fitness of every passing suggestion. The volitional energy keeps up the attention, or the active search, and the moment that anything in point rises before the mind, springs upon that like a wild beast on its prey."

Nowhere has Mr Bain expressed himself categorically respecting the difference between voluntary and involuntary actions; but he assumes the difference, and, implicitly at least, he makes it depend on the establishment of the link of feeling. "Voluntary actions," he says, in the nearest approach to a definition we can recall, "are distinguished from reflex and spontaneous activity by the *directive intervention of a feeling in their production.*" In denying the intervention of sensation in reflex-actions, he only follows the current theory; but unless he separates the involuntary from reflex and spontaneous actions, he falls into manifest contradiction with his own principles, in making the intervention of feeling *the* mark of a voluntary act. We have endeavoured to show that both voluntary and involuntary actions are reflex, following upon the stimulus given to their centres, that stimulus being sensational or ideational. Nor is this all: they are both capable of being brought under *control*—that is to say, of being restrained or originated by the influence of some other centre. That we do not habitually control (that is, *interfere* with) the action of the heart, the contraction of the iris, or the activity of a gland, is true; it is on this account that such actions are called involuntary; they obey the immediate stimulus. But it is an error to assert, as most physiologists and psychologists persist in asserting, that these actions *cannot* be controlled, that they are altogether beyond the interference of other centres, and cannot by any effort of ours be modified. It is an error to suppose these actions are essentially distinguished from the voluntary movement of the hands. We have acquired a power of definite direction in the movements of the hands, which renders them obedient to our will; but this acquisition has been of slow laborious growth. If we were asked to use our toes as we use our fingers—to grasp, paint, sew, or write with them, we should find it not less impossible to control the movements of the toes in these directions, than to contract the iris, or cause a burst of perspiration to break forth. Certain movements of the toes are possible to us; but unless the loss of our fingers has made it necessary that we should use our toes in complicated and slowly acquired movements, we can do no more with them than the young infant can do with his fingers. Yet men and women have written, sewed, and painted with their toes. All that is required is that certain links should be established between sensations and movements; by continual practice these links *are* established; and what is impossible to the majority of men, becomes easy to the individual who has acquired this power. This same power can be acquired over what are called the organic actions; although the habitual needs of life do not *tend* towards such acquisition, and without some strong current setting in that direction, or some peculiarity of organisation rendering it easy, it is not acquired. In ordinary experience the number of those who can write with their toes is extremely rare, the urgent necessity which would create such a power being rare; and rare also are the examples of those who have any control over the movement of the iris, or the action of a gland; but both rarities exist.

It would be difficult to choose a more striking example of reflex-action than the contraction of the iris of the eye under the stimulus of light; and to ordinary men, having no link established which would guide them, it is utterly impossible to close the iris by any effort. It would be not less impossible to the hungry child to get on the chair and reach the food on the table, until that child had *learned* how to do so. Yet there are men who have learned how to contract the iris. The celebrated Fontana had this power; which is possessed also by a medical man now living at Kilmarnock—Dr Paxton—a fact authenticated by no less a person than Dr Allen Thomson.* Dr Paxton can contract or expand the iris at will, without changing the position of his eye, and without an effort of adaptation to distance.

---

* *Glasgow Medical Journal,* 1857, p. 451.

To move the ears is impossible to most men. Yet some do it with ease, and all can learn to do it. Some men have learned to "ruminate" their food; others to vomit with ease; and some are said to have the power of perspiring at will.* That many glands are under the influence of the Will—in other words, that we can stimulate them to secretion by a mere ideal stimulus—is too well known to need instance here. Even the beating of the heart can be arrested. The heart has its own nervous system. The minute ganglia imbedded in its substance regulate its rhythmic movements; and long after death the heart is seen to beat. But although thus independent, it is also dependent: its nervous system is in connection with the spinal chord and brain; and influences from these will act upon it (see p. 147). Thus it is that emotions agitate the heart; the disturbance of its movements comes from the interference of brain or chord. Now, if once we recognise a channel of sensation, we recognise a possible source of control; and if the daily needs of life were such that to fulfil some purpose the action of the heart required control, we should learn to control it. Some men have, without such needs, learned how to control it. The eminent physiologist, E. F. Weber of Leipzig, found that he could completely check the beating of his heart. By suspending his breath, and violently contracting his chest, he could retard the pulsations; and after three or five beats, unaccompanied by any of the usual sounds, it was completely still. On one occasion he carried the experiment too far, and fell into a syncope. Cheyne, in the last century, recorded a case of a patient of his own who could at will suspend the beating of his pulse, and always fainted when he did so.

---

* MAYER: *Die elementarorganization des Seelenorgans*, p. 12, is the authority for the last statement.

It thus appears that even the actions which most distinctly bear the character recognised as involuntary—uncontrollable—are only so because the ordinary processes of life furnish no necessity for their control. We do not learn to control them, though we could do so, to some extent; nor do we learn to control the motions of our ears and toes, although we could do so. And while it appears that the involuntary actions can become voluntary, it is familiar to all that the voluntary actions tend, by constant repetition, to become involuntary, and are then called secondarily automatic.

The conclusion at which we arrive is this: Popular language conveniently classes actions as voluntary when a distinct conception of the object to be achieved accompanies or originates them. But Pyschology and Physiology, descending deeper than such classifications, and analysing the process which takes place in the organism, declare that all actions whatever are the responses of organs to the stimulus of their nerve-centres. Whether the action be the movement of a muscle or the secretion of a gland, it is finally determined by the centre from which the organ is supplied. This centre may be stimulated by a sensory nerve going from the surface—as when the salivary gland pours out its secretion after the stimulus of food, or the limb contracts on the stimulus of pain. The centre may also be stimulated by the action of some *other* centre: as when the *idea* of food causes a flow of saliva; or an irritation of the salivary gland causes a flow of gastric juice. But whether the action result from a direct or an indirect stimulus, it is always the same response of an organ to its centre; whether the starting-point be an idea, or a sensation, the final issue is an excitation of the particular centre, and the response of a particular action.* We cannot separate some actions from others, and call them voluntary merely because they are dependent on a link of feeling, since all actions are dependent on sensation. And if any reader objects to such a conclusion on the ground that it makes the Soul animate the *whole* body, and preside over all its actions, not simply over a few of them—if he objects that we are thereby retrograding towards the doctrine of Stahl—our reply is: we must follow Logic whither Logic leads. Any reader who is uncomfortable at the idea of retrograding, who is unwilling to believe that all the phenomena of his sensitive organism have one common source, one kindred nature, and one common name—the soul—is at perfect liberty to try and reach some other conviction which, besides being more agreeable to his feelings, will better explain the facts. It is a topic on which no man will, wisely, dogmatise. The veil of mystery will never be lifted. We who stand before that veil, and speculate as to what is behind it, can but build systems; we cannot see the truth.

---

* In the valuable work by M. LANDRY—*Traité des Paralysies*, the first part of which appeared while these sheets were at the printer's—there is a view of voluntary, involuntary, and reflex actions, almost identical with the one given above. Indeed, although M. LANDRY's language, and his psychological opinions generally, accord with current doctrines, the physiological conclusions to which his researches have conducted him agree, in the main, with those I have endeavoured to establish

VI. THE SPINAL CHORD AS A SENSATIONAL, VOLITIONAL CENTRE.—The ground has now been fairly cleared for a most important inquiry, one not only essential to the argument against the Brain as the exclusive organ of the Mind, but also essential to the right understanding of most questions relating to nervous phenomena. It was necessary that we should first come to a distinct understanding about the Reflex-Theory; because while, on the one hand, that Theory relied for support on the facts which showed that the Spinal Chord was an independent centre, acting as well without the Brain as with it—and hence its actions were assumed to be independent of sensation;—on the other hand, the facts which unequivocally declare that the Spinal Chord *is* a sensational centre, were set aside, and explained away, as being "merely reflex." To prove that reflex-actions are independent of sensation, it is first necessary to prove upon. We have no difficulty in understanding that, because the organic apparatus with which the optic centre is connected, differs from that of the auditory centre, the forms of Sensibility excited in these centres will be different. Nor should we have any difficulty in understanding why a ray of light acting on the optic centre must excite a different form of Sensibility from that which would be excited by its acting on the Cerebrum *through* the optic centre, *i.e.* giving the *sensation* of light in the one case, and the *perception* of light in the other.

By this explanation I have endeavoured to make it clear how the *forms* of Sensibility excited in the cerebral hemispheres must necessarily be in many respects unlike those excited in the ganglia, which are *directly* stimulated by sensory nerves. The difference between a *perception* of sound and a *sensation* of sound is as great as the difference between the sensation of pain when a skin-nerve is divided, and the sensation of a flash of light when the optic nerve is divided.

The Sensibilities of the Cerebrum are usually grouped under two general heads—Intellect and Emotion. Hitherto it has been mysterious how an organ so homogeneous as the Cerebrum, wherein each convolution is but a repetition of the others, should nevertheless serve functions so various as those of the Intellect and the Emotions. Even those who reject the phrenological localisation of the separate faculties, admit that the front part of the Cerebrum seems specially devoted to the Intellect, and the middle and back parts to the

Emotions and Instincts. But neither phrenologists, nor their antagonists, to my knowledge, have had any scientific explanation to offer; and as the views advocated in these pages seem to me to present a possible elucidation of the difficulty, I will first state the objection, and then endeavour to remove it.

The convolutions of the Cerebrum are everywhere similar and continuous, like so many folds in a piece of velvet. They are not separate; they are not different; they are identical. Why, then, can we suppose they are the organs of very different functions? We do not imagine that one lobe of the liver, or lobule of the kidney, plays a different part from that of its fellows; why, then, do we imagine that one convolution, or group of convolutions, can be devoted to reasoning, and another to loving, one to the perception of colours, and another to an instinct?

The phrenologists evade this difficulty by saying that "observation teaches them the facts *are* so." Without here disputing this assertion, I must remark that it in no way removes the scientific difficulty; and as Phrenology professes to be the Physiology of the Brain, it is bound to connect its observations with some scientific explanation of the nervous mechanism involved.

We need not evade the difficulty; for if Function be determined by anatomical connection, it is clear that, although the Cerebrum may be one homogeneous structure, it will have various functions if it be connected with various organs. Assuming that every convolution has the same general property of Sensibility, our principles lead to the conclusion that the peculiar *kind* of Sensibility excited in each will depend upon the nature of the stimulus; and the nature of the stimulus will of course be determined by the organic apparatus effecting it. A stimulus through the optic centre must be different from a stimulus through the gustatory centre; and both of these must be different from a stimulus through the viscera. The stimulus received through the centre of Perception, will necessarily differ from that received through the centre of the Appetites; and thus, widely as an Idea differs from an Emotion, they are both peculiar forms of Sensibility.

Although the general relations of the cerebral convolutions are thus indicated, it would require an entirely new investigation of the anatomy of the Brain to carry this into detail. For the present our knowledge is too imperfect to admit even of an attempt at detail. I will, however, venture to suggest the point of view which might direct inquiry. If we ask what are the chief divisions into which all psychical phenomena group themselves, we shall find that the popular classification into Thinking and Feeling, or Mental and Moral, lucidly and broadly indicates the primary groups. Let us, ideally, decompose these into six centres, three for each division. In the first group we may place Sensations, Perceptions, and Ideas—the various combinations of which represent intellectual activity. In the second group we may place Sensations, Instincts, or Appetites and Emotions—the various combinations of which represent moral activity.

Sensation thus forms the point of departure for each series. But we have already seen that there are various *kinds* of sensation, and that these fall into two distinguishable groups—namely, Sense-sensations and Systemic-sensations. The first are derived through the organs of special Sense, *i. e.* Sight, Hearing, Taste, Smell, and Touch. The second are derived from the Viscera, Muscles—in a word, from the system at large. The first may almost be considered as *impersonal*, because they bring us into conscious relation with external objects—with the Not-self. The second are, on the contrary, intensely personal, because they do not bring us into conscious relation with external objects, but only with what is passing within our bodies. Reason has often been considered impersonal, but no one ever considered Emotion to be so. The intellectual operations always imply an externality; even when we are speculating about our own feelings or mental operations, we always view them as if *apart* from ourselves. The Emotions have a deeper root in personality. Every man, unless he be a subtle psychologist, believes that the redness is in the rose, the sweetness in the apple, and the loudness in the thunder. But no man, not even the psychologist, believes that the love or hate, the fear or reverence, the desire or disgust, which moves him, belongs to the object which excites the feeling.

This externality of the Sense-sensations, and internality of the Systemic-sensations, creates a broad line of demarcation between the Perceptions which arise from the one, and the Appetites, or Instincts, which arise from the other; and these, in turn, issue in the very different forms of Sensibility known respectively as Thought and Emotion.

It has never been doubted that our perceptions and ideas have their origin in sensations. The old philosophic adage that nothing is in the intellect which was not previously in the sense, may be equivocal, from a restricted use of the word sense; but it points to the well-authenticated fact, that sensation lies at the basis of all intellectual operation. I feel myself justified, therefore, in considering *Ideation* as the form of cerebral sensibility which is determined by the cerebral connections with the ganglia of special Sense. In like manner, *Emotion* may be considered as the form of cerebral sensibility which is determined by connections with the ganglia of visceral sensation. It was formerly believed that the heart, the liver, and the spleen, were seats of the passions; popular language still preserves this notion; but Bichat was the last great anatomist who countenanced the doctrine. Since that doctrine has fallen into discredit there has been an undue neglect of the important fact which it endeavoured to explain, namely, the immediate influence exercised over the emotions by the condition of the viscera, and the influence exercised over the viscera by the state of the emotions. Both the ancient and the modern doctrine are reconciled in the view I have put forth, which makes the viscera the *main* source of emotions, just as the organs of sense are the main source of ideas.

Although the mental phenomena are thus grouped under two primary and six secondary divisions, to which anatomy may one day assign corresponding parts of the nervous apparatus, we must never lose sight of the fact, that there is perpetual action and interaction between the various centres. But this consideration leads us into the domain of Psychology, and we must therefore pause here. It is not for us to attempt to unravel the tangled mystery of mental operations. We have not been able to do more than indicate in a very general manner the relations existing between the Mind and the Brain; and having done so, we have now to ask, whether the cerebral hemispheres have any *other* functions? I think they have; but as physiologists have not directed their attention in this direction, there are no experiments on record to throw light on it. Lallemand conceives that there must be some necessary dependence of the Spinal Chord on the Cerebrum in the higher animals, and it is to this he attributes the fact, certainly significant, that children born without a brain never live more than three or four days: they do not die of starvation, for they swallow food quite well. He also observes that the *movements* of these brainless children are much feebler than those of others at the same age.* One might, indeed, suspect the cause to lie deeper; the same cause which determined the malformation may determine the feeble vitality. But Lallemand's idea is strengthened by the observation of Flourens,† that removal of the cerebrum retards digestion; and by the familiar fact of the intimate sympathy between the Mind and all the organic functions.

---

* Quoted by SOLLY *On the Brain*, p. 455.

† FLOURENS: *Recherches*, p. 129.

It is perhaps on this ground that we may explain the disproportion between the mental activity and the size of brain in certain animals. The elephant has a brain three times heavier than that of man; and that of the whale is nearly twice as heavy as that of man. It is true that in the elephant and whale the cerebrum is not so much greater than the other parts, as is the case in man, in whom, as we saw, it is eight times the size of the other parts; nevertheless the preponderance of the cerebrum is considerable, and there is certainly no preponderance of mental power to correspond with it. When, however, we conceive the cerebrum as the chief nervous centre having dominion over all others—when we assign to it participation in other functions besides those of mental activity, the immense size of the body of the elephant and whale seems to justify an equivalent development of the cerebrum.

On this, however, we must be content with hypotheses. Science has no carefully-collated facts to justify us in anything beyond a general surmise that the cerebral hemispheres have more than an intellectual and volitional part to play: that they are directly connected with the functions of intelligence, no reasonable man will doubt; and having arrived at this conclusion, we will now pass on to the examination of the other centres, by anatomists grouped together under the general term Brain.

CHAPTER IX.

(*Continued.*)

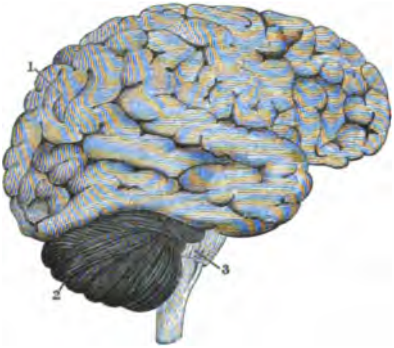
THE MIND AND THE BRAIN.

SECTION II.—THE CEREBELLUM AND MEDULLA OBLONGATA.

Description of the Cerebellum—Gall's hypothesis—Experiments of Flourens to prove that the Cerebellum co-ordinates muscular movements: arguments against such an hypothesis—Flat contradictions—The experiments of Schiff—Evidence furnished by Comparative Anatomy; and by Disease—Absence of the Cerebellum not accompanied by absence of co-ordination: Combette's case—The Cerebellum as a seat of sensation and volition—Has the Cerebellum any share in intellectual operations?—Description of the Medulla oblongata—Is the Medulla distinct from the Spinal Chord?—The Medulla a seat of volition—Why it is considered a seat of sensation—Is there a "vital point?"—Experiments of Flourens contradicted by the experiments of Schiff and Brown-Séquard—Influence of the Medulla on the heart's action—Why a "shock" of grief or joy causes death—Pain the cause of death—Summary of the functions of the Brain.

After the Cerebrum, or big brain, our attention must be claimed by the Cerebellum, or little brain, which, in man, lies underneath the greater mass, as seen in Fig. 51, 2, p. 122. The two organs present some differences in structure, but they agree in principal points: they are both composed of grey matter externally, and internally of white matter. The convolutions of the Cerebellum are more com-pressed, and have the appearance of leaflets. Like the Cerebrum, it is totally insensible to pricks or pinches. Its relation to the other centres is roughly indicated in this diagram (Fig. 52).

Fig. 51.



THE HUMAN BRAIN IN PROFILE.
1, Cerebrum; 2, Cerebellum; 3, Medulla Oblongata.

Fig. 52.

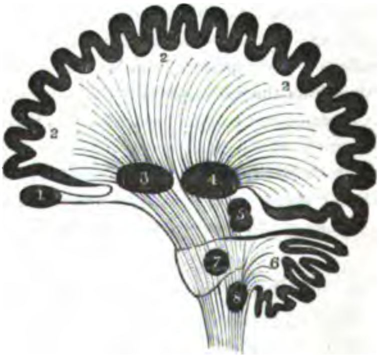


DIAGRAM OF VERTICAL SECTION OF THE BRAIN.
1, Olfactory Ganglion; 2, the Cerebral Hemisphere; 3, Corpus Striatum; 4, Optic Thalamus; 5, Tubercula quadrigemina; 6, Cerebellum; 7, Ganglion of the Pons Varolii; 8, Olivary body, or Ganglion in the Medulla Oblongata. The lines show the direction of the fibres.
(After Dalton.)

Gall, to whom we owe so great a debt for the impulse he gave to the study of the nervous system, assigned the sexual instinct to the Cerebellum. This hypothesis, after having been extensively discussed, is now, I believe, rejected by all good physiologists; and we must declare with Longet that a calm survey of the evidence forces the conclusion that neither Anatomy nor Pathology, neither Experiment nor Comparative Anatomy, justifies our assigning the sexual instinct to the Cerebellum.[*]

It is extremely probable that an antagonism to Phrenology, and a desire to find it in the wrong, has considerably sharpened the wits of physiologists, in the investigation of this point, making them at once more patient in research, and more circumspect in interpretation, than they are wont to be when no doctrine is at stake. If a little of this animus had moved them to scrutinise more closely the hypothesis first propounded by Flourens, it would never have obtained the too facile acquiescence of Europe; and our science would have been burdened with one error less. But there was nothing at stake in the hypothesis that the Cerebellum was the organ of muscular co-ordination—nothing but Truth; and, unhappily, the desire for Truth does not move the passions of men with the same energy as a desire to attack or defend a system.

If there is a position which our text-books represent as impregnable, it is that the co-ordination, or regulation, of muscular movements is the peculiar function of the Cerebellum. It seems looked on as a first truth. The experiments of Flourens and Bouillaud are considered as having established the point beyond dispute.[†] With what justice we will now inquire.

---

[*] Longet: *Traité de Physiol.* ii.; Lelut: *Rejet de la doctrine organologique;* Leuret et Gratiolet: *Anat. Comp. du Système Nerveux,* i. 427, 547. Dr Carpenter has also collected evidence on this point in his *Human Physiology.*

[†] Flourens: *Recherches,* &c. Bouillaud: *Recherches tendant à refuter l'opinion de M. Gall sur les fonctions du Cervelet,* 1827. Although

Flourens removed, by successive slices, the Cerebellum of a goose. With the first slices he noted hesitation and want of harmony in the movements of the bird; with the second slices the walk became a stagger; with the final slices all power of locomotion, all power of retaining the equilibrium of the body, disappeared. Throughout these operations the intellectual and sensitive faculties were undisturbed. "The Cerebellum, therefore," concludes Flourens, "is the seat of the coordination of muscular movement; but not the seat of intelligence, sensation, volition. The Cerebrum *feels* and *wills;* the Medulla oblongata and spinal chord *execute;* the Cerebellum *co-ordinates.*" This is one of those neat epigrams which captivate assent, and rest in the memory; and as regards the Cerebellum, it receives further assistance from the following statement made by Longet: "Take two pigeons—from the one remove the whole Cerebrum, from the other only a part of the Cerebellum; on the morrow, the first will be firm on its feet, the second will have the gait of a drunken man."*

The fact is indisputable; but does it carry the conclusion? It seems to me that there is a misconception of the bearing of experimental evidence, in the supposition that injury to an organ, followed by disturbance of a particular function, *proves* that the function

BOUILLAUD controverts FLOURENS on some points, showing that it is only over the muscles of the extremities that the co-ordinating power extends, yet he agrees with him on the main point.

* LONGET, ii. 265.

in question has its seat in that organ: nothing more than a suspicion can be warranted by such evidence; and in the present case the suspicion is proved to be erroneous, by the fact that the function can in some cases be exercised when the organ is absent. Experiment and Pathology, if sufficiently examined, distinctly pronounce against the hypothesis.

The idea of there being one particular seat of muscular co-ordination appears to me illusory. Each centre co-ordinates the muscles to which its nerves are distributed; and the co-ordination of the whole depends on the integrity of the whole mechanism. The Spinal Chord is the general centre from which spring the nerves distributed to the limbs and trunk. If this chord be divided, lengthwise, into two halves, each half will co-ordinate the muscles of each side of the body; but the two sides will not move harmoniously, because the integrity of the mechanism has been disturbed; and as regards the movements of the limbs, the two halves might be two animals. It is the same when the chord is divided across: this cuts the body into two halves; but the fore-legs are co-ordinated by the anterior half, the hind-legs by the posterior half. But if the division has not been complete, if we leave the grey matter to be on one side continuous, then Stilling assures us, the continuity enables both sides to be co-ordinated.*

The experiments of Flourens and Bouillaud certainly prove that the Cerebellum exercises some marked influence on muscular movements, at least those implied in sustaining equilibrium. But this is true only of birds and mammals. It is unequivocally *not* true of lizards, frogs, tritons, &c. Flourens indeed says: "I removed the Cerebellum of a frog; walking, leaping, even standing, were suddenly abolished. I removed the Cerebellum, bit by bit, from another frog; walking. leaping, and standing were gradually abolished."* The facts were doubtless as he describes them; but I venture to say that if he had allowed half an hour to elapse, so as to allow the shock of the operation to pass away, he would have seen the frogs leaping and swimming as vigorously as ever. Every one who has experience in these matters will remember that unless the frogs so operated on (or even after the entire removal of the brain) were carefully secured, they were soon hopping about the house—to the horror of maid-servants, and amusement of the experimenter.

---

* STILLING: *Untersuchungen über die Functionen des Rückenmarks*, 145, 146, 155.

Here then is a patent contradiction: The Cerebellum, which has such marked influence over the movements of birds, has none at all, that is appreciable, over the movements of frogs. Those who suffer phrases to do duty for ideas, may get over this contradiction by saying that birds are "differently constituted" from frogs, and therefore the Cerebellum may have a function in the one which it has not in the other. But no biologist, who pretended to philosophy, would be satisfied with a phrase so vague; he would demand in what this said difference of constitution consisted. He would know that function is determined by anatomical connection, and would inquire what were the anatomical differences in these cases.

---

* FLOURENS, p. 51.

I was groping my way through this obscurity, when the experiments of Schiff seemed to flash a sudden streak of light. He showed that the strands of nerve-fibres which pass into the Cerebellum (called the *crura cerebelli*) are instrumental in fixing the vertebral column in the region of the neck; and that when an injury to the Cerebellum disturbs the harmony of movements, it is by acting on these neck-fixing parts. If the Cerebellum, he says, be cut through on one side, the movements become irregular; but if it be cut through on both sides, in a perfectly symmetrical manner, the movements do not at all become irregular.* No one need be told that the equilibrium of the body is maintained by the adjustment of the vertebral column. This must be notably the case with a long-necked bird; it is not, however, the case with the short-necked frog, or triton, which requires no such adjustment of its column to preserve equilibrium.†

After this refutation, it is interesting and instructive to read the array of facts which can be brought from

---

* SCHIFF: *Lehrbuch der Physiol.*, i. 355.

† Since this was written, the German translation of SCHRŒDER VAN DER KOLK'S work has appeared, in which he declares it has always been incomprehensible to him how men could believe the cerebellum to be the seat of co-ordination, which he places in the spinal chord, and he adduces the movements of decapitated frogs as proof.—*Bau und Functionen der Med. Spin.*, 1859, p. 68.

Comparative Anatomy to give countenance to the hypothesis: "On ascending the scale of mammiferous animals, we cannot but be struck with the rapid advance in the proportional size of the Cerebellum which we observe as we rise from the lowest (which are surpassed in this respect by many birds) towards man, in whom it attains a development which appears enormous even when contrasted with that of the Quadrumana. In proportion, in fact, as the extremities acquire the power of prehension, and together with this a power of application to a great variety of purposes—still more in proportion as the animal becomes capable of maintaining the erect posture, in which a constant muscular exertion, consisting of a number of most elaborately combined actions, is required—do we find the size of the Cerebellum and the complexity of its structure undergoing a rapid increase. Thus, even between the dog and the bear there is a marked difference; the latter being capable of maintaining for some time the erect posture, and often spontaneously assuming it; whilst to the former it is anything but natural." *

This evidence is valuable as indicating some connection between the Cerebellum and muscular movements, but it cannot weigh against the positive evidence just adduced. Nor can much countenance be gained from the evidence of Disease, although that also is frequently invoked. Irregularity of co-ordination is indeed sometimes found accompanying disease of the

---

* CARPENTER: *Human Physiology*, p. 521.

Cerebellum; but more frequently not; and it sometimes accompanies disease of the Cerebrum. Andral has collected ninety-three cases of diseased Cerebellum, and of these "there is *only one* which tends to favour the opinion of physiologists." Longet, who quotes this, remarks that inasmuch as the diseases were for the most part chronic, it is possible that the unaffected portions may have performed the co-ordinating function; and he considers the evidence of Experiment to be more conclusive. On the other hand, Ludwig curtly declares that all experiments on birds are valueless for the physiology of man, since no one has ever observed these phenomena of imperfect co-ordination when the human Cerebellum has been injured.* I agree with Longet that experiment is more conclusive; but it concludes *against* the hypothesis. Pathology, however, records one very striking case, which of itself would suffice to settle the question, because it shows us the *absence* of a Cerebellum in a human subject. It was first published by Combette, in the *Revue Médicale*, 1831, and is constantly alluded to. Mr Solly reports it thus, in his work on the Brain:—

"This was a case in which the Cerebellum was entirely wanting, nothing being found in its place but a *quantity of serous fluid* contained in the membranes: on each side a pedunculated body, not larger than a pea, was attached to the *corpora restiformia;* all the rest seemed replaced by a nervous sac. The pons Varolii, as well as the Cerebellum, were absent, and the individual in whom this remarkable lesion was observed had attained the age of eleven years.

---

* LUDWIG: *Lehrbuch der Physiol.*, i. 210.

"This case shows that *agenesia cerebelli*, or total want of the Cerebellum, does not necessarily render existence impossible, provided the other parts of the nervous centres be well conformed. The individual may even live for a considerable time. The child here was nearly twelve years of age, and we have reason for considering the disease as congenital; for had it been acquired—had this absence of the whole Cerebellum depended on an actual destruction of the nervous substance by an organic lesion, it is not probable the child would have survived so long.

"What were the phenomena observed during life? What effect did this complete atrophy produce on the intellect? What modification in the function of motion? The intellectual faculties were obtuse, though not to a remarkable degree; the answers slow and difficult; the whole countenance expressive of stupidity: in a word, the child, though not exactly idiotic, still showed a deviation of the mental powers. The motility was also modified; the power of motion was considerably weakened in the lower limbs, which did not possess their natural force and vigour; hence the child was unable to support itself with any firmness; it fell down frequently; the legs crossed each other during walking, and the gait was irregular and unsteady. At length the child was compelled to confine itself altogether to bed, and after some time was unable to stir, even when lying in a horizontal position.

Thus the modification of motility consisted in the gradual abolition of motion: to this were joined epileptiform convulsions, which continued for some time, and finally carried off the patient. The sensation of the integumental covering was not modified in any way whatever. There was no increase of sensibility in the commencement—no obtuseness or diminution of feeling, even when paralysis was most complete; the senses also remained intact. The child could see, hear, and taste, in a perfect manner. The functions of nutrition, circulation, and respiration were carried on without any notable disturbance."

This remarkable case has been quoted to prove the truth of the opinion advanced by Flourens; it is alluded to by Longet as one subversive of *all* the hypotheses that have been advanced. If we examine it by the light of the experimental canon previously laid down—namely, that "the disappearance of a function, on the removal of an organ, is no proof whatever that the function had its seat in that organ; but the *persistence* of a function, after the removal of an organ, is rigorous proof that the function had *not* its seat in that organ"—we shall clearly see that the opinion of Flourens is refuted by this case. The co-ordinating power was weak, indeed, at least in the lower extremities (nothing is said of the upper); but the whole motility was weak, and at last destroyed. Now the hypothesis which assigns co-ordination as the function of the Cerebellum, expressly denies that organ to be the seat of motility,—that being assigned to the Spinal Chord.

It is co-ordination alone which the Cerebellum is said to effect; and when the whole of that organ is removed, all co-ordination, at least of the extremities, is said to disappear. But this was assuredly not the case in the instance just quoted; the whole of the Cerebellum was absent, and only a slight *part* of the co-ordinating power was affected. This persistence of the function in the absence of the organ is a demonstration of the error of Flourens. Whatever were the causes which affected the motility of the child, who was half an idiot, they also necessarily affected her power of co-ordinating her movements.*

Two other cases are referred to by Brown-Séquard, wherein total destruction of the Cerebellum by disease was unaccompanied by paralysis, or any other trouble in the functions of animal life. "I have ascertained," he says, further, "that it is by the irritation they produce on various parts of the basis of the brain that the diseases of the Cerebellum, or its extirpation in animals, cause the disorder of movements which has been considered as depending upon the absence of a guiding power. In fact, the least irritation of several parts of the brain, with only the point of a needle, may generate very nearly the same disorder of movements that follows the extirpation of the Cerebellum." †

---

* Dr Robert Willis, in the valuable notes to his translation of *Wagner's Physiology*, takes a similar view: "The poor child was almost an idiot; but as she spoke, and walked, and used her hands, it is obvious that the Cerebellum could have had nothing to do with the highly complicated movements requisite in all these acts" (p. 637).

† Brown-Séquard: *Lectures* in *Lancet*, 27th August 1858.

Lockhart Clarke is also indisposed to accept the current hypothesis. "That the Cerebellum," he says, "is in some way concerned in the regulation and co-ordination of muscular movements, appears to follow from experiment and pathological investigation; but I think there are many reasons for believing that the mode of action usually assigned to it is unsatisfactory, and at variance with many established facts."*

It is an interesting illustration of the powerful bias which may be given to the mind by a striking experiment, or a captivating formula, that a multitude of facts which contradicted Flourens have either been disregarded, or have been only admitted as suggestive of doubts. But the reader will, I hope, now see that the hypothesis of the Cerebellum being the organ of muscular co-ordination is not doubtful — it is utterly erroneous.

II. THE CEREBELLUM A SEAT OF SENSATION AND VOLITION.—What, then, is the function of the Cerebellum? Unhappily no clear answer can be given to that question; and mainly because the answer has not been sought in the right way. All I can attempt at present is to show that the Cerebellum must be a seat of Sensation and Volition, because it is a great ganglionic centre, or group of centres, having the same tissues as those other cranial centres which every one admits to be endowed with Sensibility.

It has been held by very eminent investigators that the Cerebellum is *the* seat of Sensibility.* This is an exaggeration; but it suffices, even as an exaggeration, to show how many facts speak in favour of Sensibility in this organ. Mr Solly has quoted the following passage from Foville, which bears upon the point: "The opinion advanced by some physiologists that the Cerebellum is the regulator of the voluntary movements, if we attentively consider the reasoning on which it rests, seems to me to strengthen the idea which places the central seat of sensibility in the Cerebellum. After having injured the structure of the Cerebellum extensively, we have observed that animals preserved their power of moving their limbs, but had lost that of co-ordinating the movements of these in a manner convenient to station, progression, flight, &c. But when we *will* to perform, and actually perform, certain movements, do we not distinctly feel that we execute them? The man who, with his eyes shut, moves his hand or his arm, does he not also distinctly feel that he moves these parts as if he followed them with his eyes? While the paralysed man who, with his eyes shut, is desired to move his paralysed limbs, may be very willing to do so, though incapable and perfectly aware of his incapability of obeying. If this be true, how can we expect that an animal deprived of the faculty of perceiving the sensation of the movements which it executes, should execute them in the *ensemble* with harmony, and in accordance with a proposed end? How can we expect it to walk deliberately and to keep its equilibrium, if it does not feel the ground upon which it stands, if it is ignorant of the position in which its limbs are placed? I remember conversing with Sir Astley Cooper on this subject towards the end of 1830. Sir Astley cited to me the case of a man completely deprived of the faculty of sensation in one arm and hand, the muscular power of which was, however, preserved. When this man was desired to take hold of and to lift anything, he did so quite well; but if, whilst holding the object, his attention was taken away from the hand, irregular contractions of the limb commenced, and very soon the object fell to the ground: as soon as the patient ceased to follow the contractions of his fingers with his eyes, nothing remained to inform him that he held the object, when, of course, it escaped from his grasp. This, and other cases of a similar description, seem conclusive as to the fact of sensation being the true regulator of the muscular motions: it is by means of sensations that we are aware of the mode or degree of action in our muscles, that we have the power of co-ordinating their contractions in a suitable manner, and of executing a succession of voluntary movements in harmony with one another."

---

* LOCKHART CLARKE: *Philosophical Transactions*, 1853, p. 581.

---

* Compare LONGET, *Traité*, ii. 263, for authorities. He himself thinks that it participates in sensation but is positive, that it is not the *exclusive* seat.

Indeed there will be few physiologists now to dispute the position, that the facts which indicate the Cerebellum to have a considerable share in muscular coordination, also prove that it must have a share in

Sensation, since without Sensation no co-ordination is possible. What that share precisely is, we cannot say. It is clear that the Senses are not dependent on the Cerebellum; for the girl in Combette's case was in perfect possession of Sight, Hearing, and Taste. It is equally clear that Pain has not its centre there. But nothing can be said very decisively as to the other sensations.*

Has the Cerebellum any share in the intellectual operations? Flourens and Bouillaud declare not; but Longet properly remarks, that inasmuch as animals deprived of that organ survive but a little time, and generally in a state of great agitation, it is difficult to form an opinion as to the state of their intelligence. Nor can much reliance be placed on pathological cases. Very frequently no trouble whatever of the intellectual faculties seems to accompany disease of the Cerebellum; but inasmuch as disease of the Cerebrum is also frequently unaccompanied by disturbance of the intelligence, no conclusion can be founded on that. Disease sometimes notably disturbs the intelligence; and in Combette's case the girl was nearly idiotic.

When, therefore, we consider the great similarities in structure and position between the Cerebrum and Cerebellum; and when, further, we reflect that the Cerebellum in the various stages of the animal scale is always found to be developed in proportion to the intellectual development of the animal, being beyond all comparison greatest in man, and smallest in fishes, there is, it must be confessed, reason to suspect that the Cerebellum also *participates* in some degree in the production of mental phenomena. At the same time we must admit that there is no direct evidence on the point, either for or against it.

---

* Dr NOBLE, in his work, *The Human Mind in its Relations with the Brain and Nervous System*, p. 49 *seq.*, advances the hypothesis that the ganglionic masses of the Cerebellum named *corpora dentata* are the seat of tactile sensibility.

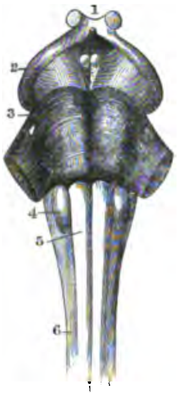
III. FUNCTIONS OF THE MEDULLA OBLONGATA.—Besides the Cerebrum and Cerebellum, there are other smaller ganglionic masses at the base, the most important of which are the *corpora striata* and *optic thalami* (Fig. 52, 3, 4, p. 122), and each of which must play some important part in the nervous mechanism. But what that part is, can in no case be stated with even an approach to certainty. The hypotheses are numerous and contradictory;* but except the share which the *corpora quadrigemina* have in vision, there seems to be no solid ground of fact yet ascertained. We will therefore pass over these ganglia, and direct attention to the third important organ of the encephalon—the *Medulla Oblongata.*

Although included among the parts of the Brain, or encephalon, because it is included within the cavity of the skull, and because its columns pass into both Cerebellum and Cerebrum, the *Medulla Oblongata* is

---

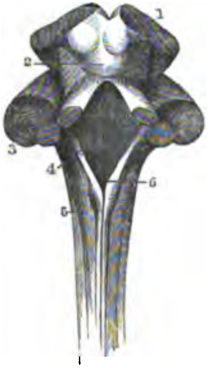
* The latest I have met with is that of Dr NOBLE, who regards the optic thalami and corpora striata as the seat of emotional sensibility.—*The Human Mind*, p. 60.

Fig. 53.



FRONT OR UNDER VIEW OF THE MEDULLA OBLONGATA.
1, The Optic Nerves; 2, *Crura Cerebri*, or strands of white matter running into the hemispheres; 3, *Pons Varolii*; 4, Olivary Bodies; 5, Anterior Pyramids; 6, Spinal Chord.

Fig. 54.



BACK VIEW OF THE MEDULLA.
1, Section of the Optic Thalamus; 2, Corpora quadrigemina; * 3, Section of the *Crura Cerebelli*; 4, Fourth Ventricle; 5, Restiform Bodies; 6, *Calamus Scriptorius*.

really and truly the upper part of the Spinal Chord, and bears more resemblance to the Spinal Chord than to any portion of the Brain. This, which is obvious on a superficial examination, becomes still clearer on a microscopic investigation; and although the point is of little importance as a matter of naming, it will presently be seen to have considerable importance when we come to the interpretation of functions. In Figs. 53 and 54 we see the Medulla in its connections with the Brain and Chord.

Such is the external aspect. Respecting the internal structure little can here be said, since there is probably no anatomical question which can surpass it in complexity and obscurity. The student desirous of pursuing the subject will do well to prepare himself by studying the important works noticed below.[*] It will suffice for our purpose if we can bear in mind that the Medulla is really a prolongation of the Spinal Chord, to which there are some additions both of grey and white matter.[†] It has ganglionic masses peculiar to it, named the *Olivary bodies* (Fig. 53, 4). It has also a great variety of commissural fibres, by means of which an immense variety of actions are co-ordinated. Sensory nerves and motor nerves issue from it; but these do not display the symmetry of arrangement noticed in the nervous pairs issuing from the Chord. It is also the ganglionic centre for the nerves of Hearing and Taste.

---

* The two lower bodies are imperfectly distinguished in the engraving.

If we now ask what are the functions of this complex part of the Encephalon, we shall find that the ever-recurring question as to whether the Brain is or is not the exclusive sensorium, may here receive considerable illustration. We shall also find that there was something more than a mere question of naming involved in the doubt as to whether or not the Medulla belonged to the Brain. For observe, the Medulla is now almost unanimously declared to be a seat of sensation and volition. If, therefore, it be *not* included in the term Brain, there can be no longer any question as to the Brain being the exclusive sensorium; and if the upper part of the Spinal Chord can be a seat of sensation and volition, there can be no valid argument against the evidence which proves that the rest of the Spinal Chord is also a sensorial seat.

---

[*] The two works of STILLING (*Ueber die Medulla*, 1843, and *Ueber den Bau des Hirnknotens*, 1846), form the storehouse whence succeeding writers have gratefully drawn their chief materials, the only considerable additions being due to SCHRŒDER VAN DER KOLK (*Bau und Functionen der Medulla Spinalis und Oblongata*); and LOCKHART CLARKE (*Philosophical Transactions*, 1858), whose admirable researches have been duly appreciated by the few persons in Europe competent to estimate them. In citing the last-named anatomist, I cannot here forbear acknowledging how much I owe to the readiness with which he initiated me into his method of making microscopic preparations.

[†] "It is well known that in the spinal chord the columns of white and grey substance are parallel to each other, and preserve the same relative position throughout their entire course; but in the Medulla Oblongata these parts not only assume a different arrangement, by becoming more or less blended with each other, and with new structures, but frequently pursue a curvilinear direction in different planes, inclined at varying angles."—LOCKHART CLARKE.

Müller says: "*It is the seat of volition.* The experiments of Flourens show that animals in which the cerebral hemispheres have been removed, though in a state of stupor, are still capable of executing voluntary movements; they have still this power after the removal of the cerebellum also." * I am not sure that I rightly understand this; but we may suppose him to mean that the Medulla is *a* seat of volition; that it is not *the* exclusive seat, will be evident when I narrate my own experiments, which show that animals manifest volition after removal of the whole Brain, including the Medulla. Müller thus continues:—

"*It is the seat of the faculty of sensation.* This is not merely shown by the anatomical fact that all the cerebral nerves, with the exception of the first and second (olfactory and optic), are connected with it, or with its prolongations into the Brain; it is proved also by experiment."

---

* MÜLLER: *Physiology*, i. 828.

The reader is requested to pay particular attention to the passage just quoted, and to the assertion that the Medulla must be the seat of sensation because nerves are connected with it. Schrœder van der Kolk, who also maintains that the Medulla and the ganglia at the base of the Cerebrum constitute the sole sensorium,* is more specific than Müller, for he says that the Medulla must be "the seat of Perception, the spot where sensory *impressions* become *sensations*," because it is there (as he supposes) that the sensory nerve-fibres terminate in ganglionic cells. Now I ask, if the fact that nerves are connected with ganglionic centres is a proof that these centres must be the seat of sensation; how can we deny that *similar* ganglionic centres lower down the Spinal Chord, which are *similarly* connected with sensory nerves, must also be the seat of sensation? And this question becomes still more pressing when we learn, as we shall presently, that the same kind of experiments as those which indicate the Medulla Oblongata to be a seat of sensation, no less decisively indicate the Spinal Chord to be one. The only *possible* answer which occurs to me is this: The nerves which issue from the Medulla Oblongata, and the ganglia above it, are nerves of *special* sense—namely, the auditory, gustatory, olfactory, and optic; each of these nerves, being in connection with a special apparatus, is capable of producing a different effect from that of all other nerves. It is only the nerves of special sense which can create sensation.

---

* SCHRŒDER VAN DER KOLK: *Bau und Functionen*, p. 88.

Easy as it is to refute such an answer, I have thought it worth while to adduce it, because Logic irresistibly forces us to conclude, that unless sensation be the product of a *particular* group of nerves and their ganglia, it must be the product of *all* nerves and their ganglia. Unless some reason can be shown why the apparatus of Sight, Smell, Taste, and Hearing, has the peculiar privilege of awakening sensation, the mere connection of a nerve with its ganglion must suffice for sensation. Now, that these special organs of sense have no such peculiar privilege is very evident, when we consider the facts. Without here insisting on muscular sensations and visceral sensations, on the multitudinous forms of sensation I have grouped under the head of Systemic consciousness, it is enough to refer to the sensations of Touch, Temperature, and Pain. No one will be hardy enough to deny that these are veritable sensations; yet these are not capable of being produced at all by the nerves of Sight, Smell, or Hearing. They have no special apparatus. There is no one tactile nerve and tactile ganglion: all the skin-nerves are nerves of touch, all produce sensations; and these sensations, there is ample evidence to believe, are produced when the whole of the brain, including the Medulla, is removed.*

This view of the relation of nerves to centres, and the consequent extension of sensation to all parts of the cerebro-spinal axis, is not so entirely novel as it may at first appear. The only real novelty is in its anatomical precision. Sir Charles Bell, to cite only one example, had a strong though dim conviction that it was an error to limit sensation to the action of the special senses. "It appears to me," he says, "that the frame of the body, exclusive of the special organs of seeing, hearing, &c., is a complex organ, I shall not say of sense, *but which ministers, like the external senses, to the mind;* that is to say, as the organs of the five senses serve to furnish ideas of matter, the framework of the body contributes, in certain conditions, to develop various states of the mind."* Although this is very vague, and, because of its vagueness, has found little acceptance, we see that the phenomena had not escaped Bell's attention. As he proceeds, he becomes still more vague; and thinks that because a blow on the eye will produce the sensation of a flash of light, which proves that the organs of the senses can be exercised when there is no *corresponding* outward impression, we can comprehend how other organs of the body may have a relation established with the mind without reference to outward impressions. As an anatomist, it behoved him to indicate by what kind of mechanism this relation was established. I have done so, by referring to the one mechanism which, in the case of the external senses, we know to be operative—namely, the connection of nerves with their ganglia: wherever a nerve excites a ganglion, there will be sensibility. The visceral-nerves excite their ganglia, the muscle-nerves excite their ganglia, the skin-nerves excite their ganglia, and the nerves of special sense excite their ganglia: the physiological process is in each case similar, although the sensations produced are necessarily various. Bell seemed to be very near the truth at one time, as may be gathered from the following passage:—"It is a singular fact in the history of physiological opinions, that the heart, an organ the most susceptible of being excited by the agitations or derangements of the body, should have been considered at one time as insensible. And yet in one sense it is true that it is so. To actual touch the heart is insensible, as was exhibited to the illustrious Harvey, in the person of a young nobleman who had the heart exposed by disease. This single circumstance, had there been no other evidence, should have earlier directed physiologists to a correct view of the matter; from its proving that the *internal organs are affected and united by sensibilities altogether different in kind from those bestowed upon the skin.* The sensibility of the external surface of the body is a special endowment adapted to the elements around, and calculated to protect the interior parts from injury. But though the heart has not this common sense of touch, yet it has an appropriate sensibility, by which it is held united in the closest connection and sympathy with the other vital organs."* Had Bell not shared the universal prejudice respecting the Brain as the exclusive seat of sensation, such considerations would have led him, I think, to the truth.

---

* The attempt to explain away such evidence, on the Reflex Theory, will be refuted by-and-by.

---

* BELL: *The Anatomy and Philosophy of Expression,* 4th edit. p. 83.

---

* BELL: *Anatomy, and Philosophy of Expression*, p. 86.

In the course of our demonstration that every ganglionic centre must be a seat of Sensibility, we have found that the majority of physiologists now regard the Medulla Oblongata as a sensational centre; and their reasons for so regarding it, both anatomical and experimental, we have said, apply with equal force to the whole of the Spinal Chord, of which, indeed, the Medulla is but the upper part. Having elicited this, we will now call attention to another interesting hypothesis which has been advanced respecting the Medulla.

IV. IS THERE A VITAL POINT?—It has been demonstrated that the Medulla Oblongata is the centre for the nerves of Respiration. The various actions involved in speech, in swallowing, sneezing, &c., are also co-ordinated here. It is because the actions concerned in Respiration have their centre here, that any other part of the spinal axis may be destroyed without destroying life; but destruction of the Medulla in mammals has been known, since the time of Galen, to bring with it instantaneous destruction of life. This fact was lighted up with sudden interest when Flourens announced that he had discovered a particular spot, a mere point, the extirpation of which caused instant death. He called this the *nœud vital*, or vital point, because every other part of the organism might be removed without death.

In that part of the grey substance of the Medulla called the "floor of the fourth ventricle," and, because it is shaped like a V, called the *calamus scriptorius* (nib of the pen), there is a smaller v to be seen (Fig. 54, 6, p. 138). The point of this second v, not larger than a pin's head, is the "vital point," according to Flourens. When experiment had verified the assertion that death instantly followed the destruction of this point, physiologists quickly perceived that the explanation of the fact was not that here life had its special centre, but that here was the special centre for the co-ordination of the breathing mechanism. Indeed, the fact that reptiles will live for months without their heads, sufficed to disprove this notion of a "vital point."

Since this passage was originally written, I have learned that the very fact upon which Flourens rests his hypothesis, is not correctly conceived by him. Schiff and Brown-Séquard have both removed this "vital point" without destroying life—the animal continuing to breathe and live for five or six days. Their conclusion is, that when extirpation of the point causes death, it is because of the irritation of the neighbouring parts in the Medulla suddenly checking the action of the heart.*

V. INFLUENCE OF ONE CENTRE ON ANOTHER.—The reader will perhaps have felt some surprise in learning that the irritation of a nerve-centre arrests, instead of exciting, the action of the heart. The fact is indubitable, whatever may be the explanation. The heart, as we formerly learned (vol. i. p. 332), possesses its own independent ganglia, in virtue of which it continues pulsating long after death, and even after removal from the body. But if the pneumogastric nerves be galvanised, these nerves, although they send filaments to the heart, will not cause the heart to beat faster—they will arrest its beating. This was first shown in an experiment devised by Weber. He transmitted an electric current through the pneumogastric, and the heart suddenly stopped; after allowing it to resume its beats till they reached the normal rapidity, he again stopped them by another current.*

---

* SCHIFF: *Lehrbuch der Physiol.*, i. 323. BROWN-SÉQUARD: *Journal de la Physiol.*, i. 217. *et seq.*

That which is effected by the electric current may be effected by irritation of the Medulla Oblongata. The galvanic stimulus acts in the same way as the stimulus of the nerve-centre. There is one point, however, to which especial attention should be called. Although the electric current arrested the beating of the heart, in the experiments just noticed, it would have *quickened* the pulsation had the current been slighter. The same phenomena will follow a powerful and a moderate excitement of the Medulla: the powerful excitement arresting, and the moderate accelerating, the pulsations.

---

* This experiment has since been repeated in various ways. See ECKHARD, *Beiträge zur Anatomie und Physiologie*, p. 147. LONGET failed when he tried the experiment, owing to an incorrect application of the method.

Hence it is that sudden death may result from the "shock" of grief or of joy. Hence it is that a strong emotion makes the heart palpitate. Every one knows the effects of a burst of strong emotion: there is a momentary cessation of the pulse, followed by an agitation: at first our "breath is taken away," and then we shout, or cry, or tremble. Some faint under a powerful shock; others die.

While noticing this influence of the Medulla on the heart, we must not omit to mention the important fact that *pain* diminishes the heart's action. Indeed, pain will sometimes cause death in this way. When patients die under an operation, it is seldom from the loss of blood; it is from the effect of pain on the heart's action, combined with the previous weakening effect of the hemorrhage. This has been proved by experiment. On the other hand, Wilson Philip found that when a rabbit had been stunned by a blow on the head, its Medulla might be destroyed without an arrest of the heart ensuing.* Claude Bernard also found that if rabbits were rendered insensible by etherisation, their Medulla could be destroyed without destroying life; whereas, if the Medulla be even irritated without previous etherisation, death inevitably ensues from cessation of the heart's action.† The immense importance of chloroform, and other anæsthetic agents, in severe operations, is thus made physiologically apparent.

---

* WILSON PHILIP: *Experimental Inquiry into the Laws of the Vital Functions,* 1817, p. 72.

† CLAUDE BERNARD: *Système Nerveux,* i. 384.

It is a consequence of the wonderful complexity of our organism, in which each part plays on another, that remote and unsuspected influences produce important results. Mental agitation will suddenly arrest or increase the secretions; imperfect, or too abundant, secretion will depress, or confuse the mind. An idea will agitate the heart, and disturb the liver; a languid liver will disturb the serenity of the mind; a worm in the intestine will produce melancholy, and even madness.—So indissolubly is our mental life bound up with our bodily life.

VI. SUMMARY OF THE FUNCTIONS OF THE BRAIN.—We have now gone over, one by one, the cardinal facts which have been ascertained respecting the functions of the various parts of the Brain; and these are the chief results to which the inquiry has led:—

1. All the ganglionic masses have one property in common, which is Sensibility; and they differ only in their functional manifestation of this property, according to their connections with different organs.

2. The Cerebral hemispheres have the functions of Intelligence and Emotion. Probably also some other functions.

3. The Cerebellum has functions which remain at present undetermined.

4. The Medulla Oblongata has the functions of ministering to the respiratory and many other reflex actions. The special senses, Hearing and Taste, have their centres in this organ; as Sight and Smell have

their centres in the corpora quadrigemina and olfactory lobes.*

5. Inasmuch as all these centres have Sensibility, they must all be organs of the Mind, because they must all minister to the general Consciousness.

6. The facts and arguments which establish the conclusion that the Mind cannot be localised in any *one* part of the Brain, such as the Cerebrum, Cerebellum, the ganglia at the base, or the Medulla Oblongata, but that each of these parts has its own special functions, and contributes its own special forms of consciousness, lead to the suspicion that other parts of the nervous system may also contribute other special forms of consciousness.

It is this suspicion we shall examine minutely in the ensuing section.

---

* There are grave doubts respecting the olfactory lobes, which will be discussed in the next chapter (OUR SENSES AND SENSATIONS), but for the present we may follow the doctrine of the schools.

CHAPTER IX.

(*Continued.*)

THE MIND AND THE BRAIN.

SECTION III.—THE SPINAL CHORD AND ITS FUNCTIONS.

Structure of the Spinal Chord—Imperfection of our microscopical knowledge—Difference between an organism and a mechanism: the organic mechanism is vital and sensitive—Theory of Reflex-Action: Marshall Hall, Müller, and Prochaska—Error of excluding sensation—Grainger's advocacy of the theory—Schrœder's reflex-fibres—Sensation without the brain: experiments on insects—Marshall Hall's experiments—Spontaneous motion in headless animals—Reflex-action in the Brain—Hydrophobia—Voluntary and involuntary actions—What is volitional control?—Volition dependent on sensation—Growth of a volition—Control over our thoughts—Involuntary actions becoming voluntary—Spinal Chord a sensational centre: views of earlier writers; the deductive evidence; the inductive evidence—Evidences of spontaneity and choice in decapitated animals—Two centres of sensibility established in one animal—Are walking and swimming reflex-actions?—Experiments on puppies and rabbits—Examination of the evidence against the sensibility of the chord—What share has the Spinal Chord in our actions?

I. Structure of the Spinal Chord.—Formerly the Spinal Chord (or Spinal Marrow, as it is still sometimes called) was held to be a large nerve-trunk issuing from the brain, and giving off numerous branches which supplied the body. Although this idea is now completely abandoned, and the Chord is known to be a great centre, or series of centres, intimately connected, as the several vertebræ are connected into one spinal column, yet men still cling unconsciously to some of the old conceptions, and are disposed to regard the Chord as a *conductor*, rather than as a *centre* of Sensibility. It is this conception we must irrevocably dismiss from our minds.* If the Chord seems to act sometimes as a conductor only, it is merely because nerve-centres once excited play upon each other; and in this sense the brain is also a conductor. Every segment of the Spinal Chord *may* act separately as an independent centre; every segment is a little brain.

The Chord commences at that imaginary line where the Medulla Oblongata is supposed to end. It terminates in man about the second lumbar vertebra; in most animals it extends along the whole length of the spinal column; but there are great varieties in this respect. It is composed of six *external* columns of white fibrous matter—namely, two anterior, two posterior, and two lateral; and of four *internal* columns of grey, ganglionic matter, called respectively the two anterior, and the two posterior, horns (*cornua*). A minute canal runs through the centre, which may be regarded as a prolongation of the fourth ventricle of the brain.

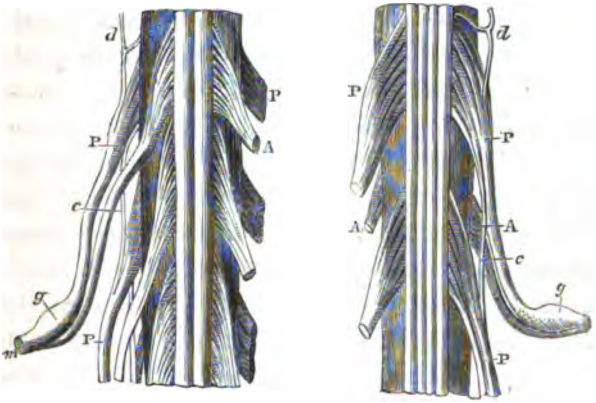
---

* It seems to me that the recent experiments of SCHIFF, BROWN-SÉQUARD, and others, which have excited so much discussion, and which attempt to determine the conductibility of different parts of the chord, are all vitiated by this initial mistake.

Fig. 55 represents the anterior and posterior aspects of the Chord with the double origin of its nerves.

As we are not here called upon to understand the whole anatomy of this organ, further details would be superfluous. The student will find ample information in the works named below.*

Fig. 55.



SPINAL NERVES AND SPINAL CHORD.—(After Bernard.)

1, Anterior view; 2, Posterior view. A is the anterior root of the nerve, and P the posterior root. One pair in each figure is represented with the division made below the ganglion (*g*). The other pairs are divided above the ganglion. At *c* and *d* are filaments connecting two posterior roots.

Of late years there has been an immense expenditure of labour in the attempt to unravel the microscopic structure of the Spinal Chord; but so excessively difficult is this task, that one may fear it will be a long while before Physiology will be in a condition to base safe conclusions on the results ascertained. This remark is especially addressed to students, as a warning against their being misled by the confident and plausible statements and deductions to be met with in modern works. I have been led to pay great attention to this subject; and have not only made a vast quantity of preparations of the chord of man, oxen, sheep, pigs (adult and newly born), cats, kittens, puppies, moles, rats, lizards, frogs, and fishes, many of which have been prepared according to the various methods employed by Stilling, Kölliker, Schrœder van der Kolk, Gratiolet, and Lockhart Clarke, so as to test the various statements of these authorities; but I have also had the advantage of seeing some of the best English and German preparations: the result of all this has been the conviction, that whoever propounds an hypothesis on the basis of the facts hitherto ascertained, is in danger of building on a sandbank.*

---

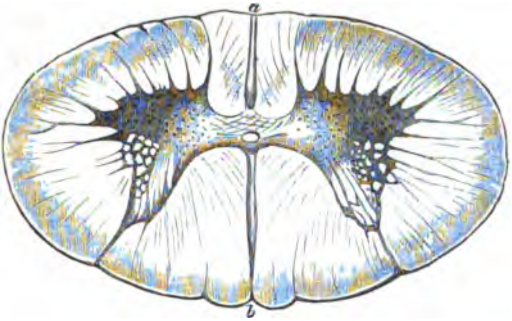
* ROLANDO: *Ricerche anatomiche sulla struttura del Midollo Spinale.* GRAINGER: *Observations on the Structure and Functions of the Spinal Chord.* TODD and BOWMAN'S *Physiological Anatomy.* TODD: *Anat. of the Brain, Spinal Chord, and Ganglions.* LEURET et GRATIOLET: *Anat. Comparée du Système Nerveux,* tome ii. HIRSCHFELD and LEVEILLE: *Névrologie.* QUAIN'S *Anatomy,* edited by SHARPEY and ELLIS.

A transverse section of the Chord, if sufficiently thin, and rendered properly transparent by any of the accepted modes of preparation, presents the appearance indicated in Figs. 56, 57, 58.

---

* KÖLLIKER, in his last edition, expresses the same scepticism. His words read like a sarcasm against SCHRŒDER VAN DER KOLK, whose confidence is astonishing, and whose published figures represent, I venture to say, what he *imagines*, not what can be *seen*. This will be evident to any one who adopts the method prescribed by SCHRŒDER; still more so, when we find that anatomist giving up his method in favour of LOCKHART CLARKE'S, which most certainly does *not* show what SCHRŒDER represents.

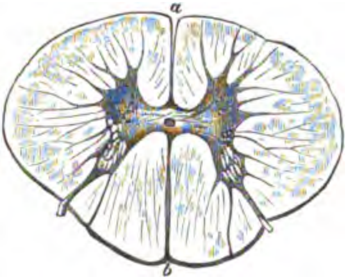
Fig. 56.



Transverse Section of Spinal Chord in Man (Cervical region).

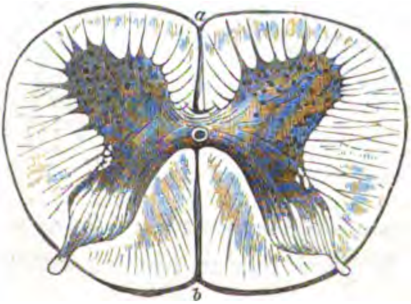
a, Fissure of the anterior columns; *b*, Fissure of the posterior columns. The central masses are the anterior and posterior horns of grey matter.

Fig. 57.



Transverse Section of the Spinal Chord (Dorsal region).

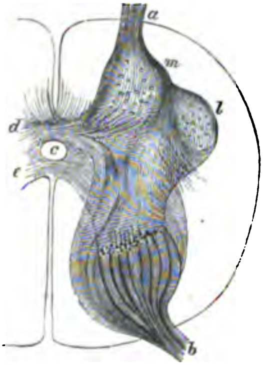
Fig. 58.



Transverse Section of the Spinal Chord (Lumbar region.)

These figures are copied from Gratiolet's Atlas. By way of comparison, we may consider Fig. 59, which is copied from Kölliker, and represents one-half of a section made in the lumbar enlargement of a child's chord.

Fig. 59.

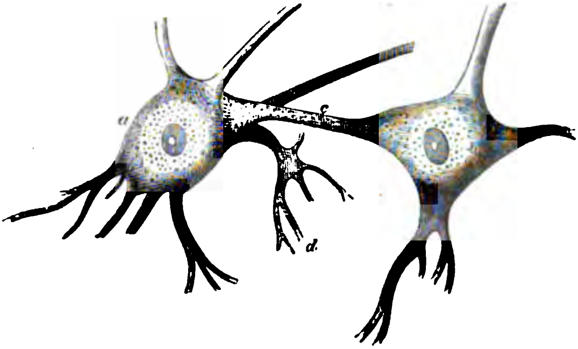


TRANSVERSE SECTION OF SPINAL CHORD IN A CHILD (Lumbar region).

a, Root of anterior nerve entering the columns of ganglionic cells *m* and *l*; *c*, Central canal; *d* and *e*, Anterior and posterior commissures; *b*, Root of posterior nerve entering the posterior horn of grey matter.

Having made ourselves acquainted with these aspects of the internal structure, we may look a little closer at the groups of large ganglionic cells to be found in the anterior horns of grey matter. They vary considerably in shape and size. A general idea may be gained from Fig. 60.

Fig. 60.



NERVE-CELLS FROM SPINAL CHORD.—(After Gratiolet.)

These ganglionic cells are identical with those found in the brain; but because they are larger than the majority of those found in the posterior horn, they have been supposed to be motor-cells, by a logical process not very admirable, as it seems to me. The anterior roots of the nerves are, as you know, held to be motor only; the posterior roots sensory. We have already seen that this is a misconception; but even on the supposition that it is correct, we shall need some other evidence for believing that the anterior horn of grey matter is *therefore* motor. The notion seems to me entirely baseless. Still less can I accept the notion of Jacubowitch that the largest cells are motor, those of the second size sensory, and the smallest sympathetic. What may be seen, or imagined, in preparations which are imperfectly transparent, must yield to what is seen in preparations made on a better method,* and so transparent that one may read such characters as in a well-printed book. In such sections I affirm that so far from there being any necessary connection between large cells and the motor function, there is no constancy whatever as to size. In the ganglia of two indubitable motor-nerves—the *spinal accessory* and the *hypoglossus*—the size varies greatly. In the posterior horn there are not only large cells† (less abundant, indeed, than in the anterior), but the largest cell I have ever seen, was one with seven processes in the posterior horn of an ox. And while there are large cells in the posterior, there are abundance of small cells in the anterior horn. Indeed, the idea of properties so widely separated, as Sensibility and Motility, being dependent on slight differences in the size of identical cells, is eminently unphilosophical.

---

* See note at the conclusion of this chapter.

† BIDDER and KUPFFER deny the existence of these cells in the posterior horn. The same denial is made by OWSJANNIKOW: *De Medullæ spinalis textura*, p. 37; and by METZLER: *De Medullæ spinalis avium textura*, p. 30, who declares that what have been mistaken for nerve-cells are nothing but lacunæ in the web of connective tissue. In LOCKHART CLARKE'S preparations, and in my own, the cells are

But we must pause no longer on this microscopic structure;* we have ascertained that the spinal chord is a series of centres, having ganglionic substance in its interior, which is in connection with the roots of the motor and sensory nerves. In a word, it has the essential characters of a series of ganglia; and minute investigation discloses that these ganglia are essentially similar to those of the Brain. We shall never under-

---

unmistakable, and their well-defined nucleus prevents any possibility of doubt. From this one statement it is certain that the preparations of the Dorpat School must be very inferior in distinctness to ours; yet these writers profess to see, and they give drawings of, appearances which can by no means be made out in our preparations; whence it is easy to conclude that they describe what they imagine to be the real structure, not what can positively be seen.

* The student is referred to BIDDER *und* KUPFFER: *Untersuchungen über die Textur des Rückenmarks,* and KÖLLIKER's reply in the *Zeitschrift für Wissenschaftliche Zoologie,* ix. 1; also KÖLLIKER: *Gewebelehre,* 3d edit.; LEYDIG: *Histologie;* LOCKHART CLARKE: *Philos. Transactions,* 1851 and 1853; LENHOSSEK: *Annales des Sc. Nat.,* 1857, vii.; LEURET *et* GRATIOLET: *Anat. Comp.,* ii.; SCHRŒDER VAN DER KOLK: *Bau und Functionen der Med. Spinalis und Med. Oblongata;* OWSJANNIKOW: *De Med. spin. textura;* METZLER: *De Med. spin. avium textura.* STILLING'S last work I have not seen; but it is said to be the most encyclopædic treatise yet produced.

stand the functions of the nervous system unless we fix firmly in our minds the conception of the cerebro-spinal axis as one whole, the functions of the various parts differing from each other, only as they are differently related to the rest of the organism. Bergmann and Leuckart do not hesitate to define the "brain as the anterior enlargement of the spinal chord;" and this definition, they remark, makes it less paradoxical to find the brain so slightly developed in the lower vertebrate animals, and not existing at all in the Amphioxus.* In the invertebrate animals, there can be no shadow of doubt that the ganglia which represent the brain of higher animals are absolutely, and in all respects, identical with the other ganglia, which represent the spinal chord.

But our attention is now demanded by the important questions raised respecting the functions of the Spinal Chord. The chief of these functions (by most writers regarded as the sole function) is the production of movements in the trunk and limbs. These movements may either be produced in obedience to the mandates of the Brain—as in voluntary motions; or independently of the Brain—as in involuntary motions, which take place by what is called the Reflex-Action of the chord.

II. THE VITAL MECHANISM.—What is meant by Reflex-Action, and what is the Reflex-Theory, so continually invoked by modern writers? Several times already we have been forced to use these terms;* and now we have reached a stage in our inquiry when a distinct understanding of them becomes indispensable. But inasmuch as the Reflex-Theory implies the conception of the animal *organism* being to a great extent a *mechanism;* and further, inasmuch as our ordinary conception of a mechanism carries with it the idea of actions performed unconsciously, without sensation; it is absolutely necessary that we should accurately determine the sense in which we are to use, and understand, such a phrase as "the vital mechanism."

---

* BERGMANN *und* LEUCKART: *Vergleichende Anatomie*, p. 515. Compare also TODD: *Anat. of Brain and Spinal Chord*, pp. 2, 77.

The results of scientific investigation during many centuries have been gradually, and of late rapidly, tending to the conclusion that the animal organism is constructed on principles analogous to those which determine the construction of a mechanism. It acts like a mechanism; and differs from one, only in the greater *complication* of its parts and its effects. On the other hand, many persons feel a very natural repugnance to such a conception. Life seems to them the antithesis to mechanical action; the very notion of a mechanism is not that of life, but of a simulacrum of life. Without life, the organism is without action. We cannot imagine the watch performing its actions prompted by any sensation; we cannot imagine the organism performing its actions *without* life. This is the source of the repugnance, often felt, against conceiving the organism as a mechanism; but this repugnance will subside, if we reflect on the wonderful adjustment of part to part, in a complicated whole, and compare it with the somewhat similar adjustments of a mechanism ; bearing in mind that this *similarity* is not *sameness*, and that the organism, although a mechanism, is a *vital* mechanism, this vitality being the source of profound differences. Some theorists have overlooked the important element of vitality ; and have imagined that a vital, *sensitive* machine, can act like an *ordinary* piece of mechanism, which is not sensitive; but this error should not force us to give up a very useful analogy. We may still cling to the fact that the *adjustments* of the complex organism determine its activities : the muscles set moving by a peculiar stimulus, are as necessarily determined by their organic adjustments, as the wheels of a watch are set moving in a particular way, and are determined in their particular action by the peculiarity of *their* adjustments. When a stimulus to the respiratory nerves brings into play the group of muscles which expand the chest, and these, in turn, bring into play the group which contracts the chest—when the stimulus of food to the nerves of the alimentary canal determines the action of the digestive mechanism, muscular and secretory—when the stimulus of pain sets going the actions of flight, struggle, or shrieking,—we have a series of actions which are *mechanical,* inasmuch as they depend on the adjustment of parts, like the adjustment of cogged wheels ; and are *vital,* inasmuch as they depend on vitality for their performance. With this qualification, the idea of a mechanical construction becomes important. We perceive how the various actions of an animal are dependent on the adjustment of its organs. Once set this mechanism going, and it can only act in the determinate direction permitted to its peculiar adjustments; and in so far it acts like a watch, or a steam-engine. The organism of the chick is so adjusted that it can peck up the grains; and it does so immediately the stimulus given to its sensational centre, through the optic apparatus, sets going the first of the necessary movements. It will do this as soon as it leaves the shell, or even before it leaves the shell. In like manner the Snapping Turtle, according to Agassiz, snaps as fiercely when it is still a colourless and undeveloped embryo, wrapped up in the fœtal membranes, as it does in after-life. This surprises us at first, because we are accustomed to attribute such actions to "a conscious and voluntary" effort. On getting over our surprise, and learning to regard these actions not as prompted by any distinct ideas, but simply as the results of an organism peculiarly constructed to act in this way—what we have to guard against is the error of those who, fixing their attention on the *mechanical adjustment*, overlook the fact of the *sensational guidance.* It is true that the animal mechanism, when set going, acts like the mechanism of a watch; but to *set* it going, and to *keep* it going, there is needed the constant presence of sensation. This has been frequently forgotten, and even denied. The mechanism has been credited with the whole phenomenon. That sensation is a necessary part of the mechanism—the mainspring of the watch—the fuel of the steam-engine—seems, singularly enough, to have been overlooked. The marvellous way in which the sensational element is bound up with the mechanical element, so that our very sensations may be said to be organised in us, is illustrated in a thousand examples of what are called "automatic actions." A piece of music with which we are familiar may be performed "mechanically," without any "effort" or "attention;" indeed, while we are talking to some one, or thinking of something else. This we are said to do "unconsciously;" and if Consciousness be restricted to Thought-Consciousness, there is no objection to calling these automatic actions unconscious; but there is every objection to calling them actions independent of sensation. The sensations excited by each note, and determining each movement of the muscles, are indispensable to the performance of music; the presence of *guiding sensations* suffices; but without this presence, the action is impossible. Although the mechanism of the fowl, whose cerebrum has been removed, is uninjured except as regards one of its sensational centres, this mechanism will not suffice to keep the bird from starvation amidst heaps of grain; because to set going the necessary train of actions which would result in pecking, there is needed the stimulus from the cerebral centre. If the grains be placed within the beak of this bird, they will not be swallowed, unless they roll near the sensitive part of the throat, and *then,* the stimulus of the sensation being given, the swallowing actions are set going, and on these follow the digestive actions. The theorists whom I have chiefly in view at present, deny that swallowing depends upon sensation—they call it simple reflex-action; an opinion which we shall presently examine. Enough, in this place, if we have made clear to ourselves the following positions:—

---

* See the explanation given at p. 55-59.

An organism is a mechanism, and acts mechanically, inasmuch as its actions are *necessarily determined by the adjustment of its organs.* But the organism differs from the mechanism in being vital, in having Sensibility as its mainspring; and its actions, which are called mechanical or automatic, are all *determined by the impulse of guiding sensations.* It further differs from a mechanism in its development of new adjustments and new capabilities. No machine has any self-development. What its parts could perform last week, they perform this week, and no more. But the organism develops. New adjustments are formed, so that a combination of actions, which to-day is impossible, will become easy, perhaps, at the end of a month. I do not, however, conceive it to be rational to suppose that a group of muscles which originally required the stimulus of a nerve-centre, as a guiding sensation, to move them, will at any time move without that stimulus; although it is very conceivable that the *secondary* sensations which the movements, when they were difficult, necessarily excited, should frequently be absent, now the movements are so easy that they claim no attention. I can conceive the possibility of *having* a sensation without *thinking* of it, and I can conceive automatic actions to be determined by guiding sensations, although the mind may not be "conscious" of them.

III. THE THEORY OF REFLEX-ACTION.—The muscular movements have from time immemorial been grouped under two heads: the voluntary, and involuntary. We shall have to consider the nature of this distinction presently, but must now inquire into that principle of Reflex-Action which, I conceive, underlies all the nervous phenomena, but which Marshall Hall conceived was characteristic of only a particular class. There were, he thought, four distinct classes: the *voluntary*, dependent on the brain; the *involuntary*, dependent on the irritability of the muscular fibre; the *respiratory*, wherein "the motive influence passes in a *direct line* from one point of the nervous system to certain muscles;" and the *reflex*, dependent on the "true spinal system" of *incident-excitor* nerves, and of *reflex-motor* nerves. These last-named actions are produced when an *impression* on the sensitive surface is conveyed, by an excitor-nerve, to the spinal chord, and is there *reflected* back on the muscles by a corresponding motor-nerve. In this process *no* sensation whatever occurs. The action is purely reflex, purely *excito-motor*—like the action of an ordinary mechanism.*

---

* MARSHALL HALL in *Philos. Trans.*, 1833. *Lectures on the Nervous System and its Diseases,* 1836. *New Memoir on the Nervous System,* 1843.

Müller, who shares with Marshall Hall the glory of having established this classification, thinks that although the absence of sensation is a characteristic of the reflex-actions, these actions may be, and are at times, accompanied by sensation. "The view I take of the matter is the following: Irritation of sensitive fibres of a spinal nerve excites primarily a centripetal action of the nervous principle, conveying the impression to the spinal chord; if the centripetal action can then be continued to the *sensorium commune*, a true sensation is the result; if, on account of division of the chord, it cannot be communicated to the sensorium, it still exerts its whole influence upon the chord; in both cases a reflex motor-action may be the result." *

This is, in brief, the Reflex Theory, which Marshall Hall and Müller have succeeded in making an European doctrine. There has been much discussion, often exploding on both sides into anger and injustice, respecting Marshall Hall's claims to originality. He has been accused of plagiarism from Prochaska; but I think that whoever attentively traces the history of the doctrine, will find that although Prochaska undoubtedly described the reflex-actions, and called them reflex, long prior to Marshall Hall—and, as it seems to me, conceived the whole phenomenon with far greater accuracy than was ever attained either by Marshall Hall or by Müller—yet it is very probable that Marshall Hall was unacquainted with his predecessor, who had long been dead, and whose theory had excited no attention; and it is certain that the theory propounded by Marshall Hall was *not* that propounded by Prochaska, but differed in essential points from it. This, however, is what neither Hall nor his adversaries seem to have known. So entirely had Prochaska's theory escaped attention, that even Müller, in announcing Hall's theory, and giving him the credit of priority over his own nearly contemporaneous discovery, never once alludes to Prochaska; and Arnold, in the historical sketch in which he enumerates those who before Hall had knowledge of the facts of reflex-action, is evidently unaware of Prochaska's claims.* When, therefore, Hall and Müller took up this subject, it was new to the scientific world; they succeeded in making the world accept their theory.

---

* Müller: *Physiology*, i. 721.

The one fundamental point in this theory which seems to me wholly inadmissible, is the supposition that the reflex-mechanism is independent of Sensibility—that reflex-actions take place *without* sensation. And, unless I have wholly misunderstood Prochaska's general views, this is the point which he also would have considered erroneous. The reader may be glad to have Prochaska's exposition of the reflex-actions. We must premise that he always means to indicate the whole of the Spinal Chord, Medulla Oblongata, Optic Thalami, and the *crura Cerebelli et Cerebri*, when he speaks of the *sensorium commune:* "The reflexion of sensorial into motor impressions which takes place in the sensorium commune, is not performed according to mere physical laws, where the angle of reflexion is equal to the angle of incidence, and where the reaction is equal to the action; but that reflexion follows according to certain laws, writ, as it were, by Nature on the medullary pulp of the sensorium, which laws we are able to know from their effects only, and in nowise to find out by our reason. The general law, however, by which the sensorium commune reflects sensorial into motor impressions, is the preservation of the individual; so that certain motor impressions follow certain external impressions calculated to injure our body, and give rise to movements having this object, namely, that the annoying cause be averted and removed from our body; and *vice versâ* internal, or motor impressions follow external or sensorial impressions beneficial to us, giving rise to motions tending to the end that the agreeable condition shall be maintained.

---

* J. W. ARNOLD: *Die Lehre von der Reflex Function*, 1842.

"Very many instances which might be adduced, undoubtedly prove this general law of the reflexions of the sensorium commune, of which it may be sufficient to mention a few. Irritation being made on the internal membrane of the nostrils excites sneezing, because the impression made on the olfactory nerves by the irritation is conducted along them to the sensorium commune, there by a definite law is reflected upon motor nerves going to muscles employed in respiration, and through these produces a strong expiration through the nostrils, whereby the air passing with force the cause of irritation, is removed and rejected. In like manner it happens that when irritation is caused in the windpipe by the descent of a particle of food or a drop of fluid, the irritation excited is conducted to the sensorium commune, and there reflected on the nerves devoted to the movement of respiration, so that a violent cough is excited; a most suitable means for expelling the cause of the irritation, which does not cease till the irritant be ejected. If a friend brings his finger near to our eye, although we may be persuaded that no injury is about to be done to us, nevertheless the impression carried along the optic nerve to the sensorium commune, is there so reflected upon the nerves devoted to the motion of the eyelids, that the eyelids are involuntarily closed, and prevent the offensive contact of the finger with the eye. These, and innumerable other examples which might be brought forward, manifestly show how much the reflexion of sensorial impressions into motorial, effected through the sensorium commune, has reference to maintaining the conservation of the body.

"Since the *principal function* of the sensorium commune thus consists in the reflexion of sensorial impressions into motor, it is to be noted that this reflexion may take place either with consciousness or without consciousness."

We must interrupt the quotation here, to remark, that when Prochaska says reflexes may take place with or without consciousness,—or, to use his own phrase, *vel animâ insciâ, vel verò animâ consciâ*—he means "with, or without, *Thought*-consciousness," and not, as so often misinterpreted, "with, or without, *sensation.*" This distinction entirely accords with the view I hold, but is different from that generally held.

"The movements of the heart, stomach, and intestines," he continues, "are certainly in nowise dependent on the consciousness of the soul, for whilst no muscular movement can be excited unless a stimulus applied to the sensorial nerves passes by a peculiar reflexion to the motor nerves, and excites contractions of the muscle, it is at the same time certain that the reflexion of the impressions suitable for exciting those movements, if it takes place in the sensorium commune, is affected without consciousness [Thought-consciousness]. But it is a question whether these impressions, in order that they may be reflected, do really travel so far as the sensorium commune, or, without taking this long circuit, are reflected nearer in the ganglia from whence these parts derive many nerves? This matter is to be considered afterwards. But that reflexions of sensorial impressions into motor are effected in the sensorium commune itself, while the mind is altogether unconscious, is shown by certain acts remaining in apoplectics deprived entirely of consciousness; for they have a very strong pulse, breathe strongly, and also raise the hand, and very often apply it unconsciously to the part affected. The sensorium commune also acts independently of consciousness in producing the convulsive movements of epileptics, and also those which are sometimes observed in persons buried in profound sleep, namely, the retractions of pricked or irritated limbs, to say nothing of the motion of the heart and the respiratory acts. To this category also belong all those motions which remain some time in the body of a decapitated man, or other animal, and are excited when the trunk, and particularly the medulla spinalis, are irritated, which motions certainly take place without consciousness, and are regulated by the remaining portion of the sensorium commune existing in the medulla spinalis."*

It is clear, from this passage, that Prochaska, in explaining the actions which take place "without consciousness," did not mean that they took place without *sensation;* he meant that they were neither determined by, nor followed by, *thought.* He uses the word Consciousness as equivalent to Thinking; and this is a very general use of the term, nowadays, as we find in writers who declare that we cannot have sensations unless we *know* that we have them. In strict physiological language, this is tantamount to saying that we cannot have spinal Sensibility unless we have at the same time cerebral Sensibility—that the spinal chord cannot act independently of the Brain; whereas we know that it *can,* and does, act independently of the Brain; and it is to distinguish such actions that the term "reflex" has been invented. To suppose, as is commonly supposed, that the cerebrum *endows* the spinal chord with sensibility—that an impression only becomes a sensation when it reaches the Brain—is to suppose what the whole course of modern investigation tends to refute, namely, that the spinal chord is simply a conductor, and not itself a centre; and it is also to suppose that the Brain is *essentially different* from the Chord. Prochaska seems to have understood that the sensorium must first be called into activity before a motion can be produced; the sensations excited in this sensorium are quite independent of any thought which they may, or may not, excite in the Brain.

---

* UNZER and PROCHASKA *On the Nervous System.* Translated by Dr LAYCOCK, p. 430-2. The original may be seen in PROCHASKA: *Opera Minora,* pars ii., cap. 2.

It is here that Marshall Hall diverges from Prochaska, and, as I think, it is here his error begins. Yet, in common with his antagonists, he imagined Prochaska held reflex-action to be wholly divorced from sensation. His distinguished ally, Mr Grainger, whose work* is far more philosophical than anything Marshall Hall ever wrote, also insists on this absence of sensation as the distinguishing peculiarity of reflex-action.

Mr Grainger seems fully impressed with the philosophical canon to which we have constantly referred, that similarity of structure necessarily implies similarity of properties; but instead of concluding that the brain and spinal chord, being similar in structure, must have sensibility in common, he assumes a diversity of structure to correspond with an assumed diversity of function. The Reflex-Theory, he truly remarks, wants an anatomical basis: "unless a set of nerves distinct from those of sensation and volition be capable of demonstration, it is impossible to establish the correctness of what, without such evidence, is merely an hypothesis." Accordingly he set himself the delicate task of demonstrating this distinct class of nerves. Sharing the prejudices of that day against the use of the microscope, and refusing to use a lens for fear of deception, his task became impossible. In the twenty years which have since elapsed, men have learnt to use the microscope with more security, and in the last ten years they have learnt how to make preparations, which, if they have not taught us the whole structure of the spinal chord, must in the course of time lead to accurate knowledge. Anatomists have long rejected the supposed system of excito-motory fibres.

---

* GRAINGER: *Observations on the Structure and Functions of the Spinal Chord*, 1837.

[While these sheets are passing through the press, I find Schroeder van der Kolk, whose statements, as I have before hinted, are to be received with extreme caution, describes a system of reflex-fibres, with a confidence rather surprising when we scrutinise his evidence. He maintains that each posterior root, on entering the white columns of the chord, divides into two bundles; the one bundle, containing purely sensory-fibres, runs straight up to the brain; the other bundle, containing purely reflex-fibres, runs into the grey matter of the chord, where the fibres terminate in the ganglionic cells. It is, he says, because the posterior root contains these two kinds of fibres, that it is larger than the corresponding anterior root. On this I have to remark, first, that the facts on which this hypothesis is based are excessively questionable; secondly, that the supposed confirmation drawn from the larger size of the posterior root is worthless, inasmuch as the fact is only true with regard to some animals—in the frog, for instance, the posterior root is *smaller* than the anterior; and finally, that if the anatomical facts were as he states them, his conclusion would not be less rash and hypothetical. For let us grant that one fibre enters the grey substance of the chord, and there terminates in a ganglionic cell, whereas another fibre runs up to the Medulla Oblongata, and there terminates in a ganglionic cell (which is the real statement of his position, stripped of equivocal language), what difference, of an essential kind, is here established, such as could justify the distinction of sensory and reflex fibres?]

The anatomical basis for the Reflex-Theory is still to seek. Indeed, were a distinct set of nerves to be discovered, they must be in connection with nerve-centres; and inasmuch as the reaction of a nerve-centre is Sensibility—inasmuch as *all* nerve-centres must have the *same property,* in spite of any difference in their *functions,*—it is clear that the reflex-fibres would excite Sensibility, if they excited the centre.*

---

* A friendly critic objects: "How, in this case, do you explain one nerve-centre appreciating only sound, and another light, and another flavour? Rigorously interpreted, your position would involve that all sensations are the same." By no means; my position is, that all sensations are forms of Sensibility, and their differences result from variations in the nature or degree of the stimulus. The same skin-nerve yields to different stimuli the very different sensations of touch, warmth, cold, tickling, pressure, pain, &c. The subject will be treated more fully in the next chapter.

Let us, however, see what physiological observations can be adduced in favour of the reflex-theory.

"We have lastly to inquire," says Müller, "how far true sensation is engaged in the production of reflex motions. Volkmann inclines to the opinion of Whytt, that the motions consequent on impressions are the result of sensations conveyed to the sensorium, giving rise to spontaneous reactions. That this is in many instances the case, appears to me indubitable, particularly with those reflex phenomena which occur in an unimpaired state of the brain and spinal chord. Thus I regard, for example, the closure of the eyes under the stimulus of a strong light, and the action of the respiratory muscles induced by irritation of the mucous membranes of the respiratory organs, or intestinal canal." Note this admission, and then consider the counter-argument he brings forward: "But when we remember that, if we divide the trunk of a salamander into several portions, each part, if it contain a fragment of the spinal chord, will still evidence the reflex motions, we can scarcely maintain the applicability of this view to all cases." *

Here the question naturally rises: Why can we *not* suppose the segments of the chord to be sensitive, and its actions to be determined by sensations, as well when the brain is absent, as when the brain is present? The only obstacle is in the prejudice respecting the brain being the sole seat of sensation (compare page 84). But look at the facts. We etherise a frog,

---

* MÜLLER, 720.

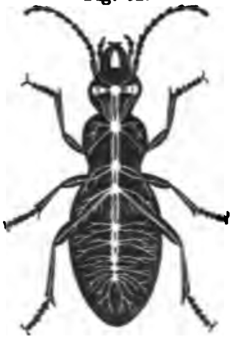
and cut off its head; after which we divide its spinal chord, in the region of the back. We have then two segments of a headless animal. When the effect of the ether has disappeared, we pinch, or prick, the fore-leg, and it is at once withdrawn; we do the same to the hind-leg, and it is withdrawn; we irritate the posteriors, and the hind-legs vehemently push the instrument away. Another frog, also decapitated, hops away when thus irritated; but this one, with the divided chord, cannot hop. All these actions of the decapitated animal are, we are told, purely reflex, totally without sensation; "because sensation is only possible when the brain is present." In the uninjured frog these actions result from sensations, because the impressions on the skin are *conducted* to the brain. All this seems clear enough until we extend our observations. Let us decapitate a frog, and see whether the head, separated from the body, will not exhibit the same signs of Sensibility which are exhibited by the body separated from the head. We touch the eye, and the eyelid closes; we throw a strong light on the eye, and the pupil contracts and the lid closes; we touch the tongue, or the inside of the mouth, and the larynx makes a motion of swallowing. All these are precisely similar to what occurs in the uninjured frog, and are clearly allied to the actions of the headless trunk; yet Physiologists would have us believe that these actions, both of head and trunk, which indicate sensation when the animal is entire, do *not* indicate sensation when the head is separated from the body; they would have us believe that the motions of the eyelid, pupil, and larynx, in the separated head, are purely reflex, wholly destitute of sensation. I confess this seems to me very arbitrary. The headless trunk is first said to be incapable of sensation, because the brain cannot be reached; and then the head itself is said to be incapable of sensation, although impressions on it *do* reach the brain. Does, then, sensation depend on the union of the spinal chord with the brain?

But however arbitrary it may seem to deny sensation to the separated head, and to consider its actions as purely reflex, there is absolute necessity for such a position, if the Reflex-Theory is to be upheld; because, if we grant sensation to the *separated head*, when we see it responding in the usual manner to stimuli, we can have no grounds for denying sensation to the *separated body*, when we see it responding in the usual manner to stimuli.

In the numerous experiments I have made with a view to a right understanding of this subject, those on insects have furnished decisive examples. When a wasp is cut in two, both halves live, and manifest Sensibility (if we assume that a wasp *ever* manifests it, and is not a mere machine) during three or four days. If you irritate one half—the head—it will *bite;* if you irritate the other half—the tail—it will *sting*. Take a beetle, such as the large water-beetle, *Dytiscus marginalis*, and remove its head. Here is one, which was decapitated eleven hours ago. At first the head twittered its antennæ, and moved its mandibles, just as it had done before decapitation: this was evidence of *spontaneous* action. It also frequently moved the larynx. Sometimes it spun round on the table by means of its long antennæ. When quiet, if I touched it, these motions were resumed. It seized any object with its mandibles, and bit fiercely. About ten hours ago all spontaneous action ceased; but any irritation produced the reactions of sensibility. All this while the *headless trunk* was swimming rapidly, and, if irritated, behaved precisely as the trunk of an *uninjured* insect behaves.

Once more, I ask, How can we refuse this evidence of sensibility, unless we boldly deny that the insect has sensibility at all? If we accord sensibility to the entire beetle, we must accord it to the separated head; and if to the separated head, then, on the same grounds, to the separated body. Cast a glance at the nervous system of the beetle, and you observe that the ganglia in the head are similar to those in the trunk, and are only united with them by two threads of nerve-fibres. Is it rational to suppose that the mere fact of union of such threads can endow actions with sensation, when we know that the actions themselves are the same whether the union exist or not; and the only difference created by a division is, that the actions of the fore-part are no

Fig. 61.



Nervous System of Garden Beetle.—(After Milne-Edwards.)

longer consentaneous with the actions of the hind-part? The nervous communication enables the two parts of the insect to *act together*; it does not, and it cannot, give sensation where no Sensibility existed. According to the view I take, Sensibility is the property of every ganglion, just as Contractility is the property of every muscle. And carrying this idea with us, we shall find experimental evidence in abundance; for, suppose we cut off one segment of an animal's body, the loss of this segment will not entail a loss of sensibility in the *rest* of the body; a dog, or a lizard, may lose its tail without losing its susceptibility to impressions. Let us now remove another segment. The insect continues to fly, walk, bite, &c., as before. From this sensitive, though truncated, insect, we continue to remove segment after segment, till we reach the last segment, or head. In the course of these operations we find the insect more and more crippled, but at no point ceasing to manifest sensibility: the head gives unequivocal signs, just as the whole body, *minus* one segment, gave unequivocal signs. There is but this alternative: either we must deny that the insect at any time has sensibility; or we must assert that only in the ganglia of the head does sensibility reside. The first of these positions would render Physiology silent altogether, and indeed would render it impossible for us to draw any conclusions respecting man; since it is only by the similarity of their actions with our own that we can conclude men to have feelings similar to our own: each man can only have direct knowledge of his *own* consciousness. The second position of the alternative, which makes Sensibility the exclusive property of the cephalic ganglia, is an assumption contradicted on the one hand by the *anatomical identity* of all ganglia, and on the other hand by the *physiological identity* of the actions of head and trunk, when separated.

That I am not doing the upholders of the Reflex-Theory any injustice in asserting that they call the actions of the separated head purely reflex, without sensation, will be apparent from the following passage from Marshall Hall's Memoir:—He decapitated a turtle. "The head being placed upon the table for observation, it was first remarked that the mouth opened and shut, and that the submaxillary integuments descended and ascended alternately from time to time, replacing the acts of respiration. I now touched the eye or eyelid with a probe. It was immediately closed: the other eye closed simultaneously. I then touched the nostril with the probe. The mouth was immediately opened widely, and the submaxillary membranes descended. This effect was especially induced on touching the nasal fringes situated just within the anterior part of the maxilla. Having made these observations, I gently withdrew the medulla and brain. All the phenomena ceased from that moment. The next observations were made upon the other parts of the animal. The limbs, the tail, were stimulated by a pointed instrument or a lighted taper. They were immediately moved with rapidity. The sphincter was perfectly circular and closed: it was contracted still more forcibly on the application of a stimulus. On withdrawing the spinal marrow gently out of its canal all these phenomena ceased." The conclusions drawn from these observations are not a little surprising to any one who has a sense of logical proof. "This experiment," he continues, "affords evidence of many important facts in physiology. It proves that the presence of the medulla oblongata and spinalis is necessary to the contractile function of the eyelids, the submaxillary textures, the larynx, the sphincters, the limbs and the tail, on the application of stimuli to the cutaneous surfaces or mucus membranes [which no one doubted]. It proves (!) the reflex character of this property of the medulla oblongata and spinalis, and the dependence of these motions upon the reflex function. It proves that the tone of the limbs, and the contractile property of the sphincter, depend upon the same reflex function of the medulla spinalis—effects not hitherto suspected by physiologists."[*]

Now what is the proof here afforded? The facts are, that the limbs and tail contract, when stimulated, although the head be removed from the body. This is assumed to be a proof that such contractions take place without sensation, because the Brain is assumed to be the sole seat of sensation. "In decapitated animals the *very organ* of sensation and volition, and the other mental faculties, is removed."[†] But unhappily for this argument, the facts cited by him show, that when the "very organ" itself is present—in the severed head—precisely similar phenomena are observed. If the brain is the very organ of sensation, it will manifest sensibility as long as any vitality remains, whether joined to the body or severed from it: just as muscles manifest contractility, and the heart beats, when separated from the body: it is but a feeble manifestation, I admit, but as long as there is *any* manifestation, it will be of the *organic function peculiar to the organ.* In most animals, the loss of blood, and the shock, destroy at once all manifestation of sensibility in the severed head;* but in reptiles, batrachians, and insects, the property still endures for a short period.

---

[*] MARSHALL HALL: *Philos. Trans.* 1833, p. 644.

[†] MARSHALL HALL: *New Memoir on the Nervous System*, p. 32.

Does not the reader perceive that if the head, containing the "organ of sensation," after being severed from the body, manifests any of those signs of sensibility which it manifests when joined to the body, we can have no right whatever to say these *identical effects* are due to *different causes?* Does he not further see that if the phenomena observed in the *body separated from the head,* are precisely similar to those observed in the *uninjured animal,* and to those observed in the *severed head,* these identical phenomena cannot be due to different causes? Deny that the separated body manifests sensibility, and you must deny that the separated head manifests it; but you cannot stop here, you must also deny that the uninjured animal manifests it.

---

* Were it not so, the sufferings of a man beheaded would be frightful. But we know, from experiments on mammals, that the loss of blood causes death before the shock passes away.

That is the interpretation I put upon the facts. The one suggested by Marshall Hall rests on the double assumption that the Brain is the exclusive seat of Sensibility (an assumption of the very point in question), and that unless the Brain be in vital connection with the rest of the body, all Sensibility is impossible. Of *proof* there is not a shadow. Indeed, I cannot help an expression of surprise at the weakness of the evidence on which this celebrated Reflex-Theory is based. Except the striking facts noted of human beings, with injured spines, who were insensible to prickings and burnings—facts which we shall hereafter see do not in any way contradict the views put forth in these pages, and do not, when properly considered, give the Reflex-Theory any countenance—there is not a single fact, or experiment, which can make us pause. As to Marshall Hall's few experiments, the facts are so inaccurately stated by him, or, when accurate, are so illogically interpreted, that a feeling of the ludicrous is irresistible when we hear him declare: "*Nothing can affect my view* of the subject, which is, as I have stated, the *simple expression of the facts.*"

Let us give Marshall Hall due honour for his ingenuity and his steady pertinacity against all opposition, but let us also rigidly question the claims even of the most honoured. I do not think that accuracy of observation, or of reasoning, were his strong points; and in the course of the following pages the reader will see grounds for this judgment. Meanwhile two important misstatements call for notice. One of the cardinal "facts" in Hall's theory is, that decapitated animals never move unless *excited* to motion by an *external* stimulus. This fact I positively deny. Had he kept frogs and tritons for days after decapitation, or weeks after division of the spinal chord, he would have observed frequent refutations of his hasty conclusion. Had he even remembered Redi's account of the tortoise, which, after the whole cranial cavity had been completely emptied, *walked* about as usual, and lived for five months,* he would never have ventured on such a statement.

Some time ago I removed the brain from a frog, and left it on a plate to recover from the effects of ether. The next morning the servant came to me, with suppressed alarm, assuring me my frog would escape. "No, there's no danger. It can't escape, its head is off." "But I assure you, sir, it's quite lively; I thought it would jump off the table." On going upstairs, I found the animal in the middle of the room. Such things are of frequent occurrence. Dr Inman, of Liverpool, writes me word that he has witnessed it on several occasions. He completely emptied the cranial cavity of a frog, yet found the animal quite vivacious: "There is no lack of spontaneous movement, and the reflex-actions are distinct enough in the eye, eyelid, and other places." Indeed, no one has investigated this subject without seeing abundant evidence of spontaneous movements after removal of the brain. These movements are, it is true, less evident when the Medulla Oblongata has also been removed; but that is simply because lung-respiration depending on the Medulla, and vital energy on respiration, the removal of the Medulla is too serious an interference with the relations of the organism to permit more than feeble evidences of spontaneity. Nevertheless, when the spinal chord is divided, the *direct* influence of the Medulla is removed from the nervous centres behind the section, but its *indirect* influence, in keeping up an active nutrition of those centres, enables the parts behind the section to move spontaneously. A Triton, or Lizard, with a divided chord, will be seen to move its tail and hind-legs spontaneously; *i. e.*, without any *external* stimulus being applied.

---

* REDI: *Observ. circa Animal. Vivent.*, p. 209.

When I use the phrase external stimulus, it is in the sense implied by Marshall Hall and Mr Grainger, meaning some irritation from the contact of a hard substance, an acid, or the flame of a taper. But as a change in the temperature of the room, a current of cold air, or the effect of exposure on the wound, may equally well range under the category of external stimuli, I think it important to show that a decapitated frog will move spontaneously when there is no external stimulus possible. The frog is first etherised; and then, its brain having been carefully removed, the whole of its skin is stripped off. The effect of the skinning is to render it totally insensible to any external impressions: you may pinch, prick, tear, burn the flesh, or cut off the limbs bit by bit, without producing the slightest sign of sensation. Yet this frog will hop spontaneously; and if you draw its hind-legs from under it, and leave them outstretched, it will draw them up again in a few minutes; thus proving that motions may take place in a brainless animal when all external impressions are rendered powerless. Moreover, I found that a triton, *divided in two,* had spontaneous movement in *both* halves: I placed the tail-half under a glass, and kept the air within moist, to prevent evaporation from the skin, yet even here this fragment of an animal occasionally moved.

So much for the cardinal fact in the Reflex-Theory—a fact which is to prove that the motions of a decapitated animal are *excited,* but not *felt.* I conceive that *all* actions, in all animals, are excited, and all are felt: an unexcited motion is inconceivable; but some motions are excited by *external,* and others by *internal* stimuli.

The second point to be noticed here, illustrates Hall's somewhat loose manner of conducting experiments which require rigid caution; and the equally loose manner in which he drew conclusions. "I divided," he says, "the spinal marrow of a very lively snake between the second and third vertebræ. The movements of the animal were immediately before extremely vigorous and unintermitted. From the moment of the division of the spinal marrow, it lay perfectly tranquil and motionless, with the exception of occasional gaspings and slight movements of the head.

It became quite obvious that this state of quiescence would continue indefinitely, were the animal secured from all external impressions." So far from this being "obvious," it is directly counter to fact. The mere division of the spinal chord gives a "shock," which for a time keeps the animal motionless, or convulsive, but never prevents subsequent spontaneity of movement. But let us continue his account: "Being now stimulated, the body began to move with great activity, and continued to do so for a considerable time, each change of position or situation bringing some fresh part of the surface of the animal into contact with the table or other objects, and renewing the application of the stimulus. At length the animal became again quiescent; and being carefully protected from all external impressions, it moved no more, but died in the precise position and form which it had assumed."

A closer reasoner would have seen that the snake's movements — if originated by a *sensation* derived from an irritation — would subside with the subsidence of the sensation; but they could never subside —while vitality remained—if the mere *excitation*, caused by contact of the table, originated each movement; since this excitation must be *renewed* at each movement of the snake, and thus one movement would necessarily bring on the next. But Marshall Hall set logic at defiance. On this very page he declares, that when a decapitated animal has relapsed into the repose from which it will never rouse itself, unless stimulated externally, "the slightest touch with a hard substance, the slightest stimulus, will renew the movements;" and he thinks that these movements *cannot* depend upon sensation, because the position into which the animal falls may be one which would be attended with extreme pain if sensibility remained—*e. g.* when the animal remains suspended over the acute edges of the table. Had he stopped here, one might have attached some weight to the argument; but he proceeds—"the infliction of punctures and the application of a lighted taper did not prevent the animal, still possessed (?) of active powers of motion, from passing into a state of complete and permanent quiescence."

As the "observation" is one on which Hall lays great stress, the reader is requested to notice the remarkable confusion in the statement. First we are told that the slightest touch with a hard substance suffices to renew the movements; and then we are told that the proof of these movements not being due to sensation, is that punctures and burnings produced *no* such renewal of movements—as if punctures were effected without "the slightest touch of a hard substance!" The truth of the case, so ill observed by him, is this, that the snake, when thoroughly exhausted, did not move, if punctured or burned; but before that exhaustion had taken place, punctures and burnings could have made it writhe, and any other stimulus would have made it move. On the whole, we cannot but agree with Arnold, when he says that Hall one moment relies on the fact that decapitated animals move, as a proof that movements may take place without sensation; and the next moment proves the absence of sensation by the fact that the animals do not move.*

These examples will suffice to show how little Marshall Hall's experiments support his theory. Let us glance at another example which is adduced in favour of the theory: "Mr Grainger found," says Dr Carpenter, "that he could remove the entire hind-leg of a salamander with the scissors without the creature moving, or giving any expression of suffering, if the Spinal Chord had been first divided; yet that by irritation of the foot, especially by heat, in an animal similarly circumstanced, violent convulsive actions were excited in the legs and tail. This fact is important, not only as showing the comparatively powerful effect of impressions on the cutaneous surface, but also as proving how little relation the amount of reflex-action has to the intensity of sensation."

On this we must remark that the fact is altogether misstated, owing to theoretic preconceptions. When Mr Grainger states that the creature did not move, or give any expression of suffering, he means, I presume, that the fore-part of the creature remained quiet; which is no more than would be expected, since the division of the spinal chord had divided the creature into two distinct parts—as regards sensibility. The fore-part was quiet, because the fore-part was not touched. But I venture to say, the hind-part was not quiet; *that* always writhes when the leg is cut off. And although the leg may be cut off from this hind-

---

* ARNOLD: *Die Lehre von der Reflex Function*, p. 54.

part without producing any considerable writhings, such as are produced by heat or other irritation, yet this by no means warrants the conclusion drawn by Dr Carpenter; and for these reasons: the sensibility of a Salamander to external stimuli is almost exclusively cutaneous; stripped of its skin, *no* amount of cutting, or pricking, produces the slightest sensation: when therefore the leg is cut off, if the scissors be very sharp, and the movement rapid, the animal scarcely responds to the stimulus; but if the scissors be not very sharp, and the cut be made slowly, the animal—or a segment of the animal—writhes energetically. An irritation of the skin, by heat or other stimulus, will, of course, produce greater writhings than cutting off a leg: but this will be the case with the uninjured animal, no less than with the animal whose chord has been divided. Thus we see that the fact in no way supports the hypothesis of reflex-actions being produced without sensation.

Indeed, I know of no facts whatever which justify the hypothesis, but abundant facts irreconcilable with it. These latter facts we shall have to consider presently, in treating of the sensational function of the spinal chord. If we are to limit sensation to the brain, it is obvious that actions which take place without a brain, must be actions without sensation. But this is the point which has to be proved, not assumed; and it cannot be proved by such facts or arguments as have hitherto been advanced. Actions may be determined by cerebral, or by spinal, sensibility; to deny sensibility to the spinal chord, because its sensations may in many respects be unlike those of the brain, is as unphilosophical as to deny sensibility to the cerebrum, because its sensations are unlike those of the optic or auditory centres: *and this has actually been maintained.* Various writers have denied consciousness to the cerebral hemispheres. Nor, until we adopt language having scientific precision, is there any ground for objecting to such a conclusion; since it is perfectly clear that the same principle of reflex-action takes place in the cerebrum, as that which we have noted in the spinal chord.

IV. REFLEX-ACTION IN THE BRAIN.—It was suspected by Prochaska, and has since become quite ascertained, that reflex-actions would take place from the Sympathetic Ganglia, without the participation of the Spinal Chord. The proof is simple. The whole spinal chord and brain may be destroyed, yet the heart will continue to beat, and the intestines to move.

But a more important step was to be taken: the Brain was to be included among reflex centres, and some at least of its actions reduced under the general law of reflexion. This step was taken by Dr Laycock in 1840. In a striking paper* read by him at the British Association in 1844, he brought together the evidence on which his view was founded. The idea has been adopted and illustrated in the writings of Dr Carpenter, who now calls the action "unconscious cerebration." If the modern-reflex theory be adopted, this extension of it to the brain is inevitable. But it seems to me that unconscious cerebration is nothing more than this: we can have a sensation without thinking of that sensation; and in like manner we can think without thinking that we think—a train of ideas may go on without exciting a particular direction of the consciousness.

---

* Printed in the *British and Foreign Medical Review,* Jan. 1845.

"I was led to this opinion," Dr Laycock says in announcing his view, "by the general principle that the ganglia within the cranium being a continuation of the spinal chord, must necessarily be regulated as to their reaction on external agencies by laws identical with those governing the spinal ganglia and their analogues in the lower animals." If, therefore, the spinal chord is a centre of reflexion, the brain must also be one. It is a matter of regret that Dr Laycock did not extend this principle, and declare that whatever was true of the *properties* of the cranial centres, must also be true of the spinal centres; if the brain have Sensibility, the spinal chord must also have it.

Dr Laycock refers to the curious phenomena of Hydrophobia in proof that reflex-actions may be excited by the optic nerves, or by a mere idea of water. When a mirror was presented to a patient, the reflection of the light acting on his retina, in the manner of a reflection from the surface of water, produced a convulsive sobbing, as in the attempt to swallow water, and the patient turned aside his head with expressions of terror. Money was given him to induce him to look a second time, but before he had looked in it a minute, the same effect was produced.

The *idea* of water excited similar convulsions. No sooner was it suggested that the patient should swallow a little water than he seemed frightened, and began to cry out. By kindly encouragements, he was brought to express his willingness to drink, but the *sound* of the water, as it was poured out again, brought on convulsions. In another case, "on our proposing to him to drink, he started up, and recovered his breath by a deep convulsive inspiration. On being urged to try, he took a cup of water in one hand, and a spoon in the other. With an expression of terror, yet with great resolution, he filled the spoon, and proceeded to carry it to his lips; but before it reached his mouth, his courage forsook him, and he was forced to desist. He repeatedly renewed the attempt, but with no more success. His arm became rigid and immovable whenever he tried to raise it to his mouth, and he struggled in vain against this spasmodic resistance."

The facts of reflex cerebral action are important illustrations of the law of nervous phenomena, but they do not in any way establish the peculiar assumption of the Reflex-Theory, which is, that actions can take place without the excitations of Sensibility.

Dr Carpenter, adopting the opinions of Marshall Hall and Dr Laycock, thus classifies all reflex-actions: 1. Those determined by the Spinal Chord; they are "excito-motor." 2. Those determined by the Sensory Ganglia; they are "sensori-motor." 3. Those determined by the Cerebrum; they are "ideo-motor." In all these consciousness is said to be absent.

Here we must once again call attention to the ambiguity of ordinary language. Consciousness is a word which, although used as equivalent to sensation, is often, and more frequently, used as equivalent to thought. To have a sensation, and to be conscious, are held as convertible terms: there can be no sensation without consciousness. But there is frequent use of phrases implying that sensations *may* exist without consciousness, as when we say, "I heard the clock strike, but was unconscious of it; I wound up my watch, but did so unconsciously." This is the meaning which Dr Carpenter seems to attach to the word; and with that meaning, there is no impropriety in his speaking of "unconscious cerebration," which some have ridiculed, as if it were tantamount to "unthinking thinking." There is no doubt that we go through many mental processes without any of that reflex-feeling which is characterised by the phrase "being conscious." The train of ideas may never diverge from the direct path: a problem may be solved, and the mind will be so intent on the solution as to be wholly "unaware" of anything else. During reverie we are not only "unconscious" of the presence of external objects, but of our own state. The intellectual mechanism acts without interruption from sensation. When the word consciousness is restricted thus, we may properly say that there can be unconscious thinking, and unconscious sensation. It is only saying that some centres of our complex organism may act, without at the same time calling some other centres into play.

The perplexing ambiguities which necessarily arise when different writers use the words sensation and consciousness, without rigorously defining their terms, I have endeavoured to avoid, by uniformly employing the word Sensibility as a general term indicating the property of *all* nerve-centres; and the word Sensation as a general term including every reaction of Sensibility to a stimulus, external or internal. By so doing, all nervous phenomena are brought under the same law; as all muscular phenomena are brought together when Contractility is assigned to muscular fibre, and Contraction to the activity of that property. A muscle may be flexor, or extensor, or sphincter; its function may be to serve expression, respiration, locomotion, voluntary or involuntary movements; but in all cases the actions depend on Contractility. The muscular mechanism, like the nervous mechanism, is so related in its parts, that some of them cannot be called into action without also playing upon others; but there are parts which can act with a certain independence. The arm may be moved without any alteration of the muscles of expression, yet it cannot be moved without affecting the muscles of respiration. But whether the parts act separately, or in combination, the necessary condition of all muscular action is Contractility. In like manner, whether the various nerve-centres act separately or in combination, the necessary condition of all ganglionic action is Sensibility.

I have thought it desirable, on many grounds, to make Consciousness an equivalent term for Sensibility, and have grouped the various forms of Sensibility under the heads of Thought-consciousness, Sense-consciousness, and Systemic-consciousness. But this is a point which may be left to the pleasure of the reader. The thesis maintained in these pages does not require assent to such a classification; but it asserts, as a scientific proposition, that Sensibility is the property of all nerve-centres; and that no action can possibly take place in the organism which has not a sensation, *of some kind*, as a basis. The Reflex-Theory, which considers the organism as a mechanism, errs in supposing that this mechanism can dispense with Sensibility. I also hold that the organism is a mechanism—but a vital and *sensitive* mechanism. It was no discovery of Marshall Hall's that many actions could take place, and did take place, without our being "conscious" of them—meaning thereby, without our *attending to*, or *thinking* of them. Every one knew that. His discovery purported to be, that whereas *some* actions have a sensational origin, and can only be performed in virtue of a sensitive mechanism; there are *other* actions which have *no* such origin, but which, like the actions of a steam-engine, depend on the reflexion of impressions into motions, one cogged wheel moving another. In this he made the immense mistake of forgetting that in the *vital mechanism each cog is a sensation.* Unless his purely mechanical view be adopted, the Reflex-Theory is merely a new name; and the reader has already seen how little evidence there is in favour of such a view, how much against it. The evidence against it will become more and more coercive as we proceed. But it is necessary for a right understanding of the claims of the Spinal Chord to the title of a sensational and *volitional* centre, that we should examine with some attention what constitutes—in a physiological sense—a volition.

V. VOLUNTARY AND INVOLUNTARY ACTIONS.—It seems an easy thing to distinguish a voluntary from an involuntary action; and yet this seemingly easy thing sorely perplexes the cunning of philosophy. It seems also an easy thing to distinguish between an animal and a plant; yet when we come to seek for the one distinctive characteristic which marks the animal world, and separates it decisively from the vegetable world, we are sorely puzzled. There is no difficulty in saying that a cow is an animal, and a cabbage is a plant: but when we descend to the simpler forms of animal life, we find them so nearly allied to plants that our classification is troubled. Still greater is our perplexity when the simpler actions are presented for analysis; positive as we may be that some actions have a volitional element, we are at a loss to mark out *what* that element is.

In popular language, those actions are called voluntary over which we can exercise control, either in the way of restraining, or of prompting them. I can move my arm, or keep it motionless, if I will to do so. But there are other actions which are beyond control; no effort of Will suffices to prompt, or to restrain them. The heart beats without my control. The eyelid winks, the wounded muscle quivers, the stomach digests, involuntarily. I can control the movement of my arm, unless a sharp pain forces me to withdraw it; and when I withdraw it under sudden pain, the action is said to be involuntary.

This is a rough classification, which suffices for our daily needs. We want a term to mark a certain group, and the term voluntary satisfies that want. But the severer exigencies of Science are not satisfied so easily. A rigorous examination shows that in most, if not in all, the so-called involuntary actions (as we shall see presently) this very volitional element of control may find a place. Although breathing is an involuntary act, it can be, and often is, restrained or accelerated by the will; but the controlling power soon comes to an end—we cannot voluntarily suspend our breathing for many seconds; the urgency of the sensation at last bears down the control. In like manner, we can partially, but not wholly, restrain the shrinking and trembling which accompany pain and terror. It has been said that these partial influences of control are due to the fact that the apparatus involves some of the voluntary muscles, and these are, of course, under the control of the will; but that inasmuch as the apparatus is not wholly constituted by voluntary muscles, it is not wholly under control. Yet this is only a restatement of the fact in different terms. The muscles are styled voluntary, because they are under control. Nevertheless, it is easy to prove that an apparatus of purely voluntary muscles will furnish an involuntary act—an act quite beyond all influence of the Will. The act of winking is an example. It is performed by voluntary muscles, and may be a purely voluntary act—as when we wish telegraphically to warn one of our hearers that we are jesting, Yet this act, which is as purely voluntary as any we perform, is habitually an involuntary act; the contact of the air with the eye causes a loss of temperature by evaporation, and the sensation caused by this dryness urgently insisting on being remedied, we wink. Not only is winking one of the typical examples of involuntary action, but we find that it occurs in spite of the most obstinate effort to restrain it: no resolution on our part *not* to wink will prevent our winking, after a certain time, or if a hand be passed rapidly before the eye.

This example shows that the partial control which the will exercises over what are called involuntary acts, does not depend on the nature of the muscles involved. The *same* action which is voluntary at one moment will be involuntary at another, according to the urgency, or intensity, of the stimulus. We laugh because we are tickled, or because some ludicrous image presents itself: both of these are involuntary actions, although both are capable, within certain limits, of control; but we may also laugh because we pretend to be tickled at the great man's joke—secretly felt to be a very feeble effort of humour. We cough because there is a tickling in the throat; and we also cough because we desire to drown the too buoyant platitudes of a remorseless orator. We yawn because we are weary, and we yawn because we determine to set others yawning. It seems clear, therefore, that the *volitional* element we are in search of, cannot lie *in the act* itself, but in something which *precedes or accompanies* the act. According to the popular opinion, an act is called voluntary if the mind has determined it by a conscious conception of the object to be attained; and if we were to say that volition is an action determined by a distinct idea, we should express the current opinion pretty accurately. Is that opinion tenable?

It is not tenable, because on the one hand actions may be determined by distinct ideas, and yet be "involuntary;" and because on the other hand actions may be voluntary, yet not determined by distinct ideas, but determined simply by sensations. Let a friend pass a finger rapidly before your eye, and although he has solemnly assured you that he will not touch you, and you have profound confidence in his word, yet no effort of Will prevents your winking. It is in vain you resolve to be firm—the eyelid drops as the finger approaches. This winking is, according to ordinary conceptions, an involuntary act, since it is performed in spite of the will; yet it is an act determined by an idea, the idea of *danger;* and the proof of this is seen when you approach a finger to the eye of an animal, or infant, in whom no such idea of danger is excited: it does not wink. Nor do you wink when you approach your own finger to your eye, because then the idea of danger is absent. We have here an action eminently *controllable,* and obviously determined by an *ideal stimulus,* having therefore the two cardinal characters of a voluntary act, yet being unmistakably involuntary. To reconcile such a contradiction we must suppose that the Will oscillated — one instant it resolved that winking should not take place, and the next instant resolved that it should. This explanation would, however, force the admission that the act of winking was not involuntary; after which, it would be puzzling to say *what* acts are involuntary. If the will can thus oscillate, and thus rescind its orders, why may it not in all the assumed cases of involuntary action be in a state of oscillation?

What is the process of control? Every action is a response to a sensitive stimulus. Muscles are moved by motor-nerves which issue from nerve-centres; these nerve-centres are excited by impressions carried there, either by sensory nerves going from a sensitive surface, or by impressions communicated from some other centre. A stimulus applied to the skin excites a *sensation,* which being reflected on a muscle excites a *contraction.* But instead of the sensation exciting a muscle, it may be reflected on some nerve-centre, and excite a reflex-feeling. This secondary, or reflex-sensation, may either play upon a muscle, or upon some other centre, and *this* will excite an action. Thus it is that the same external stimulus may issue in very different actions. We decapitate a frog, and half an hour after prick or pinch its leg: the frog hops, or suddenly draws up its leg. We now prick, or pinch, an uninjured frog in the same way, and we mostly (not always) observe that its leg is motionless; it does not hop away, it only lowers its head, and perhaps closes its eyes; a second pinch makes it hop away. In the decapitated frog, the action was reflex; the stimulus transmitted from the skin to the spinal chord was directly answered by a contraction of the leg. In the uninjured frog, the stimulus was also transmitted to the Spinal Chord; but from thence it ran upwards to the brain, exciting a reflex-feeling of alarm; but though alarmed, the animal was not forced into any definite course of action to secure escape; and while thus hesitating, a second prick came, and the urgency of the sensation then caused it to hop away. This hopping was reflex, but it was indirectly so: it was prompted by the reflex-feeling, which in turn had been excited by the original sensation. In like manner, if a dog's tail be pinched by a stranger, the dog cries out, and turns suddenly round to bite his tormentor. If the tormentor happen to be the dog's master, or friend, the dog will cry out, start away, or perhaps even turn round to bite—but he will not bite; should he get so far as to seize the hand with his teeth, he checks himself in time. This control is often touchingly seen in removing a thorn from a dog's foot: the pain causes a reflex-action, which brings the dog's head down upon the operator's hand; but instead of biting, the grateful animal licks that hand.

These are cases of control. They are possible only because reflex-feelings are excited; one sensation being rapidly followed by another, so that before one action, directly reflex, can occur, *another action* is set going, which interferes with it, controls it. An examination of the Nervous System discloses a number of centres, all capable of independent action, yet all connected with each other, and thus brought into some dependence on each other; it is through this dependence that control becomes possible. A sensation, instead of issuing in the action which usually follows it, sometimes issues in another sensation; this in turn may issue in a third sensation, instead of in an action; just as, when a row of billiard-balls is struck, the impetus is transmitted from one ball to the other, the *last* in the row flying off, and all the others remaining in their original position. At some point or other, could we follow its course, we should observe that the original sensation issued in an action, although, because the final stimulus to this said action is a reflex-*feeling*, the action itself is very unlike what it would have been if *directly* reflex. Tickle the face of a sleeping man, and by a reflex-action his hand is raised to rub the spot; tickle the face of that man when awake, and instead of this reflex-action, there will be one of vocal remonstrance, or perhaps one guiding a pillow in its descent upon your head.

Inasmuch as all actions whatever are the products of stimulated nerve-centres, it is obvious that all actions are reflex—reflected from those centres. It matters not whether I wink because a sensation of dryness, or because an idea of danger, causes the eyelid to close: the act is equally reflex. The nerve-centre which supplies the eyelid with its nerve has been stimulated; the stimuli may be various, the act is uniform. At one time the stimulus is a sensation of dryness, at another an idea of danger, at another the idea of communicating by means of a wink with some one present; in each case the stimulus is reflected in a muscular contraction. Sensations excite other sensations; ideas excite other ideas; and one of these ideas may issue in an action of control. But the restraining power is limited, and cannot resist a certain degree of urgency in the original stimulus. I can, for a time, restrain the act of winking, in spite of the sensation of dryness; but the reflex-feeling which sets going this restraining action will only last a few seconds; after which the urgency of the external stimulus is stronger than that of the reflex-feeling—the sensation of dryness is more imperious than the idea of resistance—and the eyelid drops.

If a knife be brought near the arm of a man who has little confidence in the friendly intentions of him that holds it, he will shrink, and the shrinking will be "involuntary"—in spite of his will. Let him have confidence, and he will not shrink, even when the knife touches his skin. The idea of danger is not excited in the second case, or if excited, is at once banished by another idea. Yet this very man, who can thus repress the involuntary shrinking when the knife approaches his arm, cannot repress the involuntary winking when the same friend approaches a finger to his eye. In vain he prepares himself to resist that reflex-action ; in vain he resolves to resist the impulse ; no sooner does the finger approach, than down flashes the eyelid. Many men, and most women, would be equally unable to resist shrinking on the approach of a knife : the association of the idea of danger with the knife, would bear down any previous resolution not to shrink. It is from this cause that timorous women tremble at the approach of firearms. An association is established in their minds which no idea is powerful enough to loosen. You may assure them the gun is not loaded ; "*that* makes very little difference," said a *naïve* old lady to a friend of ours. They tremble, as the child trembles when he sees you put on the mask. These illustrations show that the urgency of any one idea may, like the urgency of a sensation, bear down the resistance offered by some other idea ; as the previous illustrations showed that an idea could restrain or control the action which a sensation or idea would otherwise have produced. According to the doctrines current, the Will is said to be operative when an idea determines an action ; and yet all would agree that the winking which was involuntary when the idea of danger determined it, was voluntary when the idea of communicating with an accomplice in some mystification determined it.

The reader will have gathered already that we admit no real and essential distinction between voluntary and involuntary actions. They are all voluntary. They all spring from Consciousness. They are all determined by feeling. It is convenient, for common purposes, to designate some actions as voluntary; but this is merely a convenience; no psychological, nor physiological, insight is gained by it; an analysis of the process discloses no element in a voluntary action which is not to be found in an involuntary action. In ordinary language it is convenient to mark a distinction between my raising my arm because I will to raise it for some definite purpose, and my raising it because a bee has stung me; it is convenient to say, "I *will* to write this letter," and "this letter is written against my will—I have no will in the matter." But Science is more exacting when it aims at being exact; and the philosopher, analysing these complex actions, will find no element answering to the "will," in one, which is absent from the other: he will find this only, that in each case certain muscular groups have been set in action by certain sensational or ideational stimuli.

It is a very general mistake to suppose that every act of volition implies a distinct idea of its object. Unless such an intellectual element be present, guiding the movement, the voluntary character is said to be wanting. But it is well said by Müller, that "the ultimate source of voluntary motion cannot depend on any conscious conception of its object; for voluntary motions are performed by the fœtus before any object can occur to the mind—before any idea can possibly be conceived of what the voluntary motion effects. . . . The fœtus moves its limbs at first, not for the attainment of any object, but solely because it can move them. Since, however, on this supposition, there can be no particular reason for the movement of any one part, and the fœtus would have equal cause to move all its muscles at the same time, there must be something which determines this, or that, voluntary motion to be performed. The knowledge of the changes of position which are produced by given movements, is gained *gradually, and only by means of the movements themselves.* . . . The voluntary excitation of the origins of the nervous fibres, without objects in view, gives rise to motions, changes of posture, and consequent sensations. Thus a *connection is established* in the yet void mind *between certain sensations and certain motions.* When subsequently a sensation is excited from without, in any one part of the body, the mind will be already aware that the voluntary motion, which is in consequence executed, will manifest itself in the limb which was the seat of sensation; the fœtus *in utero* will move the limb that is pressed upon, and not all the limbs simultaneously. The voluntary movements of animals must be developed in the same manner. The bird which begins to sing is necessitated by an instinct to incite the nerves of its laryngeal muscles to action; tones are thus produced. By the repetition of this blind exertion of volition, the bird at length learns to connect the kind of cause with the character of the effect produced. The instinct of this dream-like and involuntary-acting impulse in the sensorium has some share in the production of certain movements in the human infant, which are in themselves voluntary. In the sensorium of the newly-born child there is a necessitating impulse to the motions of sucking ; but the different parts of the act of sucking are themselves voluntary movements."*

In this passage, Müller calls certain actions voluntary which writers usually consider to be reflex (involuntary), and reflex-actions with them mean actions without sensation ; but as Mr Bain remarks, "It may be by a reflex-action that a child commences to suck when the nipple is put between its lips ; but the continuing to suck so long as the sensation of hunger is felt, and the ceasing when that sensation ceases, are truly volitional acts. All through animal life, down to the very lowest sentient being, this property of consciousness is exhibited, and operates as the instrument for guiding and supporting existence. To whatever lengths the purely reflex instincts, or the movements divorced from consciousness, may be carried on in the inferior tribes, I can with difficulty admit the total absence of feeling in any being we are accustomed to call an animal ; and with this feeling I am obliged also to include this property, *which links the state of feeling with the state of present movement.*"† It is this link of feeling with action, which, according to Müller, constitutes Volition.

This idea of the nature of Volition has been adopted and developed by Mr Bain,[*] with a fulness and suggestiveness of treatment which leaves little to be desired. There are questions he leaves untouched, and points on which I do not find myself able to agree with him; but, on the whole, I have received more light from his work, than from that of any other psychologist, and shall therefore expound his views, in preference to those of any other. He considers the Will as the generalised expression of our power to perform voluntary actions; and by voluntary actions he means those which have an *established* connection with certain sensations. A sensation of pain excites the active organs: the animal struggles till it has escaped the pain; these struggles are involuntary, and they are so because beyond definite control, beyond the guidance of any one feeling; they belong to what Mr Bain calls the "diffusive wave of emotion;" whereas voluntary actions are *isolated*, and directed to a *particular* end. In the course of its struggles, the animal accidentally makes one movement which is followed by an alleviation or cessation of the pain; this makes it discontinue all the other movements, and continue that which alleviates. If any of the other movements are set going, the pain recurs, and warns the animal to cease. The continuance of an alleviating movement, Mr Bain regards as the volitional element.

---

* MÜLLER : *Physiology*, ii. 835.

† BAIN : *The Senses and the Intellect*, p. 296.

"We must in the first instance clearly and broadly separate the diffusive wave, accompanying all emotions as their necessary embodiment, from the active influence now under discussion. This is the more necessary as the two classes of movements are very apt to coexist. A blow with a whip, inflicted on a sentient creature, produces, as a part of the emotional effect, strictly so called, a *general* convulsive start, grimace, and howl; it also produces, in the case of the mature animal, an exertion *in some definite course* to avoid the recurrence of the infliction. The first effect is entirely untaught, primitive, instinctive; being intimately and indissolubly connected with feeling in the very nature of it. The other effect is based likewise on an original property, but brought into the shape that we usually find it in, after some experience and considerable struggles. The element just mentioned, of aim or purpose, in no sense belongs to the movements of the diffused wave, or those constituting the manifestation or expression of the mental state. The ecstatic shout of hilarious excitement, the writhings of pain, are energetic movements, but they belong neither to the class of central spontaneity above described, nor to the voluntary class now under consideration."

---

[*] BAIN: *The Emotions and the Will.*

When a very young infant is in pain, it struggles and squalls. That is all it can do; it does that lustily. Mr Bain considers it due to the diffused wave of emotion. Suppose the cause of the pain to be a needle pricking its foot; the child will make no effort to remove that needle, because the link between such a pain and such an action has not yet been established, and this voluntary effort cannot be made. Before it can make such an effort it must have learned to *local-*

ise its sensations. Every surgeon knows that the young infant may be allowed to have his hands free, when operated on, because it cannot with its hands interfere with the knife, not as yet knowing *where* the seat of pain is. When, later on, it has learned to localise its sensations, it may learn what actions alleviate them. A baby in discomfort from some itching of the nose is at first simply restless; it learns to rub that nose with its little fist, only after much experience of rubbings.

Mr Bain relies on the primary fact that when pain coexists with an accidental alleviating-movement, or when pleasure coexists with a pleasure-sustaining movement, such movements become subject to the control of the respective feelings which occur in their company. It is a primordial law that we shrink from pain and cling to pleasure; as long as the pain is unalleviated, movements are kept up; as soon as one particular movement brings cessation of pain, that movement is kept up. An infant lying in bed has the painful sensation of chilliness. This feeling produces the usual emotional display, namely, movement, perhaps cries and tears. In the course of a variety of spontaneous movements of arms and legs, there occurs an action that brings the child in contact with the nurse lying beside it; instantly warmth is felt, and this alleviation of painful feeling becomes immediately the stimulus to sustain the movement going on that moment. That movement, when discovered, is kept up, in preference to the others. In this way the child learns to connect certain sensations with certain movements, and at a year old will draw close to its nurse whenever the sensation of cold comes on, even during sleep. "It is an original property of our feelings to prompt the active system one way or another, but there is no original connection between the several feelings and the actions that are relevant to each particular case. To arrive at this goal, we need all the resources of spontaneity, trial and error, and the adhesive growth of the proper couples, when they can once be got together. The first steps of our volitional education are a jumble of spluttering, stumbling, and all but despairing hopelessness. Instead of a clear curriculum, we have to wait upon the accidents, and improve them when they come."

No one will withhold his assent from the proposition that a pain increasing in company with any movement must tend to cause the arrest of that movement; or that pleasure increasing in company with a movement must tend to cause the continuance of that movement.

"The spontaneous action that brings a limb into a painful contact, as when the child kicks its foot upon a pin in its dress, is undoubtedly from the earliest moment of mental life arrested. Without this I see no possible commencement of voluntary power. So a movement that mitigates a pain already in operation is maintained, as long as the creature is conscious of diminished suffering. In this way, the arms, hands, and fingers work for abating sharp agony, provided only the right member has found its way into action. No provision, as I have often said, exists at the dawn of life for getting the right member into play. The infant being must go through many a cycle of annoyance, because, among numerous stimulants to action that have occurred, the right one has been omitted. But the true impetus once arising, the mind is alive to the coincidence of this with decreasing or vanished pain; just as, on the other hand, we must suppose it alive to the coincidence of some other movement with an aggravation of the evil. The greater the pain, the more strongly is the alleviating movement sustained when once under way. For the next stage of the process, the establishment of a connection between the pain and the special action, we must fall back upon the foundation of all our acquisitions, namely, the force of contiguous association. The concurrence of a particular sensation, as a prick in the arm, with that retracting movement which rids us of the pain, leads to the rise of an adhesive bond between the two, if a sufficient number of repetitions have occurred. We cannot say how many instances of chance conjunction are requisite to generate an association so strong as to take away the uncertainties attending the spontaneous discharge; all the circumstances governing the rapidity of contiguous adhesion would have to be taken into account in this case. The excitement of strong pain on the one hand, or of strong pleasure on the other, is a favourable moment for the growth of an association: and probably not a great number of those occasions would be necessary to convert an inchoate into a full-formed volition. Full-formed, I say, because when the supposed pain can bring into play the proper movement, in the absence of all spontaneous tendency, we have a case of voluntary power complete for all the purposes of the living being. The example that I am now discussing, namely, the retractation of any part of the body from a painful contact, implies a very numerous set of coincidences between local pains and local movements. For all contacts on the back of the hand, there must be an association with the muscles of flexion; for the palm, the extensor muscles must be affected. For the outside of the arm, the tendency to draw it towards the side has to be prompted. And so in like manner for every part of the body, under an irritating smart, there must be a formed connection between painful sensation arising in the locality and the corresponding movements of retractation. This is one department of voluntary acquisition, and consists of a multitude of couples of individual sensations and individual movements, joined by association, after being commenced by spontaneity. For the class of acute pains supposed the acquirement is perfect, owing in a great measure to the simplicity of the case. It is not so with many of those muscular pains, which we are professedly considering at present, although in the foregoing illustration we have departed from them, and somewhat anticipated the subject of sensation at large. The cramps of the limbs do not ordinarily suggest the alleviating action. Owing partly to the rarity of the feeling, we have not usually a full-formed volition which enables the state of suffering to induce the alleviating action, and consequently we are thrown upon the primitive course of trial and error. This instance shows, by contrast with the preceding, how truly our voluntary powers result from education. An established link between a cramp in the ball of the leg, and the proper actions for doing away with the agony, is quite as great a desideratum as drawing up the foot when the toe is pinched or scalded; yet no such link exists, until a melancholy experience has initiated and matured it. The connection in the other case is so well formed from early years, that almost everybody looks upon it as an instinct, yet why should there be an instinct for the lighter forms of pain, and none for the severest? The truth is, that the good education in the one is entirely owing to our being more favourably situated for making the acquisition."

Hitherto we have seen voluntary actions under the guidance of sensations only; let us now observe the ideal guidance. A child is seated at table. He places his hand upon the bright teapot, and the pain of the burn makes him withdraw his hand; again the brightness attracts his curious fingers, and again the pain makes him desist. After a certain number of trials the idea of the pain is so associated with that of the teapot, that the child no longer burns himself. But he has thrust his hand into the biscuit-plate, and finds this action rewarded with a biscuit instead of a burn. On repeating it he is scolded, or slapped, or put into the corner—made to suffer pain; and if this pain be *always* inflicted when he acts thus, he will soon learn to restrain his forays upon the biscuits. This link which is established between an action and a pain, is an ideal link, and finds its place in memory; it is nearly as firm as a sensational link. The supposition, however, that this ideal link makes the action voluntary, as distinguished from an action which is guided by a sensational link, will not withstand criticism.

At first our actions are guided by sensations; then by the ideal representatives of those sensations.

"Instead of an actual movement seen, we have for the guiding antecedent a movement conceived, or in idea. The association now passes to those ideal notions that we are able to form of our various actions, and connects them with the actions themselves. All that is then necessary is a determining motive, putting the action in request. Some pleasure or pain, near or remote, is essential to every volitional effort, or every change from quiescence to movement, or from one movement to another. We feel, for example, a painful state of the digestive system, with the consequent volitional urgency to allay it; experience, direction, and imitation, have connected in our minds all the intermediate steps, and so the train of movements is set on. On the table before us we see a glass of liquid; the infant never so thirsty could not make the movement for bringing it to the mouth. But in the maturity of the will, a link is formed between the appreciated distance and direction of the glass, and the movement of the arm up to that point; and under the stimulus of pain, or of expected pleasure, the movement is executed."

It often happens that we are conscious of "an interval of suspense between the moment of painful urgency and the moment of appeasing action;" because the reflex-feelings are many, and these cross and recross each other, so that no one of them issues in action. This was the case with the frog which we cited just now; instead of hopping away when pinched, it cowered and seemed hesitating as to its escape.

And this leads us to consider how thoughts, no less than actions, can be controlled; how the mind has power over its actions, no less than over the actions of the body. The fact that we can, in some degree, control the thoughts, is indisputable; *how* we do so is not so clear.

The power of keeping up one train of thought, is analogous to that of keeping up one course of muscular action. We cling to certain ideas because they are pleasant, or interest us, or because some remote pain or pleasure stimulates us; and we repress all other thoughts as they arise, just as we should repress movements which disturbed a pleasurable sensation. It is notorious that we cannot call up any one idea at will; but having once got hold of the idea, we can keep it before the mind. What Mr Bain has said when treating of the intellectual process named by him "constructive association," seems to us the true explanation of *all* command over the thought:—

"When Watt invented his 'parallel motion' for the steam-engine, his intellect and observation were kept at work, going out in all directions for the chance of some suitable combination rising to view; his sense of the precise thing to be done was the constant touchstone of every contrivance occurring to him, and all the successive suggestions were arrested, or repelled, as they came near to, or disagreed with, this touchstone. The attraction and repulsion were purely volitional effects; they were the continuance of the very same energy that, in his babyhood, made him keep his mouth to his mother's breast while he felt hunger unappeased, and withdraw it when satisfied, or that made him roll a sugary morsel in his mouth, and let drop or violently eject what was bitter or nauseous. The promptitude that we display in setting aside or ignoring what is seen not to answer our present wants, is volition, pure, perennial, and unmodified; the power seen in our infant struggles for nourishment and warmth, or the riddance of acute pain, and presiding over the last endeavours to ease the agonies of suffering. No formal resolution of the mind, adopted after consideration or debate, no special intervention of the 'ego,' or the personality, is essential to this putting forth of the energy of retaining on the one hand, or repudiating on the other, what is felt to be clearly suitable, or clearly unsuitable, to the feelings or aims of the moment. The inventor sees the incongruity of a proposal, and forthwith it vanishes from his view. There may be extraneous considerations happening to keep it up in spite of the volitional stroke of repudiation, but the genuine tendency of the mind is to withdraw all further consideration, on the mere motive of unsuitability; while some other scheme of an opposite nature is, by the same instinct, embraced and held fast. In all these new constructions, be they mechanical, verbal, scientific, practical, or æsthetical, the outgoings of the mind are necessarily at random; the end alone is the thing that is clear to the view, and with that there is a perception of the fitness of every passing suggestion. The volitional energy keeps up the attention, or the active search, and the moment that anything in point rises before the mind, springs upon that like a wild beast on its prey."

Nowhere has Mr Bain expressed himself categorically respecting the difference between voluntary and involuntary actions; but he assumes the difference, and, implicitly at least, he makes it depend on the establishment of the link of feeling. "Voluntary actions," he says, in the nearest approach to a definition we can recall, "are distinguished from reflex and spontaneous activity by the *directive intervention of a feeling in their production.*" In denying the intervention of sensation in reflex-actions, he only follows the current theory; but unless he separates the involuntary from reflex and spontaneous actions, he falls into manifest contradiction with his own principles, in making the intervention of feeling *the* mark of a voluntary act. We have endeavoured to show that both voluntary and involuntary actions are reflex, following upon the stimulus given to their centres, that stimulus being sensational or ideational. Nor is this all: they are both capable of being brought under *control*—that is to say, of being restrained or originated by the influence of some other centre. That we do not habitually control (that is, *interfere* with) the action of the heart, the contraction of the iris, or the activity of a gland, is true; it is on this account that such actions are called involuntary; they obey the immediate stimulus. But it is an error to assert, as most physiologists and psychologists persist in asserting, that these actions *cannot* be controlled, that they are altogether beyond the interference of other centres, and cannot by any effort of ours be modified. It is an error to suppose these actions are essentially distinguished from the voluntary movement of the hands. We have acquired a power of definite direction in the movements of the hands, which renders them obedient to our will; but this acquisition has been of slow laborious growth. If we were asked to use our toes as we use our fingers—to grasp, paint, sew, or write with them, we should find it not less impossible to control the movements of the toes in these directions, than to contract the iris, or cause a burst of perspiration to break forth. Certain movements of the toes are possible to us; but unless the loss of our fingers has made it necessary that we should use our toes in complicated and slowly acquired movements, we can do no more with them than the young infant can do with his fingers. Yet men and women have written, sewed, and painted with their toes. All that is required is that certain links should be established between sensations and movements; by continual practice these links *are* established; and what is impossible to the majority of men, becomes easy to the individual who has acquired this power. This same power can be acquired over what are called the organic actions; although the habitual needs of life do not *tend* towards such acquisition, and without some strong current setting in that direction, or some peculiarity of organisation rendering it easy, it is not acquired. In ordinary experience the number of those who can write with their toes is extremely rare, the urgent necessity which would create such a power being rare; and rare also are the examples of those who have any control over the movement of the iris, or the action of a gland; but both rarities exist.

It would be difficult to choose a more striking example of reflex-action than the contraction of the iris of the eye under the stimulus of light; and to ordinary men, having no link established which would guide them, it is utterly impossible to close the iris by any effort. It would be not less impossible to the hungry child to get on the chair and reach the food on the table, until that child had *learned* how to do so. Yet there are men who have learned how to contract the iris. The celebrated Fontana had this power; which is possessed also by a medical man now living at Kilmarnock—Dr Paxton—a fact authenticated by no less a person than Dr Allen Thomson.* Dr Paxton can contract or expand the iris at will, without changing the position of his eye, and without an effort of adaptation to distance.

---

* *Glasgow Medical Journal*, 1857, p. 451.

To move the ears is impossible to most men. Yet some do it with ease, and all can learn to do it. Some men have learned to "ruminate" their food; others to vomit with ease; and some are said to have the power of perspiring at will.* That many glands are under the influence of the Will—in other words, that we can stimulate them to secretion by a mere ideal stimulus—is too well known to need instance here. Even the beating of the heart can be arrested. The heart has its own nervous system. The minute ganglia imbedded in its substance regulate its rhythmic movements; and long after death the heart is seen to beat. But although thus independent, it is also dependent: its nervous system is in connection with the spinal chord and brain; and influences from these will act upon it (see p. 147). Thus it is that emotions agitate the heart; the disturbance of its movements comes from the interference of brain or chord. Now, if once we recognise a channel of sensation, we recognise a possible source of control; and if the daily needs of life were such that to fulfil some purpose the action of the heart required control, we should learn to control it. Some men have, without such needs, learned how to control it. The eminent physiologist, E. F. Weber of Leipzig, found that he could completely check the beating of his heart. By suspending his breath, and violently contracting his chest, he could retard the pulsations; and after three or five beats, unaccompanied by any of the usual sounds, it was completely still. On one occasion he carried the experiment too far, and fell into a syncope. Cheyne, in the last century, recorded a case of a patient of his own who could at will suspend the beating of his pulse, and always fainted when he did so.

---

* MAYER: *Die elementarorganization des Seelenorgans*, p. 12, is the authority for the last statement.

It thus appears that even the actions which most distinctly bear the character recognised as involuntary—uncontrollable—are only so because the ordinary processes of life furnish no necessity for their control. We do not learn to control them, though we could do so, to some extent; nor do we learn to control the motions of our ears and toes, although we could do so. And while it appears that the involuntary actions can become voluntary, it is familiar to all that the voluntary actions tend, by constant repetition, to become involuntary, and are then called secondarily automatic.

The conclusion at which we arrive is this: Popular language conveniently classes actions as voluntary when a distinct conception of the object to be achieved accompanies or originates them. But Pyschology and Physiology, descending deeper than such classifications, and analysing the process which takes place in the organism, declare that all actions whatever are the responses of organs to the stimulus of their nerve-centres. Whether the action be the movement of a muscle or the secretion of a gland, it is finally determined by the centre from which the organ is supplied. This centre may be stimulated by a sensory nerve going from the surface—as when the salivary gland pours out its secretion after the stimulus of food, or the limb contracts on the stimulus of pain. The centre may also be stimulated by the action of some *other* centre: as when the *idea* of food causes a flow of saliva; or an irritation of the salivary gland causes a flow of gastric juice. But whether the action result from a direct or an indirect stimulus, it is always the same response of an organ to its centre; whether the starting-point be an idea, or a sensation, the final issue is an excitation of the particular centre, and the response of a particular action.* We cannot separate some actions from others, and call them voluntary merely because they are dependent on a link of feeling, since all actions are dependent on sensation. And if any reader objects to such a conclusion on the ground that it makes the Soul animate the *whole* body, and preside over all its actions, not simply over a few of them—if he objects that we are thereby retrograding towards the doctrine of Stahl—our reply is: we must follow Logic whither Logic leads. Any reader who is uncomfortable at the idea of retrograding, who is unwilling to believe that all the phenomena of his sensitive organism have one common source, one kindred nature, and one common name—the soul—is at perfect liberty to try and reach some other conviction which, besides being more agreeable to his feelings, will better explain the facts. It is a topic on which no man will, wisely, dogmatise. The veil of mystery will never be lifted. We who stand before that veil, and speculate as to what is behind it, can but build systems; we cannot see the truth.

---

* In the valuable work by M. LANDRY—*Traité des Paralysies*, the first part of which appeared while these sheets were at the printer's—there is a view of voluntary, involuntary, and reflex actions, almost identical with the one given above. Indeed, although M. LANDRY'S language, and his psychological opinions generally, accord with current doctrines, the physiological conclusions to which his researches have conducted him agree, in the main, with those I have endeavoured to establish

VI. THE SPINAL CHORD AS A SENSATIONAL, VOLITIONAL CENTRE.—The ground has now been fairly cleared for a most important inquiry, one not only essential to the argument against the Brain as the exclusive organ of the Mind, but also essential to the right understanding of most questions relating to nervous phenomena. It was necessary that we should first come to a distinct understanding about the Reflex-Theory; because while, on the one hand, that Theory relied for support on the facts which showed that the Spinal Chord was an independent centre, acting as well without the Brain as with it—and hence its actions were assumed to be independent of sensation;—on the other hand, the facts which unequivocally declare that the Spinal Chord *is* a sensational centre, were set aside, and explained away, as being "merely reflex." To prove that reflex-actions are independent of sensation, it is first necessary to prove that the actions of the Spinal Chord *are* independent of sensation. This has never been proved: it has been assumed; and assumed in the face of opposing evidence. I shall now endeavour to show that the Spinal Chord is a sensational centre; a conclusion which would give the final blow to the Reflex-Theory.

1. *Views of earlier Writers.*—Although the reasons for the opinion maintained in these pages respecting the fundamental agreement of all nerve-centres, have not, I believe, before been applied to the functions of the Spinal Chord, the opinion itself, of the sensational functions of the chord, is by no means new. Robert Whytt maintained it. Prochaska, as we have seen, held that the spinal chord formed the greater part of the *sensorium commune;* and he adduced, in proof, the familiar facts of sensibility manifested by headless animals. The next writer whom I can discover to have held this opinion is J. J. Sue—the father of the celebrated French romance-writer—who, in 1803, conceived that his experiments proved the Spinal Chord to be capable of replacing, to a certain extent, the functions of the Brain.* Next came Legallois,† who undertook to show, by a series of experiments, that the principle of sensation and movement, in the trunk and extremities, has its seat in the Spinal Chord. The mere division of

---

* SUE: *Recherches Philosophiques sur la Vitalité et le Galvanisme,* p. 9. He was not consistent, however, but adopted Bichat's opinion respecting the sensibility of the viscera, p. 68.

† LEGALLOIS: *Expériences sur le principe de la vie.* Published, I conclude, in 1811; the edition I use is the one printed in the *Encyclopédie des Sciences Medicales,* iv.

the chord, he said, produces "the astonishing result of an animal, in which the head and the body enjoyed separate vitality, the head living as if the body did did not exist, and the body living as if the head did not exist. Guinea-pigs, after decapitation, seem very sensitive to the pain caused by the wound in the neck; they alternately carry first one hind-leg, and then the other, to the spot, as if to scratch it. Kittens also do the same."

A few years afterwards, 1817, Dr Wilson Philip concluded that "the spinal marrow possesses sensorial power, as appears from very simple experiments;" but he held the Brain to be the *chief* source of sensorial power.* The following year, Lallemand supported this opinion by the very curious phenomena exhibited by infants born without brains: these infants breathed, swallowed, sucked, squalled, and gave very unequivocal signs of sensibility. The value of such observations consists in disproving the objection frequently urged against the evidence of decapitated animals, namely, that in these animals the Spinal Chord preserves the remains of a sensibility endowed by the Brain.

Longet here places an observation recorded by Beyer. A new-born infant, whose brain, during the birth, had been completely extirpated (to save the mother's life), was wrapped in a towel, and placed in the corner of the room, as a lifeless mass. While the surgeon was giving all his care to the mother, he heard with horror a kind of murmur proceeding from the spot where the body had been placed. In three minutes a distinct *cry* was heard. The towel was removed, and, to the surprise of all, this brainless infant was seen struggling with rapid movement of its arms and legs. It cried, and gave other signs of sensibility for several minutes.*

---

* WILSON PHILIP: *Experimental Inquiry into the Laws of the Vital Functions*, pp. 209, 210.

In 1828, Calmeil arrived at the same conclusion as that reached by Legallois, Wilson Philip, and Lallemand. Indeed when, in 1833, the Reflex-Theory appeared, this opinion was so firmly rooted, that we find Mr Grainger combating it as the established error of the day. He takes as much pains to show that physiologists are wrong in attributing sensation to the Spinal Chord, as I am here taking to show that they were right.† "It is, indeed, apparent," he says, "that the whole question concerning the truth or falsehood of the theory which attributes the reflex power to the Spinal Chord, hinges upon the correctness or incorrectness of the received doctrines respecting the seat of sensation and volition; so that until those doctrines are proved to be false, it is impossible to establish the hypothesis of Dr Hall."‡

The reader is requested to take note of this, because when we come to the evidence which proves the Spinal Chord to be a centre of sensation, we shall find that the *only* ground for rejecting that evidence is the assumed truth of the Reflex-Theory, coupled with the assumption of the Brain being the exclusive seat of sensation. Whereas if the evidence proves that the Spinal Chord *is* a sensational centre, then the Reflex-Theory is destroyed, and cannot be urged against such evidence.

---

* LONGET: *Traité de Physiologie,* ii. 105.

† He cites Cuvier, Majendie, Desmoulins, and Mayo as maintaining this error.

‡ GRAINGER: *Structure and Functions of the Spinal Chord,* p. 66.

Thus many of the facts which prove the sensational function of the Spinal Chord were known, and even a vague conception of their real significance was general, until the Reflex-Theory came to explain all such facts as the results of mechanical adjustment, and of a new nervous principle called "Reflexion." For twenty years this theory has reigned, and met with but little opposition. Yet the true doctrine has not wanted defenders in Germany. Nasse[*] denied that decapitated animals showed no spontaneity, and asserted that they exhibited clear signs of mental activity. Carus sarcastically pointed out that the *word* "reflex" was replacing "irritability," as a key to unlock all puzzles; and he took up a position which is very similar to the one occupied in these pages, namely, that the Spinal Chord being formed of grey matter as well as of fibres, it must have sensibility and power of reacting on nervous stimulus, no less than conductibility: in fact, it is a centre, and must act like all other nerve-centres.[†] J. W. Arnold refuted the Reflex-Theory in a very remarkable little work, in which he vindicates the claim of the Spinal Chord as a sensitive and motive centre, although denying to its actions any volitional character.[*] This was in 1844. Eleven years elapsed without any further opposition, when Edward Pflüger, in 1853, published his work on the Sensorial Functions of the Spinal Chord.[†] In this work he recurred to the old views of Prochaska and Legallois; but although he attacked Marshall Hall with merciless severity, he did not point out the fundamental error of the Reflex-Theory, which theory he seems to accept. Nor did he give his views that philosophical and anatomical basis which could alone render his interpretations acceptable. Added to this, the tone of asperity in which his work was written, created a prejudice against him; and thus, while many admitted his facts, all rejected his conclusions.[‡]

---

[*] NASSE: *Unters. zur Physiologie und Pathologie*, vol. ii. part 2.

[†] CARUS: *System der Physiologie*, iii. 101.

In 1858 Professor Owen read a paper of mine at the Leeds meeting of the British Association, on "The Spinal Chord as a Centre of Sensation and Volition," in which a rapid indication of my point of view, and an account of some experiments to illustrate it, were given—*not*, I believe, conclusive to any of the audience. Indeed, the subject was too vast to be discussed in such a paper; and my object was rather to excite new inquiry, than to make converts to a view which could only be embraced after a thorough reinvestigation of the dominant theories.

---

[*] J. W. ARNOLD: *Die Lehre von der Reflex-Function*, 86.

[†] PFLÜGER: *Die sensorischen Functionen des Rückenmarks der Wirbelthiere.*

[‡] Except AUERBACH, who repeated and varied the experiments; and FUNKE, who partially adopted the conclusions in his systematic treatise on Physiology.

In 1859 appeared Schiff's work, to which so many references have already been made;[*] and here we find a large space allotted to the discussion of Pflüger's doctrine. Schiff, whose immense experience as an experimentalist, and whose acuteness and caution, every one will highly estimate, frankly pronounces in favour of the sensational character of spinal actions; but he denies that they are volitional, and objects strongly to the introduction of any such idea as that of "psychical activity." He thinks it utterly untenable to suppose that impressions have reactions in the brain which they have not in the Spinal Chord:—if one has sensibility, the other must have it; and he thinks that, so far from the actions of the chord being distinguishable from those of the Brain by the character of "reflexion," and depending on a mechanical arrangement—*all* actions, cerebral or spinal, are reflex; *all* depend on a mechanical arrangement.[†]

From this sketch of the history, let us now pass to the evidence on which the sensational character of the Spinal Chord may be founded. The evidence may be considered under two aspects—argument and experiment; deduction and induction.

2. *The Deductive Evidence.*—In the course of our

---

[*] SCHIFF: *Lehrbuch der Physiol.*, 208.

[†] The very latest writer on the spinal chord—LANDRY: *Traité des Paralysies*—maintains that it is a centre of sensation, and that there is in it a faculty *analogous* to the perception and judgment of the brain. Compare pp. 163 *et seq.* and 305. He also cites an essay by Dr PATON of Edinburgh (*Edinburgh Medical Journal*, 1846), in which the sensational and volitional claims of the spinal chord are advanced.

inquiry we have had so many occasions to notice incidentally the various points of the deductive evidence which establishes the fundamental similarity of the brain and chord, that it will not be necessary here to do more than resume them.

Similarity in structure implies similarity in property; and the ganglionic substance of the Chord being of a similar structure to the ganglionic substance of the Brain, there must necessarily be a community of *property* between them.* It is true that there are some considerable differences in the arrangement of the tissues, in the cerebral and spinal centres; and this, at first, might raise the suspicion that the properties of the two would correspondingly differ. But the nervous system of animals reveals that the mere presence of nerve-fibres and ganglionic substance is all that is requisite for the production of sensibility and volition, quite irrespective of any arrangement of these elements. And further, we know that the Medulla Oblongata is a seat of Sensibility, yet the arrangement of its elements is extremely unlike that of the Brain.

---

* While these sheets are passing through the press, I have read OWSJANNIKOW's dissertation—*De Medullæ Spinalis Textura*—in which he briefly but distinctly expresses the opinion so often urged in these pages, that ganglionic cells must everywhere have the same property; and the brain, being nothing but a mass of such cells and fibres, must resemble every other mass of such cells and fibres. "Mihi enim cerebrum non videtur esse nisi massa, imprimi, ex ingenti cellularum nervearum numero fibrisque ex illis originem ducentibus composita. Itaque in hisce cellulis altiores functiones tum corporis tum animi repositæ sunt. *Quæ cellulæ utrum in unam catervam, pluresve easque majores collectæ sint, au per totum corpus dispersæ reperiantur, tanti non interest, dummodo facultas, qua prædita sunt, eadem maneat,*"—p. 93. If the anatomical fact announced by him at page 17 should be established—namely, that the same cell by its two processes is the origin of both anterior and posterior fibres—it would complete my view of the identity of the so-called sensory and motor nerves. But although the fact would suit my theory, I cannot convince myself of its accuracy.

It has already been seen that the Medulla Oblongata is admitted by physiologists to be a seat of sensibility. But we have also seen that this Medulla is a part of the Spinal Chord, having the same constituent parts, with a surplus of grey matter, the two forming one indistinguishable whole.* The Medulla is as much a part of the Spinal Chord as the skull is a part of the cranio-spinal axis. No anatomist now finds any difficulty in conceiving how the various separate bones which constitute the vertebral column may differ so widely in form, as at one end to become a tail, and at the other a skull; nor does he for an instant suppose that the *properties* of these bones differ at one end of the column and the other; nor that the functions or uses of these bones—protecting the enclosed nerve-centres, and affording attachment to muscles—are not fundamentally similar. A man would incur ridicule if he were to assert that the *os frontis* was essentially unlike the *os coccygis*, in properties as bone. Yet men do not scruple to maintain that the upper part of the Spinal Chord has the property of Sensibility, which they deny to the lower part; although the difference between the two is merely one of form, and is considerably *less* than the difference between two bones. No one is bold enough to assert that the upper part of a muscle is contractile, while the lower part is not so at all. Yet this is really very little more than asserting that the Medulla has sensibility, but the Spinal Chord has none.

---

* If any one, misled by certain unimportant differences, is inclined to dispute this identity of the Medulla and the Chord, he is recommended to study the comparative anatomy of the cerebro-spinal axis, and he will find on reaching the frog, triton, lizard, and probably all other reptiles, that all trace of the Medulla as a *distinct* organ has disappeared: it is then nothing but the upper part of the chord.

Mr Grainger is the only writer whom I can remember to have avoided this grave logical error. He maintains, and justly, that the difference between the Medulla and the chord is one of degree only, not of kind; and it is on this ground that he *denies* the Medulla to be a seat of sensation, because the chord, he thinks, is proved not to be sensitive. I hold that both are seats of sensation; or else, as he says, that neither is.

The only ground for denying that the actions of decapitated animals are determined by sensation, is because the Brain, or encephalon, is believed to be the sole seat of sensation. To explain the resemblance between the actions of animals with and without their brains, a theory is invented, which says: These actions are reflex; but in the uninjured animal there is reflex-action *plus* the transmission of an impression to the brain, and it is *this* which produces sensation; in the headless animal we see reflex-action, *minus* the transmission to the Brain.

As a logical deduction from the assumption that *only* the brain can be the seat of sensation, this theory is perfect. Unhappily for it, the facts contradict the assumption; they show that sensation and volition *are* manifested where there is no Brain. Moreover, the assumption of reflex-action taking place without sensation, is made on a false analogy of the vital mechanism with an ordinary mechanism. When facts show, as we shall presently see, that decapitated animals manifest the *same* kind of evidence of sensation and volition, as is manifested by the same animals with their heads on, the assumption that they *cannot* feel, because their heads are off, is rather arbitrary.

A gentleman was one day stoutly asserting that there were no gold-fields except in Mexico and Peru. A nugget, dug up in California, was presented to him, as evidence against his positive assertion. He was not in the least disconcerted. "This metal, sir, is, I own, extremely *like* gold; and you tell me that it passes as such in the market, having been declared by the assayers to be undistinguishable from the precious metal. All this I will not dispute. Nevertheless, the metal is not gold, but *auruminium;* it cannot be gold, *because* gold comes only from Mexico and Peru." In vain was he informed that the geological formation was similar in California and Peru, and the metals similar; he had fixed in his mind the conclusion that gold existed *only* in Mexico and Peru: this was a law of nature; he had no reasons to give why it should be so; but such had been the admitted fact for many years, and from it he would not swerve. He was not fond of newfangled notions, which, after all, would only lead us back to the exploded errors of the past.

To accept the statement that gold was to be found elsewhere than in Mexico and Peru, would be to return to the opinion of the ancients, who thought there was gold in the upper regions of Tartary!

Sensation is not tangible, assayable, like gold. We can understand, therefore, that the very men who would make merry with the *auruminium*, would accept easily such a phrase as "reflex-action." The decapitated animal defends itself against injury, gets out of the way of annoyances, cleans itself, performs many of its ordinary actions, but is said to do these things without that Sensibility which, if its head were on, would guide them. Even before the Reflex-Theory was invented this line of argument was used. Gall, referring to the experiments of Sue, previously noticed, says that "Sue confounds the effects of Irritability with those of Sensibility."* Not gold, dear sir, but *auruminium!*

This denial of sensation to the decapitated animal is something like what the missionary Williams tells us of the Fijian philosophy. The body of a dying man in Fiji is laid out several hours before actual death. "I have known one take food afterwards; and another who lived eighteen hours after. All this time, in the opinion of a Fijian, the man was dead. Eating, drinking, and talking, he says, are the involuntary actions of the body—of the 'empty shell,' as he calls it—the soul having taken its departure."†

---

* GALL *et* SPURZHEIM: *Anat. et Physiol. du Système Nerveux*, i. 83.

† WILLIAMS: *Fiji and the Fijians*, i. 88.

3. *The Inductive Evidence.*—We have now to consider the proofs furnished by observation. In using the word *proof*, as applied to such investigations, we must be understood as implying the kind of proof which, in the nature of things, *can* be had, and not the kind of proof to be had in physics, or chemistry. Schiff is right in saying that no demonstration is possible in such cases. Feeling is purely subjective. That others feel, as we feel, is to us a matter of positive conviction, but can never be a matter of demonstration. We conclude that other men feel as we feel, because they act as we act. This conclusion is extended to animals: the resemblance of their acts to our own, under similar conditions, forces on us the conviction that they also resemble us in feeling. This conclusion, although purely inductive, is so strong that there are few theorists, even for the sake of a theory, who can believe animals to be machines destitute of all sensation. Nevertheless, we have no *proof*, rigorously speaking, that any animal feels; none that any human being feels; we *conclude* that men feel, from certain external manifestations, which resemble our own, under feeling; and we conclude that animals feel—on similar grounds.

Now, inasmuch as the actions of animals furnish us with our sole evidence for the belief in their feeling, and this evidence is universally considered as scientifically valid, it is clear that similar actions in decapitated animals will be equally valid; and when I speak of *proof*, it is in this sense. *Spontaneity* and *choice* are two signs which we all accept as conclusive of sensation and volition. The dog rising from the ground, and walking away, not being called or driven away, is justly supposed to manifest spontaneity and volition. He is doubtless moved by some internal stimulus, of sensation or idea; but because the stimulus is not external, we call the action spontaneous. No action can be self-determined. The law of Causation forces us to assume that every action is preceded by *some* stimulus, internal or external. Without entering on a metaphysical discussion of spontaneity, we may conveniently class all actions which are determined by some *internal* stimulus, sensational or ideational, as spontaneous, in distinction from actions determined by some *external* stimulus: the dog leaps from a chair spontaneously; but when pushed from it, the action is not spontaneous.

Choice is another sign peculiar to conscious organisms. In a mechanism, without sensation to determine its actions, there is absolute *uniformity:* the same note always responds to the same key, the same wheels are set going in the same order; there is no suspension of one course of action, and substitution of another, such as we observe in the vital mechanism. It is difficult to determine what choice is, but if we say that whenever an animal brings new actions into play, to accomplish an end which it has previously missed, whenever it ceases one set of actions, and *substitutes* several others, until a successful issue is reached, it manifests volition. Indeed this, as we previously saw, constitutes a voluntary action. Philosophers have not yet agreed on the nature of volition; but all men will admit that when they observe a monkey, who has burnt his paws, seize an unsuspecting cat, and with *her* paws reach the chestnuts, a voluntary action is before them. If a tickling in your throat excites the action of coughing, and when this coughing does not succeed, if you swallow some water, this choice of new means is assuredly the sign of a voluntary act.

Thus, escaping from all metaphysical ambiguities, we may assume that *spontaneity* and *choice* are two palpable signs by which to recognise the presence of sensation and volition. Without pretending to know what takes place within an animal, we may feel confident that if its actions manifest spontaneity and choice, they are not the mere results of mechanical adjustment, but have in them the elements of sensation and volition. Our task, then, is to ascertain whether animals, after decapitation, manifest these palpable signs.

It is emphatically asserted by the advocates of the Reflex-Theory that spontaneity is utterly absent in decapitated animals.* This statement I have already shown to be erroneous (p. 184); but I will here add further evidence. I decapitated a toad and a triton; and merely divided the spinal chord of a frog and another triton. The four were placed in the same pan. At first the spontaneous movements of the decapitated pair were insignificant; but on the second day the headless toad was quite as lively and restless as

* MARSHALL HALL: *New Memoir*, 35. GRAINGER, 149.

the frog with a head; and the headless triton little less so than his companion with a head. I have at this moment a frog whose chord has been divided for some weeks: he remains almost immovable unless touched; he is generally found in the same spot, and in the same attitude to-day as yesterday, unless touched, or the table is shaken. I notice that he occasionally moves one of his fore-legs; occasionally one of his hind-legs; but without changing his position. If he were headless, this quiescence would be cited as a proof of the absence of spontaneity; whereas I have had a headless companion with him, who very frequently moved about.

It should be observed that an animal without its head is without the various stimuli which can be received through sight, smell, and hearing, each of which will determine movements; it is therefore necessarily quiescent, unless some of its visceral sensations stimulate it. And it is to the variety of states which may be determined by changes in the circulation, and the conditions of the viscera, that the great variety in the actions of decapitated animals must be attributed. Some never move, but die quietly on the spot where they were left; others are restless; others move occasionally. Nevertheless I affirm that attentive and repeated examination of decapitated animals will furnish abundant evidence of spontaneous action.*

---

* LANDRY: *Traité des Paralysies*, p. 305, also believes the Spinal Chord to originate spontaneous movements.

Let us now pass on to *choice.* Place a child of two or three years old upon his back, and tickle his right cheek with a feather. He will probably move his head away. Continue tickling, and he will rub the spot with his right hand, *never* using the left hand for the right cheek, so long as the right hand is free; but if you hold his right hand, he will use the left. Does any one dispute the voluntary character of these actions?

Now contrast the actions of the sleeping child under similar circumstances, and their sequence will be precisely similar. This contrast is the more illustrative, because physiologists generally assume that in sleep consciousness and volition are *suspended.* "The brain sleeps, the spinal chord never; volition and sensation may be suspended, but not reflex-action." This proposition is extremely questionable; yet it is indispensable to the reflex-theory; because unless sensation and volition *are* suspended during sleep, we must admit that they can act, without at the same time calling into activity that cerebral sensibility, which, in ordinary language, is supposed to constitute all consciousness. The child moves in his sleep, defends himself in his sleep; but he is not "aware" of it: that is to say, the actions are not combined with those cerebral sensibilities, which would come into play in waking moments. The spinal sensibility acts without the cerebral sensibility; but the actions are nevertheless determined by sensation and volition.

"Children," says Pflüger, "sleep more soundly than adults, and seem to be more sensitive in sleep. I tickled the right nostril of a three-year-old boy. He at once raised his right hand to push me away, and then rubbed the place. When I tickled his left nostril he raised the left hand. I then softly drew both arms down, and laid them close to the body, imbedding the left arm in the clothes, and placing on it a pillow, by gentle pressure on which I could keep the arm down without awakening him. Having done this I tickled his left nostril. He at once began to move the imprisoned arm, but could not reach his face with it, because I held it firmly though gently down. He now drew his head aside, and I continued tickling, whereupon he raised the *right* hand, and with it rubbed the *left* nostril—an action he never performed when the left hand was free."

This simple but ingenious experiment establishes one important point, namely, that the so-called reflex-actions observed in sleep are determined by sensation and volition. The sleeping child behaves exactly as the waking child behaved; the only difference being in the energy and rapidity of the actions. If the waking child felt and willed, surely the sleeping child, when it performed precisely similar actions, cannot be said to have felt nothing, willed nothing? It is not at one moment a conscious organism, and at the next an unconscious mechanism.

It is possible to meet this case by assuming that the child was nearly awake, and that a dim consciousness was aroused by the tickling, so that the cerebral activity was in fact awakened. But, plausible as this explanation may be, it altogether fails when we come to experiments on decapitated animals. If any one will institute a series of such experiments, taking care to compare the actions of the animal before and after decapitation, he will perceive that there is no more difference between them than between those of the sleeping and the waking child.*

The following may serve as a sample: I placed a large and very vigorous triton in an empty cigar-box, and noted down in my note-book the various actions it performed. After running about, and vainly trying to escape, it seemed to resign itself to its prison, and with the apathy characteristic of these creatures, remained quite motionless for some time. While thus resting, it occasionally moved its tail, or raised its head, and turned it round. Then it took a single step forwards, and stood still again. It then moved three steps forward, and rested. I touched its foot gently with the scissors, and the foot was slowly withdrawn, but the animal never moved. I touched it three times rapidly, but gently, and this produced a slow crawl. I *pressed* the scissors, and it at once started off in a rapid run. I next touched its flank with acetic acid. It ran about manifesting great discomfort, but showed no intention of rubbing the acid away with its foot—as the frog always does—indeed it seemed as unable to protect itself against the unpleasant sensation, as a baby is when a needle in its frock is pricking it: the pain produced a *general agitation*, but no voluntary act. At length, however, on a second application of acid, while running about, its flank came accidentally in contact with the side of the box, and the sensation seemed to guide the animal to a voluntary act, for it now continued crawling along, rubbing its flank against the rough wood, until the acid was all rubbed off, or the sensation had abated. Here was the link of feeling—the continuance of an action once begun — which we have already seen to characterise voluntary actions.

---

* It is better simply to remove the brain, than to remove the whole head, which causes a serious loss of blood. An etherised animal may be operated on with ease and accuracy. For many experiments, mere division of the spinal chord is better than decapitation. Great variations in the results must be expected, because the condition of the animal, its age and sex—whether fasting or digesting—whether the season be spring or summer—and a hundred other causes, complicate the experiment.

I now cut off its head, at the articulation of the occiput with the atlas, sparing the lower jaw, to save the loss of blood. It dropped on the left side, with the fore-legs pressed close to its body. Three-and-twenty minutes after the operation, it began to move the left hind-leg feebly; still, however, lying on its side. Four minutes after, the right leg began to move. Presently it rose on all fours, and turned round the remnant of its head, raising it also in the air, as if seeking something. It then relapsed into motionlessness. After eighteen minutes of rest it once more raised neck and shoulders, and began to move forward. After a pause the tail began to move backwards and forwards.

It then crawled two steps. Finally its movements became sufficiently energetic for it to crawl a distance of two inches without pausing. Having allowed it to recover itself, I repeated the tests of sensibility, which had been employed before decapitation. The reactions were precisely similar. When the foot was gently touched, it was languidly withdrawn; when irritated, the animal moved; when the foot was pressed on by the scissors, an energetic movement followed; and when acetic acid was applied, the animal almost ran. I now waited half an hour, and then touched its flank with acetic acid; as on the former occasion, this made it very agitated; but its movements were disorderly, and helpless. In vain I watched, expecting to see it rub its flank against the side of the box; it curled itself in the centre, and there writhed. I now gently pushed it towards the side, but it remained motionless; when, however, I slowly pushed it forwards, so that its flank might rub the side, the "sensational link" seemed established, for the animal now crawled slowly along, pausing at intervals, its body curved inwards towards the wood, and thus rubbed the acid away.*

The evidence of spontaneity and choice, of sensibility and volition, is unmistakable in the foregoing case. Even more striking is the following experiment, devised by Pflüger, which I have verified, and varied, many times: A frog is decapitated, or, better still, its brain is removed. When it has recovered from the effect of the ether, and manifests lively sensibility, we place it on its back, and touch, with acetic acid, the skin of its thigh (just above the *condylus internus femoris*). Let the reader imagine his own shoulder burnt at the point where it can be reached with the thumb of the same arm, and he will realise the operation. No sooner does the acid begin to burn, than the frog stretches out the *other* leg, so that its body is somewhat drawn towards it. The leg that has been burnt is now bent, and the back of the foot is applied to the spot, rubbing the acid away—just as your thumb might rub your shoulder. This is very like the action of the tickled child, who always uses the right hand to rub the right cheek, unless it be held; but when the child's right hand is prevented from rubbing, the left will be employed; and precisely this do we observe with the brainless frog: prevent it from using its right leg, and it will use its left!

---

* This was the only triton I ever observed to get rid of the acid by rubbing its body against anything accidentally coming in contact with it; but this one did so almost as well without as with its head.

To show this we decapitate another frog, and cut off the foot of the leg we are to irritate. No sooner is the acid applied, than the leg is bent as before, and the stump is moved to and fro, as if to rub away the acid. But the acid is not rubbed away, and the animal becomes restless, as if trying to hit upon some other plan for freeing himself of the irritation. And it is worthy of remark that he often hits upon plans very similar to those which an intelligent human being adopts under similar circumstances. Thus, the irritation continuing, he will sometimes cease the vain efforts with his stump, and stretching that leg straight out, bends the *other* leg over towards the irritated spot, and rubs the acid away. But, to show how far this action is from one of unconscious mechanism, how far it is from being a direct reflex of an impression on a group of muscles, the frog does not *always* hit even on this plan. Sometimes it bends its irritated leg more energetically, and likewise bends the body towards it, so as to permit the spot to be rubbed against the flank—just as the child, when both his hands are held, will bend his cheek towards his shoulder and rub it there.

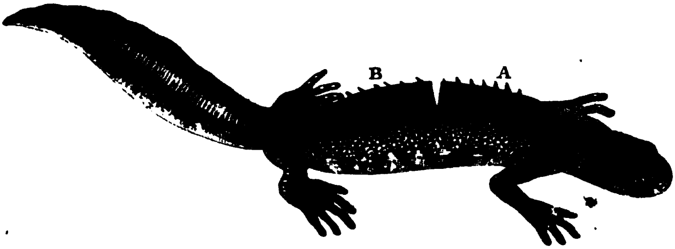
It is difficult to resist such evidence of *choice* as is here manifested. The brainless frog chooses a new plan when the old one fails, just as the waking child chooses. And an illustration of how sensations guide and determine movements, may be seen in another observation of the brainless frog, when, as often happens, it does not hit upon either of the plans just mentioned, but remains apparently restless and helpless; if under these circumstances we perform a part of the action for it, it will complete what we have begun: if we rub the irritated leg, at some distance from the spot where the acid is, with the foot of the other, the frog suddenly avails itself of this *guiding sensation*, and at once directs its foot to the irritated spot.

In these experiments on the triton and the frog, the evidence of sensation and volition is all the stronger, because the reactions produced by irritations are not uniform. If when a decapitated animal were stimulated it always reacted in *precisely the same way*, and never chose *new means* on the failure of the old, it would be conceivable to attribute the results to simple reflex-action—*i. e.*, the mechanical transference of an impulse along a prescribed path. It is possible so to conceive the breathing, or the swallowing mechanism: the impression may be directly reflected on certain groups of muscles. But I cannot conceive a machine suddenly striking out new methods, when the old methods fail. I cannot conceive a machine thrown into disorder when its accustomed actions fail, and in this disorder suddenly lighting upon an action likely to succeed, and continuing *that;* but I can conceive this to be done by an organism, for my own experience and observation of animals assures me that this is always the way new lines of action are adopted (see p. 209). And this which is observed of the sentient, conscious animal, I have just shown to be observed of the brainless animal; wherefore the conclusion is, that if ever the frog is sentient, if ever its actions are guided by sensation, they are so when its brain is removed. Ergo the Spinal Chord is a sentient centre.

Schrœder van der Kolk thinks that Pflüger was deceived in attributing sensation and volition to the frog, because the reflex-actions are, he says, so nicely adapted to their ends, that they are undistinguishable from voluntary actions. The mechanism is such that, by means of the communications established between various groups of cells, all these actions adapted to an end may be excited by every stimulus. But I deny the fact. I deny that all the actions are awakened by every stimulus. Only some few are awakened, and those are not always the same, nor do they follow the same order of succession. One decapitated frog does not behave exactly like another; does not behave exactly like himself on different occasions; but, on the contrary, exhibits great variety in his actions; and, above all, exhibits spontaneity and choice in his actions.

For a proper comparison of cerebral with spinal sensibility, the following experiment is well adapted: I divided the spinal chord of a triton as at Fig. 62.

Fig. 62.



Triton with Spinal Chord divided.

Had a physiologist been present, when the animal recovered, he would have noticed,. as a confirmation of the reflex-theory, that when the animal crawled, it dragged its hinder legs, and indeed the whole of the segment B, like a log after it; yet when the segment B was touched, the tail, or legs, moved. "Here," he would say,* "you perceive all voluntary movement in segment B is paralysed; it no longer responds to the mandates of the brain; yet its reflex-action is not

---

* Compare Flourens: *Recherches*, p. 50.

destroyed; prick it, and this segment, incompetent to obey the will, obeys the laws of reflex-action, and moves. All sensibility, you perceive, has departed from segment B. In vain you prick or burn it; the animal feels nothing, as you assure yourself by noticing the complete stillness of the segment A."

These arguments, which may be found in numberless treatises, seem victoriously conclusive, till we discover the fallacy involved in them. The cerebro-spinal axis is a compound centre: it is *one*, only in the sense that a government or army is one: it is the union of several individual centres. When that axis is divided, the effective unity is destroyed: instead of one centre, two centres are established; and these two, being no longer united, cannot act in union, but must act separately. The limbs of A will obey the volition of A, but not the volition of B, with which they are no longer united. Nor will the limbs of B obey the volition of A. There is no longer one seat of government, but two seats. There has been a "repeal of the union." Parliament sits in Dublin, as well as in Westminster.

Let us, with this new light, examine the phenomena. When we irritate the segment A, the animal crawls; but in crawling, it is only A that is active; B is *dragged* along, does not voluntarily move. Why should it move, unless it felt a stimulus? But we irritate segment B, which five minutes ago seemed so lifeless, so incapable of voluntary movement; and instantly the legs of B begin the crawling action, *while* A *remains perfectly motionless.* The body of the animal is moved sideways, and not forwards, because the quiescence of the head and shoulders renders forward motion impossible. Still the indifference of A to a stimulus applied to B, is as great as the indifference of B formerly was to the stimulus applied to A. If the tail-end was said to be deprived of voluntary motion and sensibility, because it was separated from the brain, and would not feel with or act with the fore-part of the body; so must we now say, that the fore-part of the body is deprived of voluntary motion and sensibility, since it will not act with, or feel with, the tail-end.

In fact, division of the cerebro-spinal axis is tantamount to a division of the animal into two halves; and each half obeys its own nerve-centres. The soldiers under Cromwell do not obey the orders of Rupert, nor do those under Rupert obey the orders of Cromwell. But the soldiers of each army obey their respective generals. It is thus with the triton whose chord has been divided: each half moves when irritated, and each moves independently of the other. The tail-end gives precisely the same indication of sensibility, whether united with the brain, or separated from it; and it gives precisely the same indication as the brain-end gives.

If the triton be watched for weeks, the truth of this will become irresistible. Although the animal is generally quiet, it moves spontaneously the segment A, and the segment B; *but never together.* If a lighted taper be brought near segment A, the fore-legs set to work, and the segment crawls, *dragging* B after it. If the taper be brought near B, the hind-legs set to work, and the segment moves sideways. If the tail be touched or gently pinched, it curls up; if the hind-leg be touched, it is raised in the air; but A feels nothing of this, and therefore does not move.

It should be observed that similar facts have long been known; but since the Reflex-Theory has been established they have been misinterpreted; and even before that epoch, the prejudice in favour of the Brain as the sole centre of volition, was so strong, that Mr Mayo could thus write:—"The physiological experiment of making separate nervous centres, by division of the spinal marrow, admits of explanation, on supposing the *principle of volition to continue for a short period* extended to the portions separated from the brain; a conjecture consistent with, perhaps, but far from established by, the very curious fact, that the convulsive movement of the leg of an animal thus circumstanced, when the sole of the foot is irritated, is accurately the gesture which the animal employs when, in undisputed possession of sensation, it retracts its limb from a similar aggression, and not, to appearance at least, a mere convulsive throe."* The explanation falls to the ground, when we learn that the same phenomena are exhibited weeks and months after division of the chord.

Among my notes of observation there is remarkable

---

* MAYO: *Anat. and Phys. Commentaries*, 1823, part ii. p. 19.

evidence of volition in the tail-half of a triton whose chord had been divided. It was lying, high and dry, in the wooden bowl in which it was usually kept, the small amount of water having almost entirely evaporated. I poured in a little water, slowly. In a few seconds after the water had reached the tail and one of the hind-feet, the whole segment B began to move, the legs were slowly raised, as for crawling, the back elevated, and thus the segment *sidled* into the water—the segment A remaining perfectly motionless all this time. The volitional character of this action was the more evident, because, had the act been purely reflex, excited by contact with the water, and not by a sensation, it would *always* occur when water touched the skin, whereas I had several times before added water, when the triton was lying high and dry, but never before did it seek the water with its hinder segment.

Once clearly apprehend the principle here expounded, namely, that division of the chord produces two independent centres, and all the recorded facts of experiment and pathology receive a simple explanation. Mr Grainger records the results of two experiments which deserve mention here. He removed a portion of the chord in a very young rabbit: "The peculiar fact noticed was that from time to time, *without any stimulus being applied*, the hind-legs were forcibly and repeatedly thrown back, *as if the animal were running quickly;* whilst at these times the fore-limbs, which were still under the control of the animal, remained motionless. These movements were so decided and so long continued, that it would have been difficult to have rejected the idea of sensation and volition still remaining in the hinder-part of the body, if"—and here the reader is particularly requested to follow the reasoning—" if it had not been distinctly observed that when the same animal attempted to walk it could only move its two fore-legs; by whose power the hinder ones and the trunk below the division of the chord, which were perfectly motionless, were dragged along the table." *

Surely the reader will agree with me that the facts by no means bear out this inference? If, as we have stated, division of the chord establishes two independent centres, we can no longer expect these to act in unison. When the hind-legs were agitated, the fore-legs were motionless; when the fore-legs moved, the hind-legs were motionless. After relating other similar experiments, Mr Grainger insists on the motionlessness of the hinder limbs, adding, "Now, as the very idea of volition implies a perfect control over any muscle which an animal may wish to stimulate, this utter want of such power is the most decisive proof which could be adduced to show that in this and similar cases the empire of the will was destroyed by the section of the chord." †

This decisive proof, however, rests, I think, on a misconception. The animal is spoken of as if its *organic integrity remained* after division of its chord;

---

* GRAINGER, p. 55.

† Compare also MARSHALL HALL: *Philos. Trans.*, 1833, p. 650, where a similar fact is adduced as decisive.

whereas, as we have seen, division of the chord is equivalent to cutting the animal in two; and no one would expect the muscles of the separated half to move in obedience to the brain from which it was separated. The body of an animal with a divided chord may still be one, inasmuch as it is nourished with the same food, and sustained by the same circulation; but the nervous mechanism is no longer one; and each half of the body is regulated by its own centres.

To prove this, I performed the following experiment. A triton, whose chord had been divided some weeks, was completely cut in two. The head-half immediately began crawling away with great activity; which, as this half contained the heart, and almost all the viscera, was not surprising. The tail-half remained for some time in a standing posture, and then began to crawl forward. After three steps it paused; remained quiet during five minutes, and began again, but feebly. The tail moved spontaneously, but with great slowness. When it was touched, both tail and legs moved. I then placed it under a glass, with a moistened sponge inside, to prevent evaporation from the skin, and left it there for two hours, watching its spontaneous, though very languid, movements.

If any reader remains still unconvinced, I can only recommend him to divide the spinal chord of a frog a little below the shoulders, and keep the animal for some days or weeks,* watching it, and testing its Sensibility. The evidences of spontaneous action, and of sensation, exhibited by both halves, the one no less than the other, will have more weight than pages of argument. Marshall Hall had observed some of these evidences, observed the decapitated animal defending itself variously against various irritants, but his *interpretation* of what he observed, is not, perhaps, the most convincing of inferences: "The *design* in all this is obvious," he says; "but is it design in the decapitated or divided animal? Certainly not; but of its omniscient Creator! It *coincides* with what would be design in the animal."* Now of two things, one: either *every* act in an animal must be the design of its Creator: in which case sensation and volition cannot endow the actions of the perfect animal with a design already endowed; or else the Creator has designed none of these actions, but only designed that a sensitive mechanism shall execute them: in which case the actions of the perfect and the decapitated animal being found to *coincide*—being, indeed, undistinguishable by us—what ground have we for assuming that they are not the results of similar causes?

---

* Care must be taken to prevent water getting to the chord, as that soon kills the animal.

This flying to design for refuge, merely to save an assumption, reminds me of objections which have sometimes been made in private. When I have narrated the experiments on the decapitated frog, I have been told that the frog whose foot had been amputated, and who therefore had recourse to *other* means to get rid of the acid, *felt* nothing: "He did it all from

---

* *New Memoir*, p. 31.

habit. It was *instinct.*" If frogs are in the *habit* of being irritated by drops of acetic acid, and are in the habit of having their feet amputated, which teaches them to seek other means of escape, all I can say is, that their education takes place in *other* ponds than those I have dredged. Schroeder van der Kolk refers to some experiments by Szokalski, on puppies and young rabbits, from which the brain—*i. e.*, the cerebrum, cerebellum, the corpora quadrigemina, optic thalami, and corpora striata—had been removed; these animals cried out when their tails were pinched, rubbed their noses when acids were applied, and made swallowing movements when bitters were placed in their mouths. "Szokalski thinks these were instinctive rather than reflex actions," says Schroeder, "but in this I cannot agree with him." What was meant by the word instinctive we are left to guess; but if sensation is excluded from instinct, and if sensation is excluded from actions such as those of the brainless puppy and rabbit, I know not *what* evidence of sensation can be entertained.

It is maintained by many writers that the actions of walking and swimming are reflex—due solely to the contact of the sole of the foot with the ground in walking, and the contact of the skin with the water in swimming. The impression of this contact is reflected on the muscles; but no sensation originates, or guides the actions. My experiments do not confirm this. When I touched the segment A (Fig. 61) of the triton, it dragged the segment B after it; but although in the dragging the feet of B were rubbed along the surface, they very rarely set going the walking actions: sometimes they did so; but contact was by no means sufficient to *excite* this action; whereas during their transit, while they were being dragged like paralysed limbs, a prick, or a pinch, immediately roused them into walking movements. I placed a frog, with divided chord, in a pan of water; the fore-legs immediately set going an energetic swimming action, but the hind-legs remained motionless, although they too were in contact with the water, and the impression ought to have been reflected on their muscles. I then decapitated another frog: on being thrown into the water it swam vigorously with both fore and hind legs. Presently it ceased swimming, and floated quietly on the surface; but on being touched again began swimming. This was repeated many times. It shows that mere contact of the water is not enough to excite swimming actions; whereas a sensation *is* enough. Claude Bernard found that a frog, whose skin had been removed from all its legs, swam as well as before. But when the whole skin had been removed, I found the frog *capable*, indeed, of swimming, since its power of muscular adjustment was unaffected, but making no attempt to swim.

These observations respecting the sensibility of the Spinal Chord may be closed with the following fact recorded by Volkmann. Young rabbits and puppies when taken from their mothers, manifest their discomfort by restless movements; the puppies also whine. No one doubts that sensation is present in such cases.

Now, if the brain be removed from rabbits and puppies, precisely similar phenomena are observed, when these young animals are taken from their mothers. "I observed the motions, which seemed the result of discomfort, quickly cease when I warmed the young rabbit by breathing on it. After a while it was completely at rest, and seemed sunk in deep sleep; occasionally, however, it moved one of its legs without any external stimulus having been applied, and this not spasmodically, but in the manner of a sleeping animal."* Is this cessation of the restlessness, when warmth is restored, not evidence of sensation? We see an infant restless, struggling, and squalling; and we believe that it is hungry, or that some other sensations agitate it; it is put to the breast, and its squalls subside; or a finger is placed in its mouth, and it sucks that, in a peaceful lull, for a few moments, to recommence squalling when the finger yields no satisfaction. If we accept these as signs of sensation, I do not see how we can deny such sensation to the brainless infant, or animal which will also cease to cry, and will suck the delusive finger!

VII. EVIDENCE AGAINST THE SENSIBILITY OF THE SPINAL CHORD.—Having at some length adduced the evidence which, to my mind, is conclusive in favour of the sensibility of the Spinal Chord, it is now necessary to examine the evidence which can be adduced against it. We have already seen that the main argument against it has been drawn from the universal preconception of the Brain being the exclusive sensorium; and we have also seen that this begs the question. Another argument has been drawn from the indisputable fact that many actions take place without awakening any distinct "consciousness" or attention—these are, breathing, winking, digesting, and many others. But we have seen, in the Chapter on FEELING and THINKING, that this proves nothing, or proves too much. An action may be sensational, without producing that secondary feeling, usually styled "consciousness;" and, in this sense, thinking may be proved unconscious—nay, sensations themselves may be so.

---

* VOLKMANN, quoted by PFLÜGER.

There remains only the striking fact of human patients with injured or diseased spines, feeling nothing below the injury. This fact we have more than once referred to as the *cheval de bataille* of the Reflex-Theory, which, we have also affirmed, turns out on close inspection to be the sorriest of Rozinantes. Let the fact be stated in all its force. A man with an injured spinal chord may lose all power of voluntary movement in the limbs below the injury; and also lose all power of feeling any impressions made on them. Pricks, burns, pinches, leave *him* unaffected. Hunter's celebrated patient, on being asked whether he *felt* the pain which caused his leg to kick out, answered, "No; but you see my leg does." And thus might all such patients reply. These limbs have not lost their power of movement, only their power of obeying the brain. They will not move when the patient endeavours to move them; but they are withdrawn, or convulsed, if pricked or tickled. Marshall Hall reports the case of a man in whom accident had destroyed all sensation and voluntary motion, yet who drew up his legs when they were tickled, without once feeling the sensation of tickling; and he drew them up when sprinkled with cold water, without feeling the slightest sensation of cold. This case is constantly cited, and is indeed very striking. It seems to have a far greater value than any experiments on animals can have, because we cannot question animals as to their sensations, we do not *know* whether they feel or not; we can only infer: whereas we can interrogate the human patient, and he distinctly *denies* being conscious of any impression whatever. "Do you feel this pin which is pricking you, and making your leg jerk?"—"No, I do not." Nothing seems less equivocal. We *must* accept his statement. Do I then doubt it? *Not in the least.*

Dr Baly, in the notes to his admirable translation of Müller's *Physiology*, records that he has a patient under his care attacked with complete loss of sensation and motion in the left upper and lower extremity, in whom, nevertheless, pinching or even slightly touching the sole of the foot or ankle of the paralysed leg, causes the limb to be retracted and the toes extended, the patient being unconscious both of the stimulus and the movement. When I first read this statement, having entire reliance on Dr Baly's accuracy in observing and reporting, it seemed to me to overthrow all the evidence on the other side; and this idea was further strengthened by the cases collected by Dr Budd.* It is unnecessary to occupy space here by citing these cases: they all point in the same direction, and we may therefore accept the conclusion, namely,—

That injury to the Spinal Chord wholly or partially destroys the power of obeying the Brain by voluntary movement, and the power of transmitting sensory impressions to the Brain, in the parts *below* the seat of injury; while in those *above* the seat of injury, sensation and voluntary motion remain.

Such is the conclusion rigorously deduced from numerous facts. I accept it, without reserve. But I shall now prove that it does not in the least affect the question under discussion, does not throw a shadow of doubt on the sensational and volitional character of the Spinal Chord. That it should ever have been thought to do so admits of easy explanation. Let us disclose the fallacy it involves.

On the supposition that the whole cerebro-spinal axis is everywhere the seat of Sensibility, it has already been shown that division of this axis would create two independent centres. In this case we have no right to suppose that the *cerebral* segment will be affected by impressions made on the *spinal* segment; nor, conversely, that impressions made on the cerebral segment will affect the spinal segment. The anterior limbs will obey the Brain, because they are in organic relation with it; but the posterior limbs *cannot* obey the Brain after they have ceased to be in organic relation with it. This has been fully explained (p. 249 *et seq.*)

---

* BUDD: in *Medico-Chirurg. Trans.*, xxii., cited by Dr CARPENTER.

Now, when a man has a diseased spinal chord, the seat of injury causes, for the time at least, a division of the cerebro-spinal axis into two independent centres. For all purposes of sensation and volition it is the same as if he were cut in half; his nervous mechanism *is* cut in half. How then can any cerebral volition be obeyed by his legs; how can any impression on his legs be felt by his cerebrum? As well might we expect the man whose arm has been amputated, to feel the incisions of the scalpel, when that limb is conveyed to the dissecting-table, as to feel in his brain impressions made upon parts wholly divorced from organic connection with the brain.

But, it may be objected, this is the very point urged. The man himself does not feel the impressions on his limbs when his spine has been injured; he is as insensible to them as to the dissection of his amputated arm. Very true. *He* does not feel it. But if the amputated arm were to strike the anatomist who began its dissection, if its fingers were to grasp the scalpel, and push it away, or with the thumb to rub off the acid irritating one of the fingers, I do not see how we could refuse to admit that the *arm* felt although the *man* did not. And this is the case with the extremities of a man whose spine is injured. *They* manifest every indication of sensibility. In the frog they manifest unmistakable volition. It is true that the man himself, when interrogated, declares that he feels nothing; the cerebral segment has attached to it organs of speech and expressive features, by which *its* sensations can be communicated to others; whereas the spinal segment has *no* such means of communicating *its* sensations; but those which it *has*, it *employs*. You can ask the cerebral segment a question, which can be heard, understood, and answered; this is not the case with the spinal segment; yet if you *test* its sensibility, the result is unequivocal. You cannot ask an animal whether it feels, but you can test its sensibility, and that test suffices.

In our examination of the triton and the frog, after division of their spinal chord, we saw that the limbs which could convey no impression to the brain, and could obey no impulse from the brain, exhibited the same evidences of sensibility as when they were in connection with the brain. Yet if the animal could have spoken, it would have assured us that it felt nothing in its hinder limbs. The limbs themselves told a different story.

The question we have to decide, therefore, is not whether a patient, with an injured spine, can feel impressions on, or convey voluntary movements to, limbs below the seat of injury—for as respects the nervous mechanism these limbs are separated from him, no less than if actual amputation had taken place—the question is, whether these separated limbs have any sensibility? And the answer seems to me unequivocally affirmative. I assert, therefore, that if there is ample evidence to show that the spinal centres have sensibility, when separated from the cerebral centres, such evidence can in no respect be weakened by the fact that a man with an injured spine is unconscious of impressions made below the seat of injury; since such a fact necessarily follows from the establishment of two centres: the parts above are not sensitive to impressions on the parts below; nor are the parts below sensitive to impressions on the parts above; but each segment is sensitive to its own impressions.

It should be borne in mind that although all vertebrate animals are constructed according to one general type, and all their organs have a certain general homology, the very great variations in their mechanisms will necessarily bring about corresponding variations in their functions. Hence, we may expect to see actions performed by one segment of a frog, which cannot be performed by the corresponding segment of man. And as a part of this variation in structure may be noticed the fact that in man the Spinal Chord is insignificant in comparison with the Brain, whereas in reptiles the Brain is less than the Chord. Anderson[*] estimates the weight of the triton's chord at one-fourth of a grain; that of the brain being one-seventh—showing a proportion of 100 to 180. From the figures given by Sharpey and Ellis,[†] it appears that while the weight of the human brain is 40 ounces as an average, that of the Spinal Chord is less than 2 ounces, sometimes only 1 ounce. The proportion is therefore 40 or 33 to 1. In animals the proportion is much less. In the mouse it is 4 to 1; in the pigeon, $3\frac{1}{3}$ to 1; in the triton, $\frac{5}{8}$ to 1, and in the lamprey, $\frac{1}{75}$ to 1.

---

[*] ANDERSON: *Comp. Anat. of Nervous System*, 21.

[†] QUAIN'S *Anat.*, 6th edit., ii. 435.

VIII. WHAT PART IS PLAYED BY THE SPINAL CHORD?—The reader may now ask what part is to be assigned to the Spinal Sensibility, supposing its existence granted? Do we think with our Spinal Chord? Is it a centre of Intelligence? "Why no, sir," as Dr Johnson would say.

Our previous investigations conducted us to the conclusion that the cerebrum was the centre of intelligence and emotion—at least in those animals which have a cerebrum. In the Amphioxus, which has no brain at all, the intelligence and emotion it may possibly have, must necessarily be performed through the agency of the Spinal Chord. Nor is there anything in such a supposition which is at variance with the principles laid down in these pages, however at variance with ordinary theories. For in the Amphioxus the spinal chord must be the centre in which the reflexes of sense are combined, as they are combined in the cerebrum of higher animals.

But quitting all such questions inaccessible except to inference, let us consider the probable share taken in our actions by the Spinal Chord. The intimate *consensus* which exists between every part of the vital mechanism renders it impossible very accurately to define the share of each. But if we look to the anatomical connections of the Spinal Chord, and assume that the Brain is the organ of intellectual action, we may consider that Spinal Sensibility regulates all those actions which are not set going by cerebral Sensibility.

A few illustrations will suffice. The brain of a sleeping man is supposed to be disconnected from all participation in the activity of the chord. Either the man is in profound sleep, and his brain is at rest, or he is dreaming, and his brain being cut off from almost all external stimuli, is occupied exclusively with its own excited activity—with its dreams. The man during this state breathes, swallows his saliva, coughs if any gets into the windpipe, and turns in his bed; the organic processes go on, and his brain has little if any share in all these various actions. From this sleeping man we remove the bed-clothes; the cold air soon produces a sensation of uneasiness; he turns and seeks a warmer spot; perhaps he stretches out his hand and pulls the clothes over him again. The evidence of sensation and volition here is unmistakable. If he did not *feel* the cold he would not move; if he did not *will* to move, and to pull the bed-clothes over him, he could not do it.

There are very cogent reasons for believing that in these cases the sensation and volition have their seat in the Spinal Chord, and not in the Brain. Firstly, the actions very closely resemble those of animals when the entire Brain has been removed. Secondly, brain-prompted actions—*i.e.*, those which spring from a distinct idea, or train of ideas—never seem possible in sleep. The sleeping man, who immediately moves his arm to rub his tickled cheek, or draws the bed-clothes when the cold air reaches him, or turns if he be touched, *cannot move a muscle when, in dreams, his mind is assailed with terror at danger*, or with desire to rescue his children. It is usual to account for this familiar fact by saying that in sleep "the will is suspended": a mere phrase, and a false one; for the will is not suspended when it permits the man to pull the bed-clothes over him, or to seek a warmer spot in the bed.

When the Brain is actively engaged in thought—as in profound meditation, or reverie—the volitions emanating from the Spinal Chord seem to suffice for all regular and continuous actions, such as walking or eating; or for the relief of particular discomforts, as when the tickling of a fly causes the hand to move towards the spot, or the position of the limbs is changed. We may walk through crowded streets absorbed in thought, yet every obstacle is avoided, and our destination reached with unerring accuracy. It is probable that from time to time the Brain interrupts its course of thought to glance at the houses, and assure us we are not in the wrong track; but although it is probable that, even during the profoundest meditation, the Brain does in general take cognisance of surrounding objects (for when it does *not* we miss our way), yet it seems tolerably certain that the guiding sensations of the Chord suffice during the intervals when the Brain is otherwise occupied. Indeed, men have been known to sleep on horseback, and soldiers have marched during sleep—facts which bear a close resemblance to the phenomena exhibited by decapitated animals.

Here, however, we are once more treading on the confines of Psychology, and must pause ere we get entangled in its labyrinths. If we have indicated in a general way the physiological relations of the MIND and the BRAIN, our object has been achieved. Nor will we here venture into an examination of the Sympathetic system, although we hold that here also the ganglia are centres of Sensibility. But the reader's attention will be more readily secured by the questions to be examined in our next chapter.

NOTE TO CHAPTER IX., SECTION III.

For the sake of any student who may desire to investigate the microscopic structure of nerve-centres, I will describe the best methods of preparation. Take a perfectly fresh brain or chord, and having removed its membranes, cut it in small sections, and harden them in alcohol, in chromic acid, or in bichromate of potass. Whichever solution is used should be very weak at first, and gradually strengthened. Lockhart Clarke's plan is to use a mixture of one part alcohol and three parts of water, for the first twenty-four hours; this is then thrown away, and replaced by equal parts of alcohol and water, for another twenty-four hours; and this again is replaced by pure alcohol. In ten or fourteen days it is ready. Stilling uses chromic acid; Kölliker, bichromate of potass, beginning with a solution of 1 per cent, and increasing it gradually to 4 per cent, constantly changing the solution. Clarke also now generally employs chromic acid, or bichromate of potass. He suggests a solution of ½ per cent of chromic acid for two or three weeks, followed by a solution of ½ or 1 per cent of bichromate. Both alcohol and chromic acid are excellent, and have different advantages; on the whole, I find it most advantageous, when bichromate of potass is used, to remove the sections, after three weeks, into strong alcohol, which better completes the hardening, and washes out the potass.

Having thus hardened the object, so that it is neither too soft nor too friable to permit of thin sections being made with a very sharp razor (for the spinal chords of small animals I use one of those small razors sold as corn-cutters), the upper surface of which must be previously wetted with alcohol, we have now to render the sections sufficiently transparent. This Kölliker effects by treating them first with soda (Bidder and his pupils use dilute sulphuric acid), then washing the soda out, and placing the section on a glass slide, he drops concentrated chloride of calcium on it, and covers with thin glass. This plan has never seemed to me so effective as the one employed by Lockhart Clarke. Schrœder van der Kolk hardens in alcohol, then placing a thin section on a glass slide, drops distilled water on it, and covers with thin glass, and commences a gentle pressure on the edges of the covering-glass, which pressure causes the water to rush between the fibres, and wash out the fat. When pressure no longer makes the water milky (after repeated changes), a drop of chloride of calcium is added; in six or seven days the section is clear. This also seems to me very inferior to the plan adopted by Clarke, who places the section in alcohol for a few minutes, according to its thickness, and then floats it on oil of turpentine. On the turpentine it rapidly becomes transparent. It is then removed to a glass slide, on which is a drop of Canada balsam, and allowed to remain, if not everywhere transparent; or if transparent, is covered with thin glass, and is ready for inspection.

The simplest of all plans is to harden the chord in alcohol, make a thin section, place it on a glass slide, add turpentine, and cover with thin glass; the turpentine penetrates slowly, and the section gradually clears; you may watch it day by day.

If the cells are to be exhibited, Gerlach's plan is to place the fresh chord, or the section just made from a hardened chord, in a very weak filtered solution of carmine, dissolved in weak ammonia: it is allowed to remain there a few minutes or a few hours, according to the strength of the solution. It must then be washed, and placed in alcohol.

By varying the plans, and giving the requisite patience, preparations may be produced of great distinctness; but the complexity of the structure renders it excessively difficult of comprehension, even with the best preparations.

CHAPTER X.

OUR SENSES AND SENSATIONS.

How many senses have we?—Definition of terms—New classification of the sensations—ORGANIC-SENSATIONS—The Muscular Sense: proofs of its existence; experiments to show that its seat is not the skin, but the muscles—Electrical disturbance causing nervous-action—SURFACE-SENSATIONS: various sensibilities of the skin—TOUCH: tactile corpuscles not organs of touch; how we localise all our sensations: all sensations referred to the surface; feelings in amputated limbs; some parts of the skin more sensitive than others—Feeling of "pins and needles" in the leg—TASTE: organ of; savoury substances must be soluble; dare we trust our instinct in taste? after-tastes—SMELL: organ of; are the olfactory nerves the nerves of smell?—Are they nerves at all?—Example of perfect sense of smell in the absence of the olfactory nerves—Odorous substances—Variations and uses of smell—HEARING: organ of; cases of perforated tympanum; varieties in sounds; a musical ear; effect of sounds on the emotions; subjective sounds; direction of sounds, and how discriminated—SIGHT: organ of—Are images transmitted to the brain?—Description of the Retina—Is it nervous?—It is insensible to Light; vision commences in a change of temperature—Subjective vision: curious examples—Single vision with two eyes: the paradox explained—Why are objects not seen inverted?—Colour-blindness—Psychological importance of the new view of the senses.

"How many senses have you?" inquired the traveller from Sirius, in Voltaire's exquisite satire; upon which the inhabitant of Saturn replied, "Seventy-two; but every day we live, we lament that we have so few."

The European has been taught to be so well satisfied with five senses, that he is apt to regard as an absurdity the attempt to alter, or enlarge that sacred number. Yet, if we look closely into the matter, we find that five is either too few, or too many: too few, if every distinct source of special sensations is to be called a Sense; too many, if only that is worthy to be called an organ of Sense which—as in the case of the eye or the ear—ministers to a single function, and yields only one group of special sensations. When it is said, "Man has only five senses," it is said that over and above the sensations of touch, sight, hearing, smell, and taste, he has nothing to be called sensation. This is manifestly wrong. "The division of our external senses," says Hutcheson, "into five common classes, is ridiculously imperfect. Some sensations, such as hunger and thirst, weariness and sickness, can be reduced to none of them; or if they are reduced to feelings, they are perceptions as different from the other ideas of touch—such as cold, heat, hardness, softness—as the ideas of taste or smell." *

Gerdy thinks the reduction of our senses to five, one of the most ridiculous of notions. "A sense," he says, "is nothing but a part of our body which gives different sensations from those of other parts. Is it not on the ground of such differences that men have admitted the five senses?" † Sir W. Hamilton also, after lay-

---

* Quoted by Sir W. HAMILTON: *Lectures on Metaphysics,* ii. 156. The language is loose enough, but the idea is intelligible.

† GERDY: *Physiologie Philosophique des Sensations,* p. 39.

ing down the proposition that all our senses are modifications of touch, or contact, adds "that if Sight and Hearing, if Smell and Taste, are to be divided from each other, and from Touch Proper, under Touch there must, on the same analogy, be distinguished a plurality of special senses."

It is impossible that there can be any philosophic agreement, unless we come to a distinct understanding of terms. We cannot leave undecided what is to be understood by the words "sensation" and "sense." It is clear that there is a great variety of sensations; but this does not imply a corresponding variety in our Senses. The same organ necessarily furnishes a great diversity of sensations, if it is capable of being acted on by a diversity of stimuli. Let us therefore settle our terms; since if the inquiry be, "How many distinct and special sensations have we?" the answer will not be five, but probably five-score; whereas, if the inquiry be, "How many distinct organs of sense have we?" the answer can only be settled by a profound anatomist.*

A sensation, properly so called, is the reaction of Sensibility, stimulated by an impression—the activity of a nerve-centre awakened by the Neurility of a nerve. This we have seen to be the rigorous definition. Those writers who complicate the idea of Sensation with the idea of Thought, and assert that there can be no sensation unless it be *recognised* as such by the sentient being, may equally accept our definition, since, according to them, Sensibility never *is* awakened by a stimulus unless the brain be also affected.

---

* On this, as on so many other subjects, the philosophical student is referred to the important work by VICTOR CARUS: *System der thierischen Morphologie;* see also GEGENBAUR: *Grundzüge der Vergleichende Anatomie;* and BERGMANN *und* LEUCKART: *Vergleichende Anatomie,* or the course of development of the senses in the animal series.

The stimulus may be external, or internal. It may arise from contact with agents outside, or from agents inside the body: the pressure of a pin on a sensitive surface, the chemical changes going on in the substance of an organ, or the pressure of a blood-vessel, may equally produce sensations, but the sensations produced will not be similar. For while, on the one hand, the *kind* of sensation is determined by the nature and degree of the stimulus, it is, on the other hand, determined by the peculiar structure and organic disposition of the part affected.* The same degree of temperature which, acting on a nerve through the skin, will produce the sensation of cold, will produce no such sensation but one of *pain*, if it act *directly* on this nerve; and I have found, as previously mentioned, that the same nerve which in the frog is sensitive to pricks or cuts through the skin, is not sensitive to them *under* the skin. The ray of light which, falling on the optic *apparatus*, produces a well-known sensation, produces nothing of the kind if it fall on the optic *nerve*, or any other nerve. The wave of air which produces the sensation of sound through the auditory apparatus, produces little appreciable effect on any other part. The sugar which is sweet to the tongue, is merely rough to the gums. Thus each organ of sense *determines* its specific sensations: a blow on the arm causes pain, on the eye a flash of light, on the ear a sound. Electricity causes alkaline, or acid, tastes, phosphoric odours, buzzing sounds, and flashes of light, if applied to tongue, nose, ear, and eye. *One* stimulus is thus transformed into various sensations, according as it acts through various channels.

---

* Compare VICTOR CARUS: *Morphologie*, pp. 61, 62.

Sensibility is the general property of every animal organism. This general property becomes specialised in proportion as the organism itself becomes more special and complex in structure.* Although every animal must feel, it does not follow that every animal must have every *kind* of feeling. The mollusc feels, but it is extremely doubtful whether it feels pain, and still more so whether it feels tickling. Its sense of sight is probably little more than a discrimination of light from darkness; its sense of hearing little more than a susceptibility to vibrations of the air. Nay, even to pass on to higher animals, the cat undoubtedly distinguishes sounds, and does not confound the cry of the cat's-meat-man with that of the dustman; but its susceptibility to musical intervals will be very doubtful to those who have "assisted" at gutter-concerts. And among men, there are those who cannot distinguish the colours red and blue; others who have no distinct appreciation of odours and savours; others who can scarcely follow a melody; others who are very ticklish, or not at all so; others exquisitely sensitive to pain, and others very much less so. All these *specialities* of the general Sensibility depend on specialities of the nervous system and the organs of sense.

---

* On the development of psychical complexity, see the remarkable work by HERBERT SPENCER: *Principles of Psychology*.

The precise number of our senses is a question not easily answered. If every distinct part of the organism, which is the source of distinct sensations, is to be called a Sense, we must necessarily include the Muscles and Viscera among the Senses, for the sensations derived through the muscles—sensations of adjustment, weariness, cramp, &c.—are as *specific* as those derived through the eye or tongue; and the glandular sensations are assuredly distinct from those of the muscles.

The following is the classification which seems to me desirable, after what has been said respecting the various forms of consciousness (see p. 65-77). All those sensations which arise in the organic processes, or which, belonging to the general system, are not localised in any special organ, I call *systemic sensations;* whereas those, on the contrary, which are localised in special organs—the eye, the ear, the nose, the tongue, and the hand—I call *sense-sensations.* The former only tell us of our own internal condition—they are the subjective senses.* The latter give us conceptions of things external to ourselves—they are the objective senses. Although the food which stimulates the alimentary canal, or the air which stimulates the lungs, are objects not less *external* than the colour or the odour of a rose, we do not habitually refer the sensations they produce to distinct cognisable objects, as in the case of the colour and odour. We have great difficulty in localising the sensations, and never succeed in doing so with any precision; whereas we are in no doubt that the sensation of colour is derived through the eye, and nowhere else.

---

* KANT'S classification into *sensus vagus* and *sensus fixus* corresponds pretty nearly with the above.

Systemic sensations may be divided into two classes: *Organic* and *Surface Sensations;* these we will now consider.

I. ORGANIC-SENSATIONS.—That we receive sensations from the varying states of our organs, not less than from the varying impressions of objects on our Senses, is abundantly manifest. The agitation of the heart, the pains or pleasures of the alimentary canal, the feelings of fatigue, sleepiness, hunger, thirst, and the various feelings attendant on maternity, are obviously of organic origin. It would doubtless create some surprise were we to speak of a Respiratory Sense; yet there would be more warrant for such a term than for the Muscular Sense, which has gained general acceptance. Indeed Mr Bain, who has treated this topic of the organic sensations with great fulness and felicity,* properly remarks that we have in respiration all the particulars necessary to constitute a Sense: an *external object*—the air of the atmosphere; an *organ;* and a resulting *state of feeling.* This cannot be said of the Muscular Sense, which has no external object. No one will doubt that we receive distinct sensations from the purity, or closeness, of the atmosphere; we cannot draw breath without exciting a sensation, although in general we are not "aware" of it. In like manner the sensations of the alimentary canal generally pass unperceived by us; but they go to form that stream of Consciousness which I have before noticed.

---

* BAIN: *The Senses and the Intellect.*

Referring the reader to Mr Bain's work for further details on this subject, I will now pass to the very important class of sensations arising from the condition of the muscles.

1. *The Muscular Sense.*—In his erudite dissertations affixed to Reid's Works, Sir William Hamilton has sketched the history of the conception, revived in our own day by Sir Charles Bell, of a distinct Muscular Sense. Whether it is legitimate or not to elevate this into the rank of a distinct Sense—a rank denied to the glands and alimentary canal—may be questionable, and is, indeed, often questioned; but there has long been general unanimity as to the fact that the muscles are the sources of peculiar sensations, such as those of exercise, weariness, cramp, &c. It has also been admitted that the *adjustments* necessary for all movements for walking, riding, dancing, sitting upright, and so forth, are dependent on the sensitiveness of the muscles. The body is balanced by an incessant shifting of the muscles, one group antagonising the other. But this would be impossible unless each muscle were adjusted and co-ordinated by sensation.

"When a blind man, or a man blindfolded, stands upright, neither leaning upon nor touching aught; by what means does he maintain his erect position? The symmetry of his body is not the cause. A statue of the finest proportion must be soldered to its pedestal, or the wind will cast it down. How is it, then, that a man sustains the perpendicular posture, or inclines in the due degree towards the wind that blows upon him? It is obvious that he has a sense by which he knows the inclination of his body; and that he has a ready aptitude to adjust the parts of it, so as to correct any deviation from the perpendicular. What sense is this? He touches nothing, sees nothing; it can only be by the adjustment of the muscles that the limbs are stiffened, the body firmly balanced and kept erect. In truth, we stand by so fine an exercise of this power, and the muscles from habit are directed with so much precision, and with an effort so slight, that we do not know how we stand. But if we attempt to walk on a narrow ledge, or rest in a situation where we are in danger of falling, or balance on one foot, we become subject to apprehension; and the actions of the muscles are then, as it were, magnified, and demonstrative of the degree in which they are excited."*

If we watch an infant learning to walk, or attend to our own sensations when learning some new muscular movement, we shall become aware of the necessity of a very fine sensibility for all these adjustments. It is only by means of guiding sensations that we can perform such actions. All this was admitted by physiologists; but there still remained the question whether the sensations derived in muscular contraction could properly be referred to the muscles. In his admirable treatise on Physiology, Schiff maintains that all the phenomena attributed to the muscular sense are due to the foldings and stretchings of the *skin* when the muscles contract. Spiess and Schrœder van der Kolk hold similar views. Indeed when we learn that the posterior, or so-called *sensory* nerves, traverse, but do not terminate in muscles, we are in a dilemma if we grant the muscles to be the channels of sensation; since they are supplied only with anterior, that is, the so-called *motor* nerves. How can these motor-nerves convey sensory stimuli? This difficulty does not seem to have been generally recognised. It is only to be escaped by denying that the muscles are channels of sensation; or by denying that the so-called motor-nerves are not *also* sensory nerves. Schiff rejects the muscular sense, and attributes to the *skin* all the phenomena grouped under this sense. The reader is aware that I deny the so-called motor-nerves to be *exclusively* motor; and one proof on which I rely is the proof of a muscular sense.

---

* Bell: *On the Hand*, p. 238; 5th edition.

In a paper read at the Aberdeen Meeting of the British Association,* I adduced the following decisive experiments, which, if we admit that sensations are necessary to muscular adjustments, leave no doubt that the muscles and muscle-nerves are the sources of such sensations. I etherised a frog, and then carefully removed the whole of its skin, except a very small patch about the end of the back, and another patch over mouth, nose, and eyes. These patches were left for comparison. If Schiff's views were correct, —if all the phenomena assigned to a muscular sense were due to foldings and stretchings of the skin, it was obvious that this frog could manifest none of these phenomena, having no skin to be folded or stretched. What was the fact? Why, that no sooner had the frog recovered from the effects of the ether, than it hopped off the table on to the ground. I then tested its sensibility. Those patches, where the skin remained, were as sensitive as ever; a touch there, made the frog hop, or shrink, or draw up its legs in defence. If its nose was touched with acetic acid, the fore-legs were used to rub the acid away; showing that it had the same power of adjusting its muscles as when the skin was on. But this was no absolute proof that the frog had any sensation derived through these muscles; and to obviate all objection I sought for such proof. It is to be observed that, as regards external impressions, the frog was utterly insensible. I pinched the limbs, pricked them, cut them, burnt them with acetic acid, and reduced them to cinders with the flame of a wax taper — and to all these violent stimuli the frog remained insensible, motionless,[*] although a touch on the skin-patches made it hop or wince. Here then was an animal which, if it had sensations at all, could only have them from internal stimuli, except on two minute patches of skin. Yet when it was placed on its back, it immediately turned round again, and settled in a comfortable position. Lest this change of position should be attributed to the "will" of the animal, whose brain might be thought to have originated the act, I cut its head off; the headless frog, placed on its back, always turned round as before. The position was obviously uncomfortable, and was altered. The hind-legs were then drawn out: if drawn abruptly, they were abruptly drawn up again; if drawn very gently, they remained motionless where they were placed; *but after a few minutes the legs were always drawn up again.* To what can we attribute this change of position but to a sense of muscular discomfort? Had the muscles been insensible, the legs would have remained motionless till the animal was about to spring; but that they were sensitive is shown in the fact that, after a while, the legs always were drawn back to the normal position; and this was observed to take place in a shorter time if the outstretched leg was at right angles to the body than if it were straight out. That is to say, the more *unusual* the position, the swifter was the withdrawal.

---

* *A Demonstration of the Muscular Sense.*

---

[*] Whenever I adduce experimental evidence, the reader will be pleased to remember that, unless the contrary is stated, I am always speaking of facts repeatedly observed, and controlled by comparative experiments. What is said in the text respecting the insensibility of the skinned frog is exact; it has been observed in many cases; but there is *one* strange exception recorded in my note-book which deserve

---

notice here. "Found traces of sensibility in *some* of the skinless parts. Pinching the joints or the soles of the feet, and almost anywhere on the fore-leg, made the frog struggle or jump. Acetic acid had *no* such effect. Pricking seldom had any. Back quite insensible to all stimuli. Muscles of the thigh insensible to burns or pricks, but sensitive to a prolonged pinch with the tweezers." This is wholly inexplicable to me; but the fact is worth considering.

We have thus a skinned and headless frog manifesting many unequivocal signs of those sensations which have been assigned to the conditions of the muscles; and are justified in considering this experiment as demonstrating the existence of a muscular sense. And a striking confirmation of the original conception, that a muscular sense is necessary for the adjustments on which combined actions depend, is to be found in a case recorded by Brown-Séquard. Sir Charles Bell refers to a woman who, having lost the muscular sensibility in her arm, could only hold her infant so long as she *looked* at her arm; directly she averted her eyes, the loss of this *guiding sensation* made her grasp loosen. On this Brown-Séquard remarks: "It may be said that the sensibility of the skin being lost in this case, the impossibility of holding the child arose from this cutaneous anæsthesia. There is a decisive reply to this objection: it is, that muscular sensibility alone is sufficient for the direction of voluntary movements. I have seen a child completely deprived of cutaneous sensibility (unable to feel contact, pressure, pricking, pinching, tickling, cold, or heat), yet able to walk well without looking at its feet, and undoubtedly owing this power to the persistence of guiding sensations in the muscles. In this case, besides the peculiar sensibility which guides voluntary movements, the muscles had the power of giving pain. When they were excited to contract spasmodically, the patient had the feeling of cramps."*

---

* BROWN-SÉQUARD: *Lectures in Lancet*, Oct. 16, 1858.

In singing, a wonderful adjustment of the muscles takes place; and although the direction impressed on these is mainly due to the sense of hearing, this is no more than the direction of our locomotive movements impressed by sight. Dr Kitto, in his interesting autobiographical sketch,* narrates how difficult, and even painful, it was for him to speak, on first recovering from the accident which made him deaf. But he subsequently learned to speak, and this was of course entirely due to the muscular sensations in his vocal organs. In the same way, a man blindfolded, or suddenly blinded, finds great difficulty in walking straight ahead; the loss of his eyesight is the loss of the accustomed guiding sensation; yet in a little while his muscular sense suffices to guide his steps. Mr Mayo's energetic phrase, "We lean upon our eyesight as upon crutches," is inaccurate. We derive no power of walking from sight, we only ascertain our course by it. Eyesight is a finger-post, not a crutch.

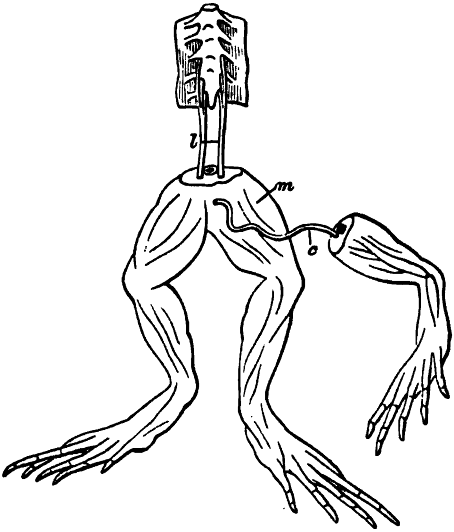
Having made clear to ourselves that we have a distinct class of muscular sensations, we shall at once recognise the important element they must form in our general Consciousness, little as they ever emerge into that prominence which causes the mind to attend to them. We shall also recognise the sensory function of the motor-nerves (see p. 29 *et seq.*) And now an interesting problem presents itself, namely, how the contractions of a muscle can awaken sensation. In the ordinary conception of sensation, it is implied that some external object should impress a nerve, and this impression, transmitted to the brain, will produce a sensation. This conception renders internal sensations difficult of explanation. But a more accurate survey of the facts leads to the conclusion that the mere *disturbance of the electrical equilibrium* in an organ is sufficient to excite a sensation.

---

* KITTO: *The Lost Senses*: *Deafness.*

Let us dissect out the lumbar nerves of a frog (*l*), and, taking the leg of another frog with its nerve dissected out, let us place this nerve (*c*) on the muscles of the first (*m*). When the two poles of a battery are applied to the lumbar nerve (*l*), the legs (*m*) are violently contracted, and this contraction so affects the

Fig. 63.



nerve of the leg (*c*) lying on the contracted muscles, that *this leg also contracts.* Here, then, the mere contraction of a muscle, stimulates another nerve in contact with it. Now, it is indifferent to our present argument what explanation we accept of this fact; the fact that a *change in the state* of the muscle will excite the Neurility of a nerve, is enough for us.[*] And it is extremely important to bear in mind that not only will a muscle in contraction excite any nerve with which it may be in contact, but it will do so if, without contracting, it even *tends* to contract.

By disturbances of the electrical equilibrium the Neurility of nerves is excited. Hence it is that the manifold chemical changes going on—the processes of nutrition and secretion—the oscillations of the circulation and the activities of the muscles, create those manifold streams of sensation which make up our general Consciousness. Hence may we deduce the exaltation of sensibility in delicate states of health; and the marked difference in the nervous excitability of frogs before and after the pairing time. But we dare not venture into so wide a field. It is enough to have indicated the existence of a vast class of organic sensations; and we will now pass on to the

II. SURFACE-SENSATIONS.—These are more appreciable than the Organic sensations, because we can to some extent measure the effects produced by external objects on the surface. We do not know what is going on in our organs, we do not know what agents are at work; but we *can* ascertain what agents are stimulating the surface, with what variations of degree these agents affect us, and in what places they affect us.

---

[*] Will not this explain the paradox of parts wholly destitute of nerves being nevertheless susceptible of intense pain? The altered *state* of a tendon may propagate an influence to the muscles, and these may excite the sensory nerves in their neighbourhood.

The skin is everywhere sensitive. It is everywhere supplied with nervous filaments; and these filaments seem more sensitive in the skin than in the trunks from which they issue. Not only are they more *variously* sensitive—capable of more distinct sensations—but they are susceptible to a greater *amount* of excitement. For example, it is through the skin, and only through the skin (and the mucous membrane, which is the internal skin) that we receive sensations of Temperature. Application of a hot or cold substance to the nerve-trunk itself, produces no sensation of heat or cold, but one of pain. It is the same with itching, tickling, and some other surface-sensations. Nevertheless it is not the skin which is sensitive; destroy the nerve-filaments, or cut off their communication with the nerve-centre, and the skin is insensible.

Further, the intensity of sensation is much greater in the skin than in the nerve-trunk. Hence the painfulness of an operation chiefly depends on the division of the filaments at the surface, and not on the division of the nerve-trunks when the muscles are cut through. Arnold, Volkmann, Weber, and others, have abundantly illustrated the superior sensitiveness of the nerve in its distribution through the skin, to that of the same nerve in any other part of its course.

But although everywhere sensitive, the skin is not everywhere equally susceptible to the same kind of impression. A specialisation has taken place. The face is more sensitive than the hand to Temperature; the sole of the foot is more sensitive than the back to Tickling; the tips of the fingers are more sensitive than the palm of the hand to Pain, and less to Tickling. In cold weather we see men beating their hands together, or clapping their sides, to stir a little warmth; but they never think of slapping their faces with this energy. The schoolboy has often to hold out the palm of his hand for a stroke with the cane, which would be intolerable on the back of his hand. It was an easy supposition that this varying sensitiveness might be due to the varying amount of nervous filaments distributed over each part; but the supposition is without the basis of anatomical accuracy. Valentin says that, although the filaments at the apex of the tongue are more numerous than those on the skin of the back, they bear no corresponding proportion to the respective sensibilities of these parts—the sensibility of the tongue being fifty to sixty times greater than that of the back.* Moreover, the part which is eminently sensitive to one kind of stimulus is scarcely at all so to another.

If we bring a portion of this sensitive surface in contact with an external substance, the result will be a sensation of *touch*. On pressing that substance we have the sensation of *pressure*, or of *pain*. If the substance be lower in temperature, the result is *cold*; if higher, *warmth*; if the difference of temperature be considerable, the result is *pain*. Contact will also give the sensation of *smoothness* or *roughness*, if the object moving over the skin be of even or uneven surface.

---

* VALENTIN: *Textbook of Physiology* (translated by BRINTON), p. 495.

Here we may notice a cardinal distinction between the skin, and the organs of special Sense. The same portion of skin will respond differently to different stimuli, or degrees of the same stimulus; but each organ of Sense can respond only in *one* way, and produce only one specific group of sensations. Thus, while the skin will give us sensations so distinct as those of pain, tickling, temperature, or mere touch, the eye will give us only sensations of light, the ear only sensations of sound, and so on, no matter how various the stimuli may be. Hence the propriety of separating these *special* organs of Sense from the general surface, although, strictly speaking, they also are parts of the general surface; and indeed the first of these organs, the Hand, has a very slight degree of specialisation, and shares with the whole surface its function of Touch.

III. SENSE-SENSATIONS.—These are derived from our Five Senses. They have five special organs devoted to them, if we include, as, for the sake of following popular conceptions, it is necessary to include, the Hand as a separate organ of Touch. We must remember, however, that, properly speaking, Touch is not localised in any one special part of the surface. We can touch an object as well with the foot as with the hand, with the nose as with the finger, with the tongue as with the knee. The hand, by reason of its flexibility, enables us to feel an object rapidly in various ways; but this is its sole speciality. The sensations it conveys are not different from those conveyed by any other part of the skin; nor are they superior in intensity and variety. The hand is notably inferior in delicacy of tact to the tip of the tongue. Had not exact experiments demonstrated this, we should at once admit its truth, on recollecting how, when touched by the tongue, a cavity in a tooth seems to be thrice as large as it really is; because, being accustomed to estimate size by sensations of touch from the hand, we are misled, by the greater sensitiveness of the tongue, into the belief that the cavity is as large as it would be if the finger had given us such sensations.

1. *Touch.*—The organ of Touch, therefore, although really coextensive with the whole surface, we may, for convenience, localise in the Hand. I shall say nothing of this wonderful organ here, but refer the reader to Bell's delightful and instructive work, wherein everything that was then known will be found elucidated. Nor is it necessary to do more than allude in passing to the so-called "tactile corpuscles" discovered by Meissner, and too readily admitted as "organs of touch" by many physiologists. These corpuscles are microscopic structures found in the papillæ of the skin

on the sole of the foot, palm of the hand, and occasionally in the lips and tongue. There is still great dispute as to the nature of these structures;[*] and my own very limited acquaintance with them does not enable me to express a preference for any one opinion; but I feel quite certain that they are *not* tactile organs, because the sensations of touch are as various and delicate in parts without these organs, as in parts with them.[†]

Instead, therefore, of saying anything about the organ, we will devote our brief space to the sensations of Touch. The first question which meets us is, How we *localise* our separate sensations, since the whole of the surface is sensitive, and sensitive to nearly all the same stimuli?

Every one knows that we feel an impression in the sensorium, to which the nerve conducts the impression; nevertheless, we *refer* this impression to a distinct spot on the skin, and not indiscriminately to any spot. How does this localisation take place? How is it that sensations really felt in the centre should irresistibly be referred to the surface, so that to an uninstructed audience it would sound absurd to tell them their fingers did not feel the object which they touched?

---

[*] MEISSNER: *Beiträge zur Anat. und Physiol. der Haut;* and in *Siebold und Kölliker's Zeitschrift*, vi. 296; KÖLLIKER in the same Journal, iv. 43; and *Gewebelehre*, 1859, p. 106; HUXLEY in *Quarterly Journal of Micros. Science*, Oct. 1853, p. 1; FUNKE: *Lehrbuch der Physiol.* i. 580. LEYDIG in *Müller's Archiv*, 1856, p. 150, and *Lehrbuch der Histologie*, p. 68.

[†] SCHIFF is also of the same opinion. He remarks that the hands of the squirrel are without these corpuscles.

The young infant has no such tendency to localise impressions. It feels the sensation of a pin pricking its legs, or the lancet lancing its gums, or the fire burning its finger; but has not learned to refer these sensations to special localities. It has not yet conceived the idea of externality.

"The baby new to earth and sky,
What time his tender palm is prest
Against the circle of the breast,
Has never thought that 'this is I.'

But as he grows, he gathers much,
And learns the use of 'I' and 'me,'
And finds 'I am not what I see,
And other than the things I touch.'

So rounds he to a separate mind,
From whence clear memory may begin,
As through the frame that binds him in,
His isolation grows defined."*

The infant world is wholly subjective. To conceive an external, objective cause of its sensations, would be more difficult for the infant, than it would be in after life to *dissociate* the external object from the sensation, and to conceive sensations as nothing but states of the organism. Only the psychologist can conceive that heat is not in the fire, hardness in stone, or sound in the thunderclap. We begin life with a complete ignorance of the external world; as we advance, the presence of this external world becomes more and more obtrusive, till we need the aid of philosophy to reinstate the vanished conception of our inner life.

---

* *In Memoriam.*

The growth of the tendency to refer all sensations from within outwards, and to localise them in particular spots, depends on two causes: First, there is a difference in the sensations excited by impressions on different parts—those on the toe being somewhat unlike those on the finger, and both unlike those on the back or face. These differences the infant soon learns to distinguish, and learns to connect them with its experience of external bodies,—which is the second cause in operation. The infant incessantly moving its arms and legs, brings them in contact with objects, and learns to connect the two facts—contact and sensation—by which a dim conception of an external is reached. The same object is found to produce different impressions on hands and feet—and in time the recognition of each sensation is inseparable from a recollection of the part affected.

It is because we thus learn to connect sensations with external objects, owing to the incessant contact of the surface with various objects, that even the sensations of our viscera are, so to speak, *projected* from the nervous centres, and conceived as external to the sentient organism. When the optic nerve is pressed, a flash of light appears *before* the eye—that is to say, there where light appears under ordinary circumstances. When the ulnar nerve is pressed—at what is popularly called the "funny bone"—a pricking sensation is felt in the fingers. When the auditory nerve is pressed, sounds are heard which seem to come from without, like all other sounds. So indissoluble is the connection of sensation with some distant spot on the surface, that after an arm, or a leg, has been amputated, the patient constantly *feels* sensations in the lost fingers and toes. In vain experience contradicts the sensation; in vain he sees that his fingers and toes are not there to feel; he feels them as distinctly as ever he felt them when they were parts of his living body. Nay, so urgent is this conviction at times, that men have actually had the feet cut off because of the pain felt there; and the pain still continuing after the feet were removed, they have had the leg removed from the knee; this not succeeding, they have had the hip joint removed. Here the seat of injury was not in the foot, but the sensation was *referred* to the foot. Schiff tells of a man whose leg had been cut off in childhood, and who felt pains in the toes of that foot during the whole of his subsequent life: at times he would forget his crutch, spring up from the sofa, as if both legs were at command, and only be aware of his lameness when he was prostrate on the ground.

Long after we have learned to refer all sensations to the surface, we have but an indistinct conception of the exact spot on that surface where the impression is made; and throughout life we are totally unable to refer with any accuracy to the particular portions of the viscera, back, neck, and legs, which are affected; whereas the hands, feet, tongue, and face admit of marvellous nicety in this respect.

Weber measured the relative sensibility of various parts of the surface by means of a pair of compasses the points of which were tipped with cork. He noted the various degrees of distance at which the two points could produce two distinct impressions, when the patient's eyes were bandaged. These measurements show that the tip of the tongue can distinguish two impressions when the compass-points are only half a line* apart; the tip of the finger when they are one line apart. Other spots vary still more widely; for example, the distance of the lips is 2 lines, the tip of the nose 3 lines, the cheek 5 lines, palm of the hand 5 lines, forehead 10 lines, back of the hand 14 lines, chest 20 lines, back and thigh 30 lines.

Sir William Hamilton remarks,† that if this experiment be repeated with a pair of compasses capable by slight pressure of exciting a sensation of pricking or scratching, it will be found that there is no corresponding difference between the parts in their sensibility to pain. On the contrary, in places where the sense of touch is most alive, the sense of pain is in the first instance at least deadened, and the parts most obtuse in discriminating the duplicity of the touching points are by no means the least acute to the sensation excited by their pressure. The tip of the tongue has fifty times the tactile discrimination of the arm; but the arm is more sensitive to a sharp point applied, but not strongly, to the skin, than either the tongue or the finger, and at least as alive to the presence of a very light body, a hair, or feather, drawn along the surface.

---

* A line is the 12th of an inch.

† HAMILTON: *Dissertations* in REID'S *Works*, p. 863.

How much the degree and kind of sensation in various parts will depend on the nature of the epidermis covering those parts, may be gathered from Weber's experiments on patients who had lost a portion of the epidermis by burns. The sense of *touch* was completely destroyed in those parts; the sense of *temperature* was so imperfect that no discrimination was possible between substances differing so widely in temperature as 48° and 113°: the patient sometimes declared he was touched with the colder substance, when in truth it was the warmer, and *vice versâ*. On another occasion, when the substance was still higher in temperature, no sensation of warmth was felt, but one of pain.

Curiously enough, the right hand, which is more sensitive to Touch than the left, is less sensitive to Temperature. If the two hands be dipped in two basins of water at the same temperature, the left hand will feel the greater sensation of warmth; nay, it will do this even when thermometers show that the water in the left basin is really somewhat colder than that in the right basin. I suspect that with "left-handed" persons the reverse would be found.

Having established these two points—first, that all sensations are referred to the surface; and, secondly, that various parts of the surface differ in their degrees of Sensibility—we shall now be able to understand how it is that sensations come to be accurately localised.

Touch a cold object with the foot, with the hand, with the arm, or with the cheek, and sensations of different quality will be felt, which experience readily recalls as arising from those different parts. That it is experience recalling the past, and enabled to do so by the multiplicity of similar sensations in the past, is evident from this, that only those portions of the body which are habitually employed in such service are capable of rendering *exact* local references. Thus, let an insect sting us on the back, neck, arm, or leg, and we cannot, without the aid of some other guiding sensation, place our fingers on the spot, but only *near* it; whereas on the face, tongue, or hand we can unerringly find out the spot at once. If the reader will rub his finger gently over the surface of his hand, he will find a succession of sensations, all slightly different, arising in each portion of the rubbed surface: let him do the same with his arm, and he will find the sensation is almost continuous. These differences are registered in the mind; and it is because different spots yield different sensations that we can unerringly refer each sensation to its particular spot. And thus we may explain why it is that a sensation of temperature cannot be excited by the application of a hot or cold body to the *trunk* of a nerve, although the same application to the filaments of that nerve, distributed through the skin, excites the sensation: our experience of Temperature is indissolubly connected with the *peculiar* sensations excited in the skin, and *these* cannot of course be excited where there is no skin.

Before quitting this subject, it may not be uninteresting to notice the familiar phenomenon which succeeds pressure on the nerve-trunk of the arm or leg, and is popularly called "pins and needles," or "leg asleep." It arises thus: By pressure, for a certain length of time, the sensibility of the nerve is greatly blunted.* When this pressure is removed suddenly, the sensibility will *gradually* be revived: as each nerve-fibre, composing the trunk, returns to its *normal* condition of sensibility, a pricking sensation is felt, and the successive prickings from the successive awakenings of the numerous fibres cause the "pins and needles."

There are many other points connected with the Sense of Touch, but they belong properly to the province of psychology, and we shall not treat of them here, having to consider the other surface-sensations.

2. *The Sense of Taste.*—This is indubitably a *special* sense. It has a special apparatus, and only in this apparatus can the sensations known as those of Taste be excited by savoury bodies, and only *these* sensations can be excited in it by electrical or mechanical stimuli. Instead, therefore, of seeking for the source of these sensations in the general surface of the body, we have to seek it in a particular part of the surface. What is that part? Ordinary usage has assigned to the tongue alone the function of Taste;

---

* The phrase "sensibility of the nerve" is used in the text in its popular acceptation. The reader is aware that I admit sensibility only in nerve-centres.

and I think ordinary usage is here physiologically correct, in spite of the disputes which have been, and still are, kept up by various experimenters.

Müller, for example, maintains that the back part of the mouth (*fauces*) is the seat of this sense, which is, however, more especially seated in the tongue. Valentin thinks the whole lining membrane of the mouth, as well as the upper and under surfaces of the tongue, are the seat of this sense. And as these are two of the most eminent names in modern science, a great weight necessarily attaches itself to their opinions. The subject is one, however, in which experiment is apt to be delusive, unless excessive caution be employed, because when a savoury body is brought in contact with any part of the mouth, the dissolved particles are diffused by the saliva through the mouth, and quickly come in contact with the nerves of Taste. If, however, a sweet or bitter substance be rubbed against the gums, the palate, or the under part of the tongue—care being taken that the tongue does not touch gums or palate—no sensation of sweetness or bitterness will *at first* be felt. Soon afterwards a sensation is felt, but that arises from the diffusion of the savoury particles. If the tongue be allowed to touch the spot, the sensation will be instantaneous. This experiment is easily performed, and its results are uniform.*

For a substance to be tasted, it must either be in solution, or be soluble in the moisture of the tongue.

---

* The investigations of SCHIRMER, *Nonnullæ de gustu disquisitiones*, 1856, entirely confirm this view.

If insoluble, it excites a sensation of touch, but not of taste. The sensations of Taste may also be excited by mechanical and electrical stimuli—by gases, or a stream of cold air; which shows that the quality of sweetness, sourness, bitterness, or the like, depends on the nerve affected, even more than on the nature of the substance affecting it. Thus Henle found that a stream of cold air directed upon the tongue gave rise to a cool saline taste, like that of saltpetre. Dr Baly found "that if the end of the finger be made to strike quickly but lightly the surface of the tongue at its tip, or its edge near the tip, so as to affect, not the substance of the tongue, but merely its papillæ, a taste sometimes acid, sometimes saline, like the taste produced by electricity, will be distinctly perceived. The sensation of taste thus induced will sometimes continue several seconds after the application of the mechanical stimulus."* I have found that active gargling with cold water often produces a decided sweet taste, which lasts for several seconds; yet drinking cold water will not produce the slightest sensation of taste. And Stich has recently published researches † which prove that gases are tasted no less than solutions. If the tongue be outstretched, carefully dried, the lips pressing upon it, and the nose stopped, a stream of carbonic acid gas directed upon the *edges* of the tongue will instantly produce the acid sweet taste of carbonic acid; whereas carbonic acid may be directed through water for a long while without rendering it perceptible to taste. Chloroform vapour also produces a taste under similar circumstances; but if the edges of the tongue be covered, and the vapour be directed on the upper or under *surface*, no taste is felt. That electricity produces a sensation of taste, every one knows. Indeed, it was in this very fact of taste arising from the contact of two metals in the mouth, that Volta's discovery originated; and among the marvels of science we may surely rank this, that an observation, apparently so trivial, should have been the starting-point of a series of discoveries which have changed the whole body of science, and have profoundly altered the whole possibilities of industrial art. Here, as throughout the study of Nature, we learn that nothing is trivial except to trivial minds; and to trivial minds nothing is important unless it directly concerns their interests.

---

* MÜLLER's *Physiology*, ii. 1062; Dr BALY's note.

† See CANSTATT: *Jahresbericht*, 1857, p. 111.

Our senses are the sentinels which guard us against the approach of danger. The sense of Taste warns us against swallowing deleterious substances, as that of Smell warns us against noxious gases, and against some that are not noxious, while it allows others to pass which are very injurious. Indeed, the value of this service must not be exaggerated, as by many writers it is, who seem blind to daily fact, when they have a thesis to maintain. The Senses are sentinels which sometimes sleep, and sometimes allow an enemy to pass, if the watchword be given; and thus children are poisoned by agreeable berries, men eat with great relish substances which prove very noxious. "Among the lower animals," says Dr Carpenter, "the instinctive perceptions connected with this sense are much more remarkable than our own"—a statement which is somewhat startling, considering that man's instinct has enabled him to detect so many eatable substances which prove eminently beneficial; but the grounds on which the statement is made are questionable: "Thus," he continues, "an omnivorous monkey will seldom touch fruits of a poisonous character, although their taste may be agreeable."* Agreeable to whom?—to the monkey? If the taste were agreeable to the monkey, the poisonous character of the fruit would not prevent his eating it; if disagreeable to the monkey, there is surely small marvel in his leaving it untouched, however pleasant the flavour may be to *another* animal.

Sensations of Taste are properly only those of flavours. Many pungent substances, such as mustard, produce powerful sensations; but although frequently classed among the sensations of Taste, these are really nothing more than irritations, differing in degree, but not in kind, from those produced by the same substances on other parts of the surface.

Taste and Smell act so constantly together that we not unfrequently confound them. Many substances which are considered eminently savorous are tasteless if the sense of Smell be in abeyance—as when the nose is held, or at the early stage of a cold in the head. The *bouquet* of wines intensifies their flavour; and there is no more popular method of taking a black draught than that of holding the nose till all be swallowed.

---

* *Human Physiology*, p. 696.

There is another point in which Taste resembles Smell: a moist surface is absolutely necessary for the organ to become sentient. No substance, however savoury, can be tasted if the tongue be perfectly dry; whereas gases, as we have seen, require that the tongue should *not* be moist if they are to produce a taste.

After-tastes are sometimes observed. They endure long after the substance which excited them has been removed, and modify the taste of other substances. Müller remarks that after he had chewed a piece of the root of sweet-flag (*Acorus calamus*), milk and coffee had a *sourish* taste. The intensely bitter taste of tannin is followed by sweetness. Every one knows how sweets injure the flavour of wines, and how some other substances, such as olives or cheese, improve it. There appears to be something of the same relation existing among tastes which is observed among colours; those which are opposed, or complementary, rendering each other more vivid.

Taste soon becomes blunted, unless varied; as the sensation of colour becomes more and more indistinct the longer the eye dwells on it. In illustration of this there is a familiar experiment: a man's eyes are bandaged, and he is made to drink alternately of port and sherry; at first he readily distinguishes the one wine from the other; but after a few sips it becomes impossible for him to say which is port and which sherry: whereas if he were allowed to eat a bit of biscuit, or to drink a little water from time to time, he could continue distinguishing each wine indefinitely.

"The reaction of the sense of taste seems capable of being excited also through the medium of the blood, in the same way that the sense of vision is affected, so as to produce flickering before the eyes, &c., by the presence of narcotic substances in the circulation. M. Majendie has observed that dogs, into whose veins milk has been injected, lick their lips with their tongue, as if they tasted. It is probable that the sense of taste is sometimes modified, and peculiar sensations of taste excited by internal changes in the condition of the nerves; but it is difficult to distinguish such phenomena from the effects of external causes, such as changes in the nature of the secretions from the mouth." *

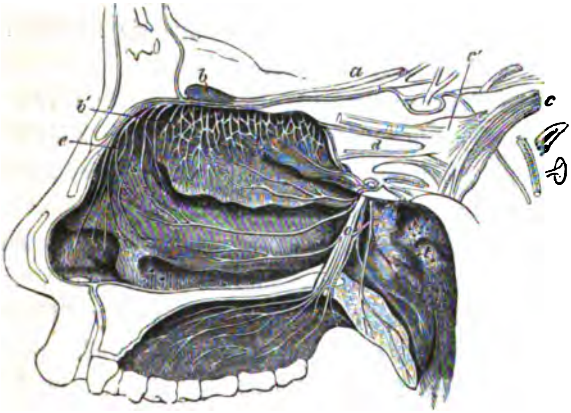
3. *The Sense of Smell.*—The organ of Smell is situated in the upper part of the interior of the nose. This much is certain; but the reader will probably be surprised to learn that serious doubts are permissible as to whether the so-called Olfactory nerves (the first pair) are in truth olfactory in function. Let us first take a general view of the parts supposed to contribute to the sense of smell, as represented in fig. 64, after Sömmering.

From the base of the brain there issues on each side a long process, with a bulb at the end (*a* and *b*). This process is called the Olfactorius. It is now no longer regarded as a nerve, but as a portion of the cerebrum. It was thus also that Galen regarded it, not on very accurate knowledge, we must admit; and till the time of Willis this opinion prevailed. From Willis down to our own day the process was considered to be the

---

* MÜLLER: *Physiol.*, ii. 1324.

Fig. 64.



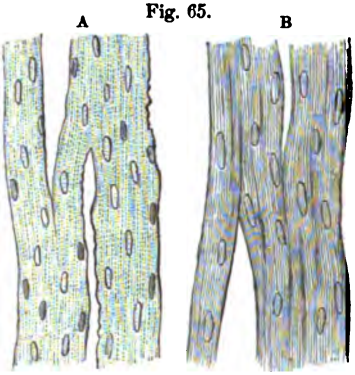
olfactory nerve, until microscopic examination showed that it was not a nerve, but a ganglionic mass. The olfactory bulb (*b*) lies upon that part of the skull named the cribriform plate. Through the holes in this plate some five-and-twenty smaller processes, called the olfactory filaments (*b'*), descend, and ramify on the mucous membrane of the nose.

To complete our survey of the organ, let us glance at the other nerves which enter the nose. At *e* we observe a twig of the ophthalmic branch of the fifth nerve, which has nothing to do with the sense of Smell. At *c* is the fifth nerve, and the Gasserian ganglion *c'*; and at *d* its superior maxillary division, sending off filaments which anastomose with the olfactory filaments. At *o* there are the twigs which supply the palate.

All these are nerves; but whether the olfactory filaments are *nerves,* is not, to my mind, absolutely certain; and whether they have any intrinsic relation to the sense of Smell is even more dubious.

If the olfactory filaments are really nerves, they are unlike every other nerve in the body. The bulb is a ganglionic mass, but the filaments which descend from it are unlike all other nerves issuing from ganglia. They are neither formed of tubular fibres, nor surrounded with the investing membrane (neurilemma), such as is found in other nerves, but with a very different membrane. They consist of a striped granular mass, not of distinct fibres. Nuclei are abundantly, but irregularly, scattered through this mass. Todd and Bowman remark, that if these filaments be compared with the filaments of the fifth nerve, which anastomose with them, the contrast will be very obvious. Those anatomists have given a representation of an olfactory filament of the dog, which is here reproduced.

Fig. 65.



Olfactory Filaments of Dog.
A, in acetic acid; B, in water.

This peculiarity in the structure of the olfactory filament has been carefully investigated,[*] and likened to that of the *grey* or *gelatinous* fibres which Remak first discovered intermingled with true nerve-fibres in the sympathetic system. These grey fibres, however, are by many recent anatomists pronounced to be peculiar forms of connective tissue, and not nerve-fibres. Be this as it may,[†] we must remember that the grey fibres only intermingle with the ordinary fibres; they do not constitute the whole, or greater part of a nerve; so that, if the olfactory filaments are nerves, they are markedly different from all other nerves. Indeed, two recent writers, Seeberg and Erichsen, declare them to be formed of connective tissue, and not to be nerves. Bidder, their master, holds the same opinion.

There is one fact which is almost decisive. We know that all nerves degenerate when the ganglion with which they are in connection is destroyed. If, therefore, the olfactory filaments were nerves, they ought to degenerate after destruction of the bulb; and as they do *not* degenerate after destruction of the bulb, it is almost certain that they are not nerves.

Having thrown doubts on the nature of the olfactory nerves, I must now proceed to throw doubts on their olfactory function. These doubts are by no means new. Majendie thought he had proved by experiment that Smell persisted after the nerves had been destroyed;

[*] TODD and BOWMAN: *Physiol. Anat.* ii. 8; KÖLLIKER: *Gewebelehre;* LEYDIG: *Histologie*, 215; SHARPEY and ELLIS: *Quain's Anat.* iii. 82.

[†] See KÖLLIKER: *Gewebelehre*, p. 343; LEYDIG: *Histologie;* BIDDER und KUPFFER: *Untersuchungen über die Textur des Rückenmarks*, p. 27.

but his experiments have been rejected, as proving nothing. Majendie said it would be important to ascertain whether the congenital absence of the olfactory nerves was coincident with the absence of Smell. "I do not know," he added, "that any such case has been observed." Hereupon Longet replies with triumph, "In my *Treatise on the Nervous System*, vol. ii. p. 38, may be read cases reported by Schneider, Rolfinck, Magnemus, Falmer, Rosenmüller, Cerutti, Valentin, and Pressat, in which the complete absence of Smell coincided with a congenital absence of the olfactory nerves."[*]

This answer seemed very conclusive to M. Longet, and may seem so to the reader, until he reflects that the fact of a function being absent at the same time that a particular organ is absent, is no proof of a direct and absolute relation between the two; whereas if one case can be adduced in which the function was present, and this particular organ absent, *that* is categorical proof of the function not being dependent on the organ. One such positive would outweigh a hundred negatives.

Majendie cited one case in which disease had entirely destroyed the olfactory nerves without destroying the sense of Smell. But Bérard, who communicated the case, subsequently came to the conclusion, "that the information respecting the olfactory sensibility of this patient having been collected after the destruction of the nerves was ascertained, it is far from reliable, and, I am convinced, was false."[†]

---

[*] LONGET: *Traité de Physiol.*, ii. 278.

[†] Ibid.

In cases of disease we are never certain as to the precise effects, for we can never be certain as to the extent of the organic interference. But in cases of congenital deficiency we sometimes meet with instructive evidence; and on this very subject we can bring forward one which has all the characters demanded by rigorous scrutiny, reported by an authority of the highest eminence, Claude Bernard. When he was Majendie's assistant at the Collège de France, he commenced the dissection of the head of a woman who had died of consumption in the hospital. On opening the skull he was startled to find a complete absence of the olfactory nerves. Closer investigation showed that in all other respects the brain was of the normal structure, its membranes and vessels normal, and the origin of all the other nerves perfectly regular; but of olfactory nerve, or bulb, there was not a trace. This was not a case of absence from disease, but of congenital malformation. This interesting anatomical specimen is still preserved in the Collège de France; and a figure, representing the brain and the base of the skull, is given by M. Bernard.*

In the presence of such a remarkable fact as this, M. Bernard naturally sought its physiological interpretation. He went to the persons with whom the young woman had lived for the last six months, and without giving them any clue as to the object of his questions, interrogated them minutely respecting her modes of life, and her likes and dislikes, taking especial care to draw the conversation to topics which would elicit details respecting her sense of smell. From them he learned that "Marie found the odour of tobacco insupportable," and that "particularly in the morning, when she came into the room in which any one had been smoking overnight, her first act was to open the window to let out the unpleasant smell of stale tobacco—*la mauvais odeur de pipe renfermé.*" He learned also that she frequently complained of the fetid smell of a closet which was near her room; and that for six weeks she officiated as cook, tasting sauces, &c., and being rather distinguished for her skill at it.

---

* CLAUDE BERNARD: *Système Nerveux*, ii. 232.

M. Bernard then went to the man with whom Marie had cohabited during four years. He said that Marie was fond of flowers, and always smelt at them. In fact, she tasted and smelt like every one else. A third person, who had tended Marie during her last illness, mentioned in particular that Marie often complained of unpleasant odours.

It is impossible to escape the conclusion that Marie really did possess the sense of Smell, unless we suppose a conspiracy on the part of all these persons to state what they knew to be false—a conspiracy as incredible as it would have been idle. Even assuming an exaggeration in their statements, the particulars are such as force our belief that some degree of Smell must have been manifested; and the presence of even a feeble function in the complete absence of the olfactory nerves, is enough to prove that the function in question cannot be that of those nerves. For observe, it is not a case of degree we have here; it is not a case of olfactory nerves partially destroyed, or incompletely developed; it is a case of entire absence of those nerves, ganglion and all; yet with this entire absence there is unequivocal evidence of the sense of Smell being present.

M. Bernard remarks, that in no case hitherto recorded has the absence of olfactory nerves been *predicted* from the imperfection of the sense of smell, and this prediction subsequently found to be accurate; but that after the anatomical fact has been discovered, men have sought for proofs of the absence of a sense of smell in what nurses or relatives could narrate. "If," he says, "any one case can be shown me in which, during life, the physician has observed absence of the sense, and predicted a corresponding absence of the nerves, which prediction has subsequently been verified, I will willingly hold my case to be insignificant. No such case, however, exists." M. Bernard here seems to me more ready to concede the insignificance of his case than philosophy justifies. However great a presumption might be founded on the successful prediction, it would still be very far below the value of such a case as that recorded by him.

This inquiry into the organ of Smell has only interest for the student; the general reader must pardon its introduction — a pardon he can the more easily grant, since he has probably skipped the paragraphs devoted to the inquiry. Enough, then, if we know that the mucous membrane lining the upper cavity of the nose is the *seat* of smell; and as this part is also furnished with filaments from the fifth pair of nerves, we may conclude that these can serve the function of Smell as their fellows can serve the function of Taste.

The odorous substances are very numerous; but it is indispensable that they should be in a gaseous or volatile condition before they can excite the sensation of smell. Musk itself, powerful as is its odoriferous property, would produce no sensation on the olfactory organ, if applied to it in a solid state; nor can liquids produce odours till they evaporate.* Our distinguished chemist, Mr Graham, contributes the following valuable note to Mr Bain's work on the Senses and the Intellect:—

"Odorous substances are in general such as can be readily acted on by oxygen. For example, sulphuretted hydrogen, one of the most intense of odours, is rapidly decomposed in the air by the action of the oxygen of the atmosphere. In like manner, the odorous hydrocarbons are all oxydisable—the ethers, alcohol, and the essential oils that make aromatic perfumes. The gases that have no smell are not acted on by oxygen at ordinary temperatures. The marsh gas, carburetted hydrogen, is a remarkable case in point. This gas has no smell. As a proof of the absence of the oxydisable property, Professor Graham has obtained a quantity of the gas from the deep mines where it had lain for geological ages, and has found it actually mixed up with free oxygen, which would not have been possible if there had been the smallest tendency for the two to combine. Again, hydrogen has no smell, if obtained in the proper circumstances: now this gas, although combining with oxygen at a sufficiently high temperature, does not combine at any temperature endurable by human tissues. It is farther determined, that unless a stream of air containing oxygen pass into the cavities of the nostrils along with the odoriferous effluvium, no smell is produced. Also, if a current of carbonic acid accompanies an odour, the effect is arrested. These facts go to prove that there is a chemical action at work in smell, and that this action consists in the combination of the oxygen of the air with the odorous substance."[*]

---

* "It throws some light on the diversity of taste which prevails in regard to scents, that the same substance may be agreeable in a diluted which is offensive in a concentrated state. The volatile oils of neroli, thyme, and patchouli are in themselves unpleasant, but when diluted with a thousand times their bulk of oil or spirit, their fragrance is delightful."—JOHNSTON'S *Chemistry of Common Life*, ii. 244, last edition.

In few things do human beings differ more widely than in their sense of Smell. Not only is the acuteness of this sense markedly different in different men, but of twenty men having average susceptibility, perhaps no two will be found to agree in considering the same odours agreeable. Musk is notoriously very offensive to many persons; others do not like mignonette; some do not recognise any odour at all in a flower considered very odorous by others. The *Iris persica* was found by Turner to have a pleasant odour by forty-one out of fifty-four persons, a disagreeable odour by one, and very little scent by four others. Of thirty persons, twenty-three held the *Anemone nemorosa* agreeable in its perfume, and the other seven did not think that it smelled at all.*

---

[*] BAIN: *The Senses and the Intellect*, p. 163.

The uses of smell are important, and in animals much more so than in man. It is by smell that they are guided to their food; by smell they hunt their prey; by smell they recognise each friend or foe in the dark. That we are not endowed with such keenness of scent, is probably owing to the comparatively small exercise of that function in civilised life. Having so many other avenues of sensation, so many other modes of recognising objects, this one falls into abeyance, and becomes weakened from disuse. But in men born with one or more of the other senses defective, Smell is seen to be an important avenue to the mind; and in savages the keenness of scent notoriously surpasses that of civilised races. James Mitchell was born blind, deaf, and dumb; yet by smell he at once perceived the entrance of a stranger into the room. People who have a strong antipathy to cats, detect their presence by the odour in circumstances which would be thought impossible. A lady in my study one day suddenly remarked —"There is a cat in the room;" on my assuring her there was none, she replied, "Then there is one in the passage." I went out to satisfy her; there was no cat in the passage; but on the first landing-stairs, looking through the railings, there, sure enough, was the cat!

---

* DRAPER: *Human Physiol.*, 427. Comp. also MÜLLER, ii. 1317. There is a valuable collection of facts with regard to smell in CLOQUET, *Osphresiologie*, 1821, a work often cited, but one I have not seen.

Humboldt, whose authority renders credible what might otherwise seem questionable, declares that the Peruvian Indians can, in the dark, distinguish by the smell the different races, European, American-Indian, or Negro.

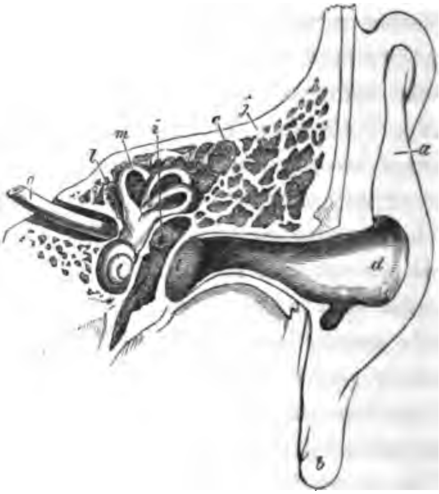
There is one more point needful to be noted here: it is, that the animal effluvia are all dense gases (except sulphuretted hydrogen), and are diffused slowly. In course of a little time, they will mingle with the lighter gases, according to the law of diffusion, but inasmuch as they thereby become diluted, the odour will best be perceived somewhere near the ground. It is on this account that the pointer and bloodhound run with the nose to the ground. The unwholesome effluvia of decaying matter will be felt in the ground-floor, scarcely perceived by the persons in the first-floor, and perhaps not at all in the garret. Hence the danger of lying on the ground in tropical swamps. "Swung on a tree fifty feet high, one may pass the night safely."

4. *The Sense of Hearing.*—The sensations of sound are quite special, and are only capable of being excited in a special organ, the Ear. This organ is very complicated, and as the functions of its various parts have not yet been all ascertained, it would lead us into unnecessary detail were we here to examine its struc-

ture minutely. Referring the student to anatomical writers,* we will endeavour to render the general structure intelligible.

The following figures represent a section of the auditory organ, somewhat magnified in respect to the deeper structures. They are copied, with alterations, from Milne Edwards:—

Fig. 66.



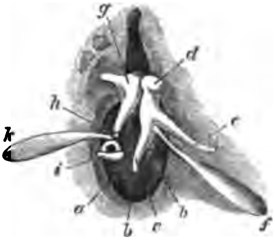
The ear-bones removed from this figure are exhibited magnified in Fig. 67.

---

* TOYNBEE: *Philos. Trans.*, 1851; TODD and BOWMAN: *Physiol. Anat.*, ii.; SHARPEY and ELLIS: *Quain's Anat.*, iii.; KÖLLIKER: *Gewebelehre*, 1859; CORTI in SIEBOLD u. KÖLLIKER: *Zeitschrift*, 1851, iii. In MÜLLER'S *Physiology*, the chapter on the Sense of Hearing is very exhaustive. LONGET, also, is worth consulting: *Traité de Phys.*, ii.

The essential part is, of course, the auditory nerve, numerous minute filaments of which are distributed over the membrane which lines the spiral chambers of the Labyrinth, or internal ear. This membrane is a sac, enclosing liquid. It is also surrounded by liquid, which intervenes between it and the bony walls of the spiral chambers. The impression of the vibrations, produced in this liquid, on the auditory filaments, causes the sensation of sound.

Fig. 67.



How are these vibrations produced? When bodies are struck, vibrations are communicated to the atmosphere. These reach the external ear. A mechanical contrivance of great simplicity, which has been imitated in the ear-trumpet, collects and concentrates the vibrations on the membrane of the tympanum (or middle ear). The membrane has been likened to the skin stretched across a drum.

According to popular conceptions, the pulsations of air upon this membrane stretched across the tympanum are the causes of the sounds we hear. This is a mistake. A drum is silent if its skin be burst; because the skin is the vibrating surface of the drum. But the ear is not a drum, and the membrane of the tympanum may be burst, not only without destruction of hearing, but in some cases with a terrible *increase* in susceptibility to sounds. Cheselden destroyed both membranes in dogs, and found they were terrified at all loud sounds; and there is a case on record of a man with a hole in his tympanum, to whom the whistling of another man in an adjoining room was intolerable.

Nevertheless, we must bear in mind that the vibrations of this membrane are really of great assistance in hearing; and it is found that a thickening, or stiffening of it, or even unusual dryness, will render hearing dull. Many a temporary deafness has passed away after the ear has been well syringed with warm soap-and-water. According to a valuable memoir by Dr Edward Clarke, an American physician, the membrane plays a more important part than physiologists have been willing to admit. Against the facts cited above may be placed his observations of seventy-five cases of perforated membranes: of these, in five cases, the tick of a watch was not heard; in eleven others it was only heard when the watch was close to the ear; in twenty-three others only at a distance of five inches from the ear; in sixteen others only at a distance varying from six inches to a foot; in fifteen others at a distance of one to two feet; and in four cases at two to four feet; whereas in the ordinary condition of the ear the tick is heard at fourteen or fifteen feet.* These observations are valuable, but I would call attention to a source of fallacy in them. It is forgotten that a man with one perforated tympanum has still the *other* ear unaffected, and with this other ear he ought distinctly to recognise the tick of a watch anywhere within fourteen feet. If he does not, there must be some other cause besides the perforation of one tympanum.

---

* CLARKE: *American Journal of Medical Science*, 1858, p. 13; abstracted in BROWN-SÉQUARD'S *Journal de la Phys.*, i. 644.

The elastic membrane stretched across the drum of the ear is easily set vibrating by waves of air. These vibrations are communicated to the chain of little bones, called respectively the *malleus*, or hammer (directly attached to the membrane), the *incus*, or anvil, and the *stapes*, or stirrup, which is attached to the oval-shaped membrane stretched across an opening into the labyrinth, and called the *fenestra ovalis.* On the other side of this oval membrane, and filling the cavity of the labyrinth, are, 1st, the liquid, and, 2d, the membranous sac, on which ramify the auditory filaments, previously described. Thus is a communication established between the vibrations of an external body, say a violin string, and the vibrations of the membrane containing auditory filaments. Sonorous waves first agitate the tympanic membrane, which agitates the little bones, which agitate the labyrinthine liquid, which agitates the labyrinthine membrane, and here presses on the nerve-filaments. The process is really *more* complicated than this; but this is a general outline, which must suffice.

The character of the sensation produced will of course greatly depend on the nature of the impression, whether rapid or slow, intense or delicate. There are three physical peculiarities in the sonorous vibrations.

1°. *Intensity,* or loudness and feebleness of sound.

2°. *Pitch,* or the note sounded.

3°. *Quality,* as when the same note is sounded on different instruments, as the piano or the violin, the flute or the organ.

An ear may be very susceptible to mere intensity of sound, *noise,* and little so to pitch or quality The unmusical ear seems almost incapable of discriminating between two sounds differing only in pitch; and there are great individual varieties in the susceptibility even to pitch. Thus the cry of a bat is so acute as to be inaudible to some persons,—passing out of their range. It has been said that the deepest note audible by the human ear is one produced by 32 vibrations in a second; but M. Savart's experiments prove that sounds produced by 14 vibrations in a second are audible. The acutest note audible is one produced by 48,000 vibrations in a second.*

What is the difference between a musical and an unmusical ear? It resides in this: That the musical ear is sensitive to the distinction between a *noise* and a *note.* "A musical note is in itself a harmony, being the equal tuning of successive vibrations or pulses. It is, in a minute or microscopical subdivision, the same effect as equality of intervals, or time, in a musical performance; although the one may be a thousand beats in a second, and the other not more than two in the same time."†

The effect of music on our emotions is an exaltation of the effect which all sounds produce, especially *tones.* The influence exercised by the voice of an actor or an orator, far transcends that which the mere *meaning* of his words would excite; and if we disengage the mind from all influence of meaning—as when a foreigner speaks in a language unintelligible to us—we shall appreciate this powerful effect of tone. The speech which roused an audience to extravagant enthusiasm, may be read by people unacquainted with the speaker, and therefore incapable of even faintly imagining the *tones,* with complete calmness, and discriminating criticism; nay, even the audience may subsequently read with calmness the speech which they heard with rapture. Hence it is that great orators can never be fairly appreciated by those who have not heard them. And we may also observe that this personality of the speaker has often gained a reputation in society for wit, or eloquence, or capacity, which amazes those who have only the printed wit, or wisdom, on which to found their judgment.

---

* MÜLLER: *Physiology,* ii. 1299.

† BAIN: *The Senses and the Intellect,* p. 207.

It is notorious that children and animals are affected by our voices, when quite incapable of understanding the meaning of our words. It is notorious also that the meaning of a phrase may be diametrically changed, by repeating the phrase with a slight variation in the tone; and it is *this* which constitutes one of the iniquities of those who repeat to us what has been said behind our backs. The phrase may have been careless or insignificant, but the tone in which it is repeated may make it a wound.

What are called *subjective* sounds—*i. e.* those sensations of sound which have no external cause—depend upon a pressure of blood-vessels on the auditory nerve. All students know the singing in the ears which accompanies overwork of the brain; and all persons who have attended reviews, or made long and noisy journeys in rattling vehicles, will remember the continuance of the sounds long after the original cause had ceased.

"A sudden noise excites in persons of excitable nervous system an unpleasant sensation, like that produced by an electric shock throughout the body, and sometimes a particular feeling in the external ear. Various kinds of sounds, such as the friction of paper or scratching of glass, cause in many people a disagreeable feeling in the teeth, or, indeed, a sensation of cold trickling through the body. Intense sounds are said to make the saliva collect in the mouth in some people."*

One of the marvels of hearing is the appreciation of the direction from which sounds proceed. Sensations of Touch are referred to particular spots, only after multiform experience of the difference in the degree of the sensations excited in various spots. It is the same with hearing. We judge of the distance and direction of the sound by the kind of impression produced. The rumbling of a waggon in the street is thus often mistaken for distant thunder, or the distant thunder is mistaken for the rumbling of a waggon. The voice is known to proceed from above, or below, or behind us, solely by the *kind* of impression produced. How delicate is the susceptibility to shades of difference, may best be illustrated by the wonderful accuracy with which blind men thread their way along crowded streets: not only do they learn to recognise the different kind of persons—policeman, porter, or gentleman —by the sound of the tread, but they learn, it is said, to recognise the difference between a man standing still, and a lamp-post at a short distance, simply by means of the reverberations of their own footsteps.

---

* MÜLLER, ii. 1311.

5. *The Sense of Sight.*—Although that objective something, whatever it may be, which we call Light, affects the whole general surface, and in some animals excites very appreciable evidences of Sensibility, yet it is only through the Retina that it is capable of causing that specific sensation we call Sight. When rays fall upon the eyeless Polype, or Actinia, they cause it to move towards them, or away from them—they affect it; but not with the sensation of *light;* when they fall upon the optic nerve they produce no sensation of light; but when they fall upon the *retina,* and through it affect the optic nerve, the special sensation of light is produced. That it is not the *object*—undulating ether, or luminous corpuscle—which gives this specific quality to the sensation, but the condition of the optic apparatus itself, is evident from the fact, that pressure or irritation of the optic nerve also produces the sensation of a flash of light.

It is obvious that if the whole of this retina were exposed directly to the light, the whole surface would

3°. *Manhood* is a sort of table-land of life, but its limits are very variable. In it the body does not increase, except in solidity and vigour. The balance of waste and repair is tolerably even.

4°. *Old Age.* — The balance begins to lean; the movement of Assimilation slackens, and Death slowly advances. The limits of this epoch are the most variable of all.

These Ages are the Cycles of Change through which every organism must inevitably pass, if it be not earlier destroyed, and they are therefore inseparable from our conception of Life. We have seen how each epoch varies in duration, and we can understand, therefore, how the whole of each life will vary one from the other; so that although it is possible to assign the limits for the race, this can only be done as an *average*,—we can never know what will be the duration of an individual life.

And here it may be well to point out a common error in works which treat of Longevity, and one which is explicitly stated by Flourens, the latest writer on that subject.* It is that of supposing the duration of Life to be measurable by beats of the pendulum. We are told that the normal length of life for a human being is one hundred years; and that if the majority of men die earlier, it is because accident, disease, or imprudent habits, have shortened the term. The argument rests on the fallacy that, because some men reach the age of one hundred, all men would reach it were there not disturbing causes (*des causes troublantes*); but in-

---

* FLOURENS: *De la Longévité Humaine.* 1855.

The refracting humours are as follows:—

I. The *Aqueous* (10) is the watery humour contained within the cornea.

II. The *Crystalline* (12) forms the lens.

III. The *Vitreous* (13), or glassy, fills the bulk of the globe, and is enclosed in a delicate membrane called the *hyaloid.*

In these parts, only the Retina and the Choroid coat are concerned in producing the *sensation* of Sight. The rest of the apparatus is entirely optical, and subserves the purpose of throwing images on this sensitive apparatus. If we suppose an animal to possess an eye destitute of this optical apparatus, and having only a layer of pigment and a layer of nervous matter, we shall conclude it to have luminous sensations, but no images; it can distinguish light from darkness, but has none of the wondrous vision of higher animals.*

Two questions present themselves, What are visual images, and, Where are they formed? The old philosophers, following Democritus, believed that objects were continually throwing off attenuated images—*eidola*—of themselves; these entered the soul through the avenues of sense. This naïve material conception was in time replaced by one less material, but less intelligible, namely, that the mind perceived objects through the medium of *ideas,* which were exact *copies* of objects. This conception was displaced, and the direct perception of the things themselves, without any medium of ideas, was substituted for it. Throughout

* Compare *Sea-Side Studies,* p. 342.

the course of metaphysical speculation, no one hesitated to believe that images of some kind were formed in the eye, as in a mirror, and thence "transmitted to the mind." The fact, now familiar, that images can be *seen* upon the retina of an animal, as in a mirror, seemed to place this belief beyond dispute.

How many physiologists and philosophers of the present day really entertain this notion of images being transmitted to the brain, it is difficult to say, because ordinary language is so impregnated with the old conception, that men who have long rejected it as an hypothesis may still use it as a metaphor. We must be on our guard against such metaphors. Even on the supposition that images are formed on the retina, as the final condition of the sensitive stimulus—as the *cause* of the sensation—these images cannot be transmitted *as* images to the brain. That which is transmitted is an excitation of the Neurility of the optic nerve, which will excite the Sensibility of its centre; or in more untechnical language, it is the sensation which is transmitted, not the image. The optical apparatus which converges the rays of light into definite images may be, and is, indispensable for the proper excitation of definite sensations; as the acoustic apparatus of a pianoforte is indispensable for the production of those tones which in the ear will excite musical sounds: a succession of waves of light will produce no images without such an apparatus, any more than a succession of waves of air will produce musical chords.

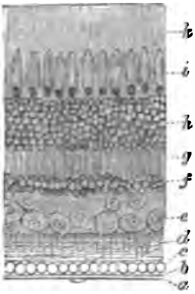
The formation of an image on the retina is the precursor of a visual sensation; but this image is not transmitted to the brain. The oxidation of a volatile substance is the precursor to an olfactory sensation; but this oxidation is not transmitted to the brain. The destruction of tissue which is the precursor of a sensation of a burn is not transmitted to the brain. That which is in each case transmitted is the excited sensation.

For centuries metaphysicians have been misled by the notion that the idea of an object is a *copy* of that object—an image. The source of the fallacy is their having taken vision as their standard. If it seemed plausible to say that the image seen of the fire is an accurate copy of the fire, the plausibility vanished when the pain of a burn was the sensation spoken of. When I see a rose, my eye is affected; when I smell it, my nose is affected; when I touch it, my fingers are affected: these three sensations are entirely different; yet how can I be entitled to assume that the eye gives me a more accurate copy of the rose, than the hand or nose gives me?

Moreover, as regards the formation of images on the retina, it is necessary to be borne in mind, that wherever the image is formed, it has to be *decomposed* before it becomes a sensation. And this leads us to the second inquiry, Where are images formed? The usual answer is, On the retina. But there is every reason for believing that the retina is not the sensitive surface—not the analogue of the photographer's iodised plate. As this will probably surprise the reader, we may pause a while to consider the evidence.

The retina is not simply an expansion of the optic nerve; it is that, and something else; it has a very complex structure. The optic nerve, on entering the globe of the eye, expands, and forms a layer of fibres; underneath this layer, and forming part of the retina, there is a layer of matter generally supposed to be nervous, but declared by some recent histologists to be nothing of the kind. The accompanying figure, copied from Kölliker, represents the appearance of the retina in a transverse section, as seen under the microscope.*

Fig. 69.



Transverse Section of the Human Retina.

a, hyaloid membrane; *b*, limitary membrane; *c*, bright globules (nature unknown); *d*, layer of optic fibres; *e*, large cells; *f*, granules; *g*, radial fibres; *h*, granules; *i*, *k*, rods and cones.

Here we perceive a layer of fibres (*d*), beneath this a layer of large cells (*e*), beneath this layers of granules (*f*, *h*), united by *radial* fibres to the cells; and beneath this again a layer of perpendicular rods and cones (*i*, *k*), formerly known as Jacob's membrane. All these layers are radially connected; so that the nerve-fibre is said to pass downwards from the inner surface of the retina to the pigment layer of the choroid. It is generally held by modern anatomists that these rods and cones are the real percipients of light: from these the impression is transmitted to the cells, and thence to the fibres of the optic nerve.

---

* It may save some trouble and vexation to the student who desires to investigate this structure, if he learn that the appearance thus figured will probably never be seen by him in any one section. After making at least fifty attempts in vain, I learned, in Germany, that Müller himself, who first described it, professed never to have seen the whole in any one preparation; his picture was made up by combination of various sections.

It was hinted just now that the nervous character of the retina was under question, only the fibres of the optic nerve being admitted by some writers to have that character. The doubt came from the Dorpat school, and was at first hotly disputed.* The question is too abstruse to be argued here, but there is one experiment, which, if confirmed, I should consider decisive. The reader knows that when a nerve is separated from all connection with ganglionic substance, it degenerates; but so long as any fibres are united with ganglionic substance, they survive. Now the cells found in the retina are held to be ganglionic, and indeed resemble those of the brain. If they were ganglionic, they would preserve the fibres of the optic nerve with which they were connected. Here, then, experiment may pronounce, and has pronounced. Lehmann divided the optic nerve of a dog, and twenty days afterwards examined the retina—he found *only* the layer of optic fibres degenerated; and as these were not protected by the cells, the conclusion is that the cells are not ganglionic. Thus the whole of the retina, except the optic fibres, is held to be connective tissue.*

---

* See BLESSIG: *De retinæ structura*, 1855; CANSTATT: *Jahresbericht*, 1855; BIDDER und KUPFFER: *Textur des Rückenmarks*, p. 27; FUNKE: *Physiologie*, i. 711; KÖLLIKER: *Gewebelehre;* LEYDIG: *Histologie*.

Let this question be decided as it may, the point for our present consideration is this, that the surface of the retina which is sensitive to light, is *not* the surface on which the rays first impinge—*not* the layer of optic fibres—but the inner surface, which is in immediate contact with the pigment layer. A moment's consideration will now make it evident that images cannot be formed on the retina, as they are on the receiving-screen of a camera lucida. In the first place, we must remember that during life the retina is transparent as glass. The rays of light will consequently pass *through* it, as through glass, and will be arrested by the black pigment on which they impinge. In the second place, it is certain that the optic fibres are insensible to light. There is a blind spot in every eye, which may be proved by the following experiment: Fix a red wafer against the wall, close one eye, and slowly retreat from the wall, looking steadfastly at the wafer: on reaching a certain distance, the wafer will suddenly vanish from sight. If you now take a step backward, or a step forward, the wafer then becomes visible; but always at that particular distance where the rays converge so as to throw the image on one spot of the retina, the wafer is invisible. Now this blind spot is none other than the spot where the optic nerve enters the eye, and where, consequently, nothing but nerve-fibres exist.

---

* LEHMANN: *Experimenta quædam de nervi optici dissecti ad retinæ texturam vi et effectu*, 1857. Cited by HENLE: *Bericht über Anat.*, 1858, p. 162.

If, therefore, the retina is directly insensible to light, the rays of which necessarily pass through it to be absorbed by the pigment layer, we have to inquire how the retina becomes affected by the light after impinging on the pigment. That the pigment is really the part first affected, and that the retina is secondarily affected through this pigment, seems to me demonstrable from the fact that in the invertebrate animals the pigment layer is in *front* of the retina, not *behind* it, as in vertebrates, consequently the rays of light must first affect the pigment;* and no one will suppose that images can be formed on a transparent substance, if the light first falls on a black surface covering the transparent substance.

But what is the nature of the effect on the pigment when the light has traversed the retina? Here Professor Draper comes to our aid. Franklin, he reminds us, placed variously-coloured pieces of cloth in the sunlight on the snow. They were so arranged that the rays should fall on them equally. After a certain period he examined them, and found that the black cloth had melted its way deeply into the snow, the yellow to a less extent, and the white not at all. The conclusion Franklin drew has since been abundantly confirmed, namely, that surfaces become warm in exact proportion to the depth of their tint, because the darker surface absorbs the greater amount of rays. Applying this to the eye, Professor Draper maintains that the black pigment layer is the real optical screen. Its perfect opacity causes it to absorb the rays of *light*, turning them into *heat*. An image is thus burnt in upon the retina. "The primary effect of rays of light upon the black pigment is to raise its temperature, and this to a degree which is in relation to their intensity and intrinsic colour. In this local disturbance of temperature the act of vision commences."* In the blind Crustacea there is no pigment; and in Albinos, whose vision is very imperfect, the pigment is of a lighter colour than ordinary.

---

* The point is argued in *Sea-Side Studies*, p. 347-52.

The conclusion at which we have arrived is, that the optical apparatus of the eye is necessary for the formation of definite images on a sensitive surface, which would otherwise be indefinitely stimulated. These images, or rays of light, pass through the retina, and impinge on the black pigment, in which they cause a change of temperature exactly proportionate to the intensity and intrinsic colour of each ray. This change of temperature acts upon the inner layer of the retina, the rods and cones; which in turn stimulates the Neurility of the outer layer, the fibres of the optic nerve; and this Neurility awakens the Sensibility of the optic ganglion, which may or may not awaken that of the brain. How far this is from the doctrine of "images transmitted to the brain" need not be pointed out.

Our conclusion further elucidates the *subjective* nature of vision. We can no longer suppose that we see the objects themselves. Our visual sensations are simply excited states of our sentient organism. Hence it is not more wonderful that a man whose eye was extirpated should perceive, when the other eye was closed, different images, such as lights, circles of fire, dancing figures, &c., floating in front of the eyeless orbit, than that a man whose leg had been amputated should feel distinct prickings in the absent toes. The fact that luminous sensations are excited ordinarily by the action of the retina, but also, exceptionally, by internal causes irritating the optic centre, enables us to understand *spectral* illusions. It is well known that persons suffering from brain disease, or disturbance of cerebral circulation, have seen spectral objects with a vividness equal to that of actual vision. A black cat is seen to run up the wall; a person is seen to enter the room; and no assurance of the bystanders to the contrary will persuade the patient that what he sees so vividly is not actually present.

---

* DRAPER: *Human Physiology*, p. 387.

At the Leeds Meeting of the British Association, Professor Stevelly narrated the following anecdotes:—

At the close of the last college session he had been in weak health, and had gone out to his brother-in-law's seat in the country for a few weeks. While there, he had become greatly interested in the economy and habits of the bees. "One morning, soon after breakfast, the servant came in to say that one of the hives was just beginning to swarm. The morning was a beautifully clear, sunny one, and I stood gazing at the insects, as they appeared projected against the bright sky, rapidly and uneasily coursing hither and thither in most curious yet regular confusion, the drones making a humming noise much louder and sharper than the workers, from whom also they were easily distinguished by their size; but all appearing much larger in their rapid flights than their true size. In the evening, as it grew dark, I again went out to see the beehive, into which the swarm had been collected, removed to its stand; soon after I was much surprised to see, as I thought, multitudes of large flies coursing about in the air. I mentioned it to my sister-in-law, who said I must be mistaken, as she had never seen an evening on which so few flies were abroad. Soon after, when I retired to my chamber, and knelt to my prayers before going to rest, I was surprised to see coursing back and forward, between me and the wall, what I now recognised as the swarm of bees, the drones quite easily distinguishable from the workers, and all in rapid whirling motion as in the morning. This scene continued to be present to me as long as I remained awake, and occasionally when I awoke in the night, nor had it entirely faded away by next night, although much less vivid. This was the first instance I had ever heard of moving impressions having become permanently impressed on the retina, nor can I give the slightest guess at the *modus operandi* of the nerve. Notices of fixed impressions, particularly after having been dazzled, are now common enough. The Rev. Dr Scoresby, at the late meeting at Liverpool, had given a detailed account of some which had presented themselves to him; and a very curious one had occurred to me some years since. I was walking down the streets of Belfast with Sir John Macneill, the eminent engineer, when he said to me—'What has become of my old friend Green who kept that shop? I see new people have got it.' Turning suddenly to look at the shop indicated, I was completely dazzled by the bright reflection of the sun shining on the new brass-plate under the window of the shop, so that for some seconds I could see nothing. As we walked on, I soon observed before me in the air the words 'J. Johnstone & Co.,' in blood-red characters, which soon, however, changed to other colours. With an exclamation of surprise I stated the fact, and we turned back to see whether or not this was really the inscription on the brass-plate, and found that it was. The optical account of this was simple enough. The retina had been partially paralysed from the intense light reflected from the plate, but as I had turned with pain from it instantly, the part corresponding to the black letters on the plate had escaped; and as I walked on, the red strong light reflected from surrounding objects on this part became contrasted with the darkness, as yet showing itself on all the surrounding parts of the disordered retina: as the retina recovered its tone, other colours in succession took possession of the place which at first had been red. Sir J. Macneill then told me that when first he had gone to reside in London, a murder had been found out by a similar circumstance. The murderer, then unknown, had been dazzled by the reflection of the sun from a bucket of water which another man was carrying before him; and soon after seeing in the air what he took for a bucketful of blood going before him, he was seized with such horror that he declared himself the murderer, and disclosed such facts as brought the crime home to him, so that he was convicted and executed."

These curious cases are doubtless referable to different causes. In the first and third, we have the effect of *suggestion* acting on an over-excited brain, and producing a spectral illusion, similar to that of mania, or dreaming. In the second, we have simply the effect of the *duration* of a sensation. It is well known that a sensation endures some time after the cessation of the actual impression. According to Plateau, the sensation endures about 1-35th of a second after the impression has ceased. But this duration must of course vary with the intensity of the impression. If we look at the moon and close our eyes, the image will vanish much more rapidly than the image of the sun under similar circumstances. If we fix our eyes on the panes of a window through which the sunlight is streaming, the image of the panes will continue some seconds after closure of the eyes.

It would be pleasant and profitable to enlarge on this subject of ocular spectra, were there space at disposal; but we have still more pressing topics to which we must address ourselves. There is, for instance, the interesting and often-mooted problem of how, with two eyes directed to a single object, we have only one image, and not two. Both eyes are equally capable of seeing; both retinas are certainly affected by the object; both eyes can alternately be directed towards it, and see it; yet when both are simultaneously directed towards it, only one image is seen, only one sensation is perceived. Many explanations have been offered of this paradox; but I think they have all failed, because they have all been sought in a wrong direction. A few years ago I proposed an explanation, which still seems to me the only one consistent with what is known of the other senses. Instead of seeking an anatomical or optical cause, it seemed to me that we ought to seek a psychological cause. Instead of trying to explain single vision as the result of the decussation of the optic nerves, or of those optical principles which Professor Wheatstone has made familiar by his stereoscope, let us first ascertain whether the fact to be explained is *peculiar* to the sense of sight? To ask this question is to answer it. If we have two eyes and single vision, we have also two ears and single hearing, two nostrils and single smelling. The mere citation of such facts suffices to overthrow all the anatomical and optical explanations. The cause must lie elsewhere; and must be common to all sensations.

Inasmuch as the object affects both eyes *simultaneously*, and with *equal intensity*, it cannot produce two sensations, but only one sensation. Simultaneousness of two impressions renders them indistinguishable from each other. Let two sounds of precisely similar

CONCLUSION.

AND now, reader, we have reached the end of our toilsome, but, let me hope, not profitless journey. There are many other questions of interest and importance which the physiologist has to study, but they do not fall within the range of common interests, and must therefore be omitted from these pages. My object was thus explained in the original prospectus:—

"No Scientific subject can be so important to Man as that of his own life. No knowledge can be so incessantly appealed to by the incidents of every day, as the knowledge of the *processes by which he lives and acts.* At every moment he is in danger of disobeying laws which, when disobeyed, may bring years of suffering, decline of powers, premature decay. Sanitary reformers preach in vain, because they preach to a public which does not understand the laws of life—laws as rigorous as those of gravitation or motion. Even the sad experience of others yields us no lessons, unless we understand the *principles* involved. If one man is seen to suffer from vitiated air, another is seen nose, the two impressions will be simultaneous, and only one sensation will be felt.

Another interesting question is, Why objects are not seen inverted? It is demonstrable, on optical principles, that the image of an object formed on the retina must be inverted. Why then do we not see it so? Here again the question is psychological, not optical. If *all* objects are inverted, the relative position of each will be unchanged; when all are inverted there can be no perception of their inversion: it is as if none were so. It is in our own sensations we must seek for top and bottom. We learn to associate certain sensations with certain relative positions. "An object seems to us to be up or down according as we raise or lower the pupil of the eye in order to see it; the very notion of up and down is derived from our feelings of movement, and not at all from the optical image formed at the back of the eye. Wherever this image was formed, and however it lay, we should consider that to be the top of the object which we had to raise our eyes or our body to reach."*

Among the remarkable phenomena connected with vision is that of an inability to distinguish certain colours and shades of colour, which recent investigations have proved to be much more frequent than was suspected.† Dalton, the celebrated chemist, published an account of his own infirmity in this respect, and the name of "Daltonism" was for a long period given to

---

* BAIN: *The Senses and the Intellect*, p. 233.

† WILSON: *Researches on Colour-Blindness.*

How far this object has been attained, the reader must judge; but I have received such cheering and generous encouragement from the profession and the public, during the course of monthly publication, as fully to repay the great pains and labour bestowed on the work; and I now part from it, and the reader, with reluctance and regret.

this is remarkable—that those colours which to ordinary vision are most strongly contrasted, are more frequently confounded by the colour-blind than the intermediate tints.

In closing here our necessarily imperfect survey of the great subject of our Senses and Sensations, the chief points of which have alone been noticed, it will be needless to indicate the deep psychological interest which attaches to such inquiries, since every one is aware of the strange blending of Thought in the act of Perception. We fancy that we *see* the solidity of an object, when in truth we *infer* it; we say "we heard thunder," when all that we heard was a sound, which we *inferred* to be the sound of thunder. The other day, during a country walk with a friend, the sound of horses' hoofs behind us reached our ears, unaccompanied by any voices. Presently the riders passed us, and I remarked to my companion: "It's very odd, but I was convinced that there were two women and a man on horseback behind us, and, sure enough, there they are." My companion declared he had formed the same conjecture. This conjunction of our inferences made it improbable that it should have been mere vague fancy—a guess without justifiable grounds; and I began to consider what could have been the indications which led us both to form it. All that we had to infer from was the sound of horses. In what could that suggest the sex of the several riders? Do women ride differently from men? Yes: they canter, men trot. The sound of two cantering horses accompanying the sound of a trotting horse, suggested that two women were riding with one man. I may further remark that neither of us had, to our knowledge, ever *noticed* the fact, as distinctive of women's riding; we must have noticed it, and silently registered it in our minds, otherwise the sound would not have suggested the sex of the riders: but we had never taken distinct cognisance of it; and had we been asked if there was any peculiarity by which the sound of a horsewoman could be distinguished from that of a horseman, it is most probable that we should have been unable to specify it.

Finally, I would call attention to the psychological importance of that vast class of sensations which has been termed Systemic-consciousness, and which psychologists and physiologists have so strangely neglected. They have given to the Sense-Sensations an almost exclusive part in the formation of our sensational activity, and often spoken of the mind as a mere educt of the Five Senses. The most striking example of this is seen in Condillac's famous statue, which is endowed successively with each of the five senses, and with each endowment developes gradually a complete mind. Monstrous as this hypothetical statue is, it is only a logical development of the conception that mind is the combination of the five senses. In these pages an attempt has been made to show that Mind is the psychical aspect of Life—that it is as much the sum total of the whole sensitive organism, as Life is the sum total of the whole vital organism—that various organs may be set apart for the performance of various special functions, mental as well as vital, but that no one exclusive organ of Mind can be said to exist, any more than one exclusive organ of Life can be said to exist. The reader may reject this view, which is submitted to him as the result of many years' meditation, and with that hesitation which naturally belongs to an opinion incapable of proof; but he is not at liberty to reject the fact that, over and above the sensations derived through our five Senses, there is a vast class of sensations derived through the Muscles and Viscera, sensations not less specific, not less important, than those of eye or ear: he is not at liberty to reject this, because it is capable of proof, as rigorous as the proof of the existence of Sight or Taste.

CHAPTER XI.

SLEEP AND DREAMS.

What is sleep?—Sleep and Death—Characteristics of sleep—Irresistible nature of the demand for sleep—Story of Dr Solander—Sleep under control—Heat and cold predisposing causes—Duration of sleep—Instances of refreshment from "forty winks"—Sleep of children and old people—Is sleep the period of repair?—Error of the ordinary opinion—Influence of sleep on the organism—A new theory of dreaming.

"Our little life is rounded with a sleep." The toils and anxieties, the schemings and excitements of the day, all merge in Sleep, which brings peace and renovation to mind and body. The fretful child cries itself asleep; in sleep the weary man loosens his overstrung faculties; the wretched man forgets his misery. Nearly a third part of our lives is passed in sleep, which, for all the active purposes of life, is a blank.

I. Sleep.—What is Sleep? We do not know. We can only say that it is a condition belonging to almost every animal organism (at any rate, to every organism possessing a high nervous development), which seems naturally brought about by the activities of that organism, and which, in some unexplained manner, helps to reinstate exhausted energy of brain and muscle. What is called the sleep of plants has, I conceive, only a superficial analogy with the sleep of animals, and is altogether dependent on totally dissimilar processes. The phenomena of sleep are restricted to the brain and higher senses; there can be no sleep where these are absent.

It is not difficult to decide whether a man is asleep, or dead, or swooning, or in a state of coma, or in a profound reverie. Artfully as he may feign to be asleep, observers will soon detect the feint; not because we know what sleep is, but because we know the many invariable signs of sleep. This is very much the position in which Physiology stands with reference to the problem of sleep. Certain phenomena have been well observed, but the organic condition, or sum of conditions, on which these depend, remains so entire a mystery, that we cannot even venture on a tolerable definition of Sleep. Let us, therefore, leave it undefined, and be content with noticing some of its leading characteristics.

Look at that child: wearied with play, he has thrown himself upon the ground, and, resting a flushed cheek upon one arm, he lies there breathing equably, with motionless limbs, eyes closed, brain shut out from the lights and noises around him. If you touch his hand, he will withdraw it; if you tickle his cheek, he will impatiently turn his head aside; but even should he turn his whole body round, he will not, perhaps, open his eyes—will not *know* who it is, or what it is, that molests him; he will not awake. His mind, engaged in dreams, is disengaged from external things: they may make impressions on him, excite sensations in him, but these sensations are not wrought up into knowledge.

His senses are dormant, or but feebly active, and his brain is occupied with dreams; his limbs are motionless, his fingers relax their grasp, and the muscles of his neck no longer support his head. But the heart beats vigorously, and pumps the blood incessantly all over the body, the chest expands and contracts, the stomach and intestines digest, all the secretions are going on.

We thus perceive how very superficial is the analogy of Sleep and Death, supposed by the ancient mythology to be brothers, and even by moderns supposed to resemble each other so closely that Death is called an eternal sleep. But, strictly speaking, there is not only no true antagonism between Sleep and Life, there is not even an antagonism between Sleep and Waking. In Death, *all* the activities peculiar to the vital organism cease; in Sleep they *all* continue: and if some of them are more languid and intermittent than during the waking hours, and if, in consequence of this languor, their mutual action and reaction are modified, this will constitute a difference, but not an antagonism. Sleep is a form of life, not a cessation of life. Indeed, Grimaud, Brandis, Fessel, Buffon, Burdach, and probably other physiologists, have declared Sleep to be the normal condition of Life, out of which it periodically passes into the waking condition; and certainly the whole of the life of the embryo is passed in sleep: nor does the infant, during the first months after birth, keep awake for more than brief periods; sleep is to it the rule, and waking the exception.

It would perhaps be more consistent with all our ideas to consider both Sleep and Waking as normal conditions of the vital organism, which periodically succeed each other.

After this survey, can we decide on any one characteristic which would serve as the sign of Sleep? The motionlessness of the limbs will not serve us, for limbs are motionless in coma, in repose, or in feigned sleep, and they are active in somnambulism. Nor will the stillness of the mind and senses serve us, for the mind is indubitably active during dreams, and quiet during coma; while the senses are almost as heedless of impressions during reverie. As we think, walk, and talk during sleep, it is clear that sleep must be some condition which permits all forms of vital activity, though not all the ordinary combinations.

The impossibility of specifying any one characteristic, disposes us to adopt Sir Henry Holland's view, that "sleep is not a unity of state, but a *series of fluctuating conditions*, of which no two moments are perhaps strictly alike."* This renders it intelligible that men are known to sleep on horseback, soldiers to sleep while marching, and village minstrels while playing to indefatigable dancers. It is a rapid oscillation between sleeping and waking. The muscles are in constant activity, and the Sensibility is sufficiently active to respond to delicate stimuli. I can see no reason for adopting the ordinary explanation of these actions during sleep, namely, that they are "automatic," or "secondarily automatic," if by these terms sensation is meant to be excluded (as it always is meant). On the contrary, it seems clear that in each case a suspension of the Sensibility would be followed by a cessation of the actions. The muscular sensibility must be excited, otherwise the reins would fall from the relaxed grasp of the rider, the musket would fall from the hands of the soldier, and the fiddle from the hands of the minstrel. This would at once cause them to wake up in alarm; or, if the stupor had become too intense, the men themselves would fall. The better explanation is that of Sir Henry Holland: "At every moment, the mind lapses into a dozing state, from which the loss of the balance of the body as frequently and suddenly arouses it. Neither the sleep nor the waking consciousness is perfect, but the mind is kept close to an intermediate line, to each side of which it alternately passes. No such line, however, really exists; and it is merely a rapid shifting to and fro of conditions of imperfect sleep and imperfect waking, giving curious proof of the manner in which these states graduate into one another."

---

* HOLLAND, *Chapters on Mental Physiology*, p. 81.

"Watch again," he continues, "the loss of voluntary power in a person sinking quietly into sleep—how gradual it is—how exact a measure of the state coming on. An object is grasped by the hand, while yet awake; it is seen to be held less and less firmly, till at last all power is gone, and it falls away. The head of a person in a sitting posture gradually loses the support of the muscles which sustain it upright; it droops by degrees, and in the end falls upon the chest. Here again we have proof of the rapidity with which the loss and recovery of voluntary power may alternate on the confines of sleep. The head falls by withdrawal of power from particular muscles. The slight shock thence ensuing partially awakens and restores this power, which again raises the head.

"The gradual changes which occur in the perceptions from the senses, while sleep is coming on, afford the same curious notices of the condition of the mind in its relations to the world without. The sight, the hearing, the touch, all show the progressive lessening of sensibility through every stage of change, with the same fluctuations which attend those of voluntary power; and giving similar proof that the state of sleep is ever varying in degree as respects these several functions. We find, for example, one condition of sleep so light that a question asked restores consciousness enough for momentary understanding and reply; and it is an old trick to bring sleepers into this state, by putting the hand into cold water, or producing some other sensation not so active as to awaken, but sufficient to draw the mind from a more profound to a lighter slumber. This may often be repeated, sleep still going on; but make the sound louder and more sudden, and complete waking at once ensues. The same with other sensations. Let the sleeper be gently touched, and he shows sensibility, if at all, by some slight muscular movement. A ruder touch excites more disturbance, and probably changes the current of dreaming; yet sleep will go on, and it often requires a rough shaking, particularly in young persons, before full wakefulness can be obtained."*

The series of fluctuating conditions which we name sleep—and which passes by insensible gradations from waking to dozing, from dozing to dreaming sleep, and from that (perhaps) even to dreamless sleep—is determined by the state of our organs. It cannot be brought on by any effort of our will, unless other predisposing causes co-operate. Every one knows the wretchedness of a sleepless night. On the other hand, no effort of our will can prevent sleep under certain conditions of the exhausted organism. Neither pain nor fear of death will suffice. Damiens slept on the rack, exhausted by torture; and many a victim to chronic pains knows the temporary refuge of sleep. Gunners fall asleep beside the booming cannon. An engineer has been known to fall asleep within a boiler which his comrades were beating outside with their ponderous hammers. Men sleep soundly the night before their execution; and they will rather brave death by sleep, than escape death by keeping awake. A curious example is recorded in the first volume of *Cook's Voyages.* Mr Banks and Dr Solander had been botanising among the hills of Terra del Fuego. On their return towards the ships, after travelling through considerable swamps, the weather became bitterly cold, accompanied by sudden blasts of piercing wind, and heavy snow. Finding it impossible to get back to the ships before morning, they resolved to push on through another swamp that lay in their way into the shelter of a wood, where they might build a wigwam and kindle a fire. Dr Solander, who had more than once crossed the mountains dividing Sweden from Norway, and who well knew that extreme cold, especially when joined with fatigue, produces a torpor and sleepiness which are almost irresistible, conjured his companions to keep moving, whatever pain it might cost them, and whatever relief they might be promised by an inclination to rest. "Whoever sits down," said he, "will *sleep;* and whoever sleeps, will *wake no more.*" Thus at once admonished and alarmed, they set forwards; but they had not gone far before the cold became so intense as to produce the effects that had been most dreaded. Dr Solander was the first who found the inclination, against which he had warned the others, invincible, and he insisted on being suffered to lie down. Mr Banks entreated and remonstrated with him to no effect. Down the Doctor lay upon the ground, and it was with difficulty he was kept from sleeping. One of the black servants, named Richmond, did the same; when he was told that if he did not go on he would soon be frozen to death, he replied that he desired nothing more than to lie down and die. The Doctor said he was willing to go on, but he must first take some sleep, although he had only recently told them that to sleep was fatal. After an angry altercation, the two were propped up against some bushes, and in a few minutes were profoundly asleep. Five minutes after, some of the men returned to say a fire was kindled a quarter of a mile farther on. Mr Banks then roused Dr Solander; but although he had only slept five minutes, he had almost lost the use of his limbs, and the flesh was so shrunk that his shoes fell from his feet. He consented to go forward with such assistance as could be given him, but no attempts to relieve poor Richmond were successful. He, together with another black left with him, died.

---

* HOLLAND: *Chapters on Mental Physiology,* p. 84.

Although we can neither fall asleep by willing it, nor keep from falling asleep under particular states of exhaustion, we have a certain power over the predisposing circumstances, and can thus help to bring on or ward off sleep. Nay more, we can induce a habit of going to sleep at any time, or of awaking at any time. Seamen and soldiers on duty soon learn this habit. They fall asleep almost immediately their "spell" comes. Napoleon could take "forty winks" at almost any hour of the day. Captain Barclay, during the performance of his famous feat of walking 1000 miles in 1000 hours, learned to fall asleep the instant he lay down. We all awake at the usual hour every morning, no matter what time we go to bed; and every one knows that, going to bed with a strong desire to wake at a certain unusual hour in the morning, we are tolerably certain to awake at that hour, or even before it.

Heat is one of the predisposing causes of sleep.

"We often witness this in the summer season, sometimes in the open air, but more frequently at home, and above all in a crowded church. An intolerable lassitude falls over the spirit; we are unable to walk, or move, or think; our eyes become heavy and languid; we are seized with yawning, and, resting upon the first suitable object which presents itself, drop into a profound slumber. This is perhaps the most rapid of all sleep, excepting that from apoplexy or narcotics. The mind seems in a few minutes to glide away, and sinks into a state of overpowering and almost instantaneous oblivion. The slumber, however, not being a natural one, and not occurring at the usual period, is seldom long: it rarely exceeds an hour, and when the person awakes from it, so far from being refreshed, he is usually dull, thirsty, and feverish, and finds more than common difficulty in getting his mental powers into their usual activity. A heated church and a dull sermon are almost sure to provoke sleep. There are few men whose powers are equal to the task of opposing the joint operation of two such potent influences. They act on the spirit like narcotics, and the person seems as if involved in a cloud of aconite or belladonna. The heat of the church might be resisted, but the sermon is irresistible. Its monotony falls in leaden accents upon the ear, and soon subdues the most powerful attention. Variety, whether of sight or of sound, prevents sleep, while monotony of all kinds is apt to induce it. The murmuring of a river, the sound of the Eolian harp, the echo of a distant cascade, the ticking of a clock, the hum of bees under a burning sun, and the pealing of a remote bell, all exercise the same influence. So conscious was Boerhaave of the power of monotony, that in order to procure sleep for a patient, he directed water to be placed in such a situation as to drop continually on a brass pan. When there is no excitement, sleep is sure to follow. We are all kept awake by some mental or bodily stimulus, and when that is removed our wakefulness is at an end. Want of stimulus, especially in a heated atmosphere, produces powerful effects; but where sufficient stimulus exists, we overcome the effects of the heat, and keep awake in spite of it."*

Cold prevents sleep, unless it be of sufficient intensity to produce internal congestion of the blood, in which case, as all readers of arctic travels well know, cold produces a stupor which is fatal. Heavy meals predispose to sleep, especially when the digestion is feeble.

Whatever tends to quiet the brain, tends to produce sleep; whatever tends to keep the brain in excitement, tends to prevent sleep; and that is why maniacs often remain sleepless for weeks.

II. DURATION OF SLEEP.—There are great varieties among individuals, both in the lightness of the slumber and the extent of its duration. There are heavy sleepers and light sleepers; there are those who require several hours, and those who require but few. It is unnecessary here to repeat the statements which uncritical writers have accepted respecting extraordinary achievements both as to extreme length and extreme shortness of sleep; but there is no doubt that many persons have been satisfied with five hours in the twenty-four, which is the amount attributed to John Hunter and Frederick the Great; while on particular emergencies a much shorter sleep seems to have sufficed. It is difficult to assign the degree of credit due to such statements as those of Pichegru, that during a year's campaign he had not slept more than one hour in every twenty-four. People make great mistakes respecting their own experience in such matters, and very seriously declare they have not slept many minutes, when we know them to have slept a couple of hours. It must, however, be admitted that even "forty winks" is an extraordinary refreshment, so much so as to render Pichegru's statement not wholly incredible. I have on several occasions, during nights of hard study, fallen asleep over a book, remained thus for some five minutes, and have awaked to find myself so reinvigorated that drowsiness did not recur during the next two or three hours; and every student, or watcher by the bedside of sickness, can doubtless recall similar cases. The following is the most remarkable example of this in my own experience. I had spent an exciting and fatiguing day in company with several distinguished artists and men of letters, rehearsing for private theatricals, and filling up the intervals with merriment, discussion, and excitement of various kinds. At one o'clock in the morning, after having performed in three pieces, I sat down with the rest to supper, which was not the least exciting part of the day. In the midst of the noise, the quips and cranks, the laughter and the jingling of glasses, I suddenly fell asleep. How many seconds this sleep lasted I cannot say; but they could have been but very few, since no one at the table, or on either side of me, was aware of the fact. It was a flash of unconsciousness. Suddenly I awoke, and felt as ready for fresh excitement as ever; nor did any sense of fatigue make me shorten our pleasant sitting. If sleep be caused, as seems most probable, by a temporary congestion, the foregoing case may be understood as a momentary congestion; the re-established circulation brought back with it mental activity.

---

* MACNISH: *Philosophy of Sleep*, p. 15.

Children sleep much. In old age sleep is generally slight and brief; but in extreme old age there is a sort of return to the sleepiness of infancy. It is well to bear these facts in view, although we are at present quite unable to explain them. Writers who find phrases as satisfactory as they are facile, and who care little about clearly understanding the processes they describe, will tell you that in infancy sleep is enormous in amount, because in infancy the processes of growth are so enormous, and it is during sleep that Nutrition goes on. They will further tell you that in extreme old age likewise sleep is excessive, because then the Nutritive processes are slower and feebler, and require this long repose for their accomplishment. But every inquirer who is really anxious to have some distinct understanding of the process, will feel, I think, very considerable difficulty in realising to his mind any such conception. There is, I am aware, a general belief, which is almost accepted as an axiom, that Nutrition takes place mainly, if not solely, during sleep, and that the purpose of sleep is to permit the waste of tissue to be repaired. But without venturing to speak decisively on a subject so very obscure as the process of Nutrition, I feel impelled to say that this general belief seems to me wholly unwarranted by any facts hitherto ascertained. On the contrary, as far as present knowledge enables us to pronounce, the truth seems to be that Nutrition goes on *incessantly*, in conjunction with destruction of tissue; and that during sleep a very considerable waste of tissue takes place. Only a false analogy could suggest that sleep was the period for the building up of the fabric, which had been worn out in waking activity. Physiology knows of no such definite periods for destruction and reconstruction.

Assimilation (or Nutrition) is a molecular process. Molecule by molecule the tissue is destroyed, and molecule by molecule it is repaired. These processes depend on chemical affinities, the one bringing about conditions which favour the other. No one really imagines the organism to be like a machine, which wears out in action, and is repaired when at rest. No one imagines that the tissues are only worn out during the hours of waking activity, and not at all worn out during the hours of sleep. Many *say* this, but do not picture it to their own minds. They do not imagine, for example, that the Duke of Wellington repaired in two or four hours of sleep the waste of twenty-two or twenty hours of anxiety and activity; and was the next day ready for the same amount of exertion and the same waste. Nor do they picture to themselves the condition of those who have remained sleepless for weeks, and even months—a condition which, as it must have been produced by all waste and no repair, would have made the tissues present very much the aspect of a moth-eaten coat. Above all, they do not consider the contradiction which exists between this position of theirs respecting Nutrition during sleep, and their other position that sleep is caused by a slight congestion of the brain, or by the slower and feebler circulation: since they are fully aware that congestion, or feeble circulation, is the very worst condition for active nutrition; and they must either give up the idea of sleep being the period of most active nutrition, or else give up the idea of sleep being caused by congestion, or feeble circulation.

What we know leads us to affirm that Assimilation is a continuous process; new material is incessantly being formed; old material is incessantly, but with varying intensity, being destroyed. The waste and repair of the scarf-skin may furnish an example. Incessantly new cells are in process of formation and development, which take the place of the outermost layer of cells as these fall off, dead. Should there be a greater waste than the repairing process can immediately supply, the skin is rubbed *bare;* and if the bare spot be rubbed, it becomes a *sore;* but if left undisturbed, it soon becomes covered with a fresh layer of protecting cells. It is thus with all the tissues: they grow, particle by particle; they act; they die particle by particle, and are replaced. The growth is not more rapid during sleep than during any other period; rather less, if anything. And that the destruction is not arrested during sleep, is positively proved by many striking facts—for example, by the amount of carbonic acid and excretory matters, and by the very obvious loss of substance in all the tissues which Valentin has shown to occur in the winter sleep of the hybernating animals. If, therefore, it can be shown that Assimilation goes on during all periods, and that waste of tissue goes on during all periods, the idea of sleep being a period specially allotted for the repair of the tissues must be given up.

And yet the fact remains that sleep *is* "tired Nature's sweet restorer." The fatigues of the day are vanquished during this repose. We awake reinvigorated. What has been the change? Müller speaks with caution on this point. "The excitement of the organic processes in the brain," he says, "which attends an active state of mind, gradually renders that organ incapable of maintaining the mental action, and thus induces sleep, which is to the brain what bodily fatigue is to the other parts of the nervous system. The cessation or remission of mental activity during sleep, in its turn, however, affords an opportunity for the restoration of *integrity to the organic conditions* of the cerebrum, by which they regain their excitability." But what are these organic conditions, and how is their integrity restored? Light seems to fall upon this question from the fact already recorded—namely, that the nerve of a frog's leg, separated from the body, becomes exhausted by stimulus, and will no longer cause the muscle to contract. If this nerve be allowed repose, it will gradually *regain* its lost power, and on the application of a stimulus it will cause a contraction. According to the vague conceptions of Nutrition which prevail, this looks as if the functions of a nerve could be restored without any influence of Nutrition; and it has been cited to prove that position, even by so eminent a physiologist as Virchow.* But I would suggest that Nutrition, being essentially the process of molecular Assimilation, will continue as long as there is any material to be assimilated; the circulation may be necessary for the conveyance of material to the tissues; but the material, once conveyed, is under the laws of Assimilation. The case of the over-stimulated nerve is probably not unlike that of an overstretched piece of elastic substance. If India-rubber be in a state of continued tension, its elasticity is destroyed; but alternate relaxations preserve its elasticity. Is not this the analogue of those alternate tensions and relaxations which the nerves and muscles of the heart, lungs, and alimentary canal exhibit; and the reason why these, although always in activity, are never fatigued? The action is not greater than the repose. The heart and chest expand, but they also contract: the muscles are as long in repose as they are in action. If our senses and voluntary muscles require longer intervals of repose, it is because their tension is longer; but even with them there is a kind of alternation; and we all know the unusual amount of fatigue which is produced by a very slight tension of the muscles, provided that tension be continuous. To hold the arm out straight for five minutes will produce more fatigue than to employ that arm in various movements for two hours. The same principle may be seen in intellectual or emotional excitement. Every student knows that diversity of work is a sort of relaxation, and that nothing is so exhausting as concentration of thought.

---

* VIRCHOW: *Die Cellularpathologie*, p. 263.

The conclusion to which the foregoing arguments point is, that Sleep, strictly considered, is a condition of repose of the brain and senses, produced by a slight congestion in their blood-vessels; and during this repose they recover that molecular integrity, or that molecular polarity, on which their vigorous action depends, and which has been disturbed during prolonged activity.

Sleep is caused by fatigue, but not because fatigue has wasted a large amount of tissue which needs repose for its restoration. It is caused by fatigue, because one of the natural consequences of continued action is a slight congestion; and it is the *congestion* which produces sleep. Of this there are many proofs. The causes which tend to produce sleep at unaccustomed hours, and before fatigue is felt, are all causes which lower the action of the heart, or which tend to a retardation of the circulation in the brain; the causes which tend to wakefulness are all causes which accelerate this circulation, or which diminish the pressure of the blood on the brain. If fatigue were in itself the cause of sleep, we could not be awakened until repose had restored the integrity of the nervous system; whereas it is notorious that just as we are dropping off, or only a short time after we have been asleep, an alarm of fire or thieves, a dash of cold water, a sudden pain, or even a strong light, will at once arouse us into energetic activity. This is a consequence of the relieved congestion and accelerated circulation—not a consequence of the few minutes' repose. The fatigue remains; it is the congestion which disappears. On the other hand, while fatigue causes sleep, by causing congestion, fatigue, if carried beyond a certain point, prevents sleep, by causing a feverish exaltation of the circulation. Every student and every sorrower knows what it is to be rendered sleepless by over-excitement of the brain. Let a man, wearied with bodily and mental labour, unable to keep from drowsiness, suddenly hear thrilling news, or alight on an illuminating track of thought, and the drowsiness will vanish as by enchantment—the wearied brain will have energy for hours of activity.

We conclude, therefore, that it is not *as* fatigue, but as producing congestion, that fatigue causes sleep; and the hybernating animal, which in full activity can be thrown into a state of sleep merely by a sudden fall in external temperature, and will continue thus until warmth accelerates its circulation, presents us with a striking illustration of such a conclusion. We conclude, moreover, that sleep is not produced by the necessity of repairing the waste of tissue, since that repair goes on irrespective of sleep. Finally, we conclude that sleep is not dependent on any need of repose, since it is exhibited in greatest intensity by those who least need repose, having least actively exerted themselves. It is not persons of active and excitable temperaments who are the greatest sleepers; it is infants, very old people, people of sluggish, indolent, lymphatic temperaments. And men, no less than animals, fall asleep when perfectly quiet, perfectly free from external or mental stimulus to excitement.

III. DREAMS.—It was my intention to have treated the subject of Dreaming in some detail, but the length to which the chapters on the nervous system have extended, forces me to be very brief, and to abstain altogether from illustrating the subject with the numerous stories which might have given it interest. It naturally encroaches on Psychology at almost every point; but the explanations hitherto offered, at least those which I have seen, have the initial defect of not being based on a proper understanding of the physiological process. Only a true philosophy of nervous action can give us the clue to the mystery of Dreaming. For the sake of brevity, I shall venture to assume that the exposition of nervous action given in the foregoing pages is the true one, and endeavour by its aid to explain Dreaming.

Dreams are mental processes carried on during sleep, and are closely allied to the Reveries carried on during waking hours. In sleep the external senses are almost entirely closed against their ordinary stimuli, and the active organs are in repose. The consequence is that all, or nearly all, those multifarious sensations which, through the Five Senses and the Muscles, stimulate the activity of the Brain during the waking hours, have no longer any influence in swelling the stream of Consciousness, or in determining the direction of the thoughts. We do not see objects, smell odours, hear sounds, or taste flavours; even the sense of Touch has no distinctness, such as would create the perception of objects, but is confined to a general feeling of contact with the bed-clothes. Now, whoever for a moment reflects on the immense influx of sensation which is incessantly stimulating the brain through the senses, during waking hours, and reflects on their influence in determining the direction of the thoughts and in furnishing the mind with materials, will see at once that the removal of such a source of excitation must very considerably *alter* the mental conditions. Instead, therefore, of marvelling that dreams should be incongruous with waking thoughts, our marvel must be to find so much congruity as we often find between them.

To understand Dreaming, we must try and discover what the action of the brain would be under such conditions as are present in sleep; and we can only do so by analogies drawn from our waking experience, coupled with a correct interpretation of nervous action in general. For example, in our waking condition, we are familiar with what has been styled *subjective* sensations; that is to say, we *see* objects very vividly, where no such objects exist; we *hear* sounds of many kinds, where none of their external causes exist; we *taste* flavours in an empty mouth; we *smell* odours, where no volatile substance is present; and we *feel* prickings or pains in limbs which have been amputated. These are actual, not imaginary, sensations. They are indistinguishable from the sensations caused by actual contact of the objects with our organs. They are sometimes so intense, or accompanied by a cerebral excitement which so completely domineers over the controlling suggestions of other senses, that they produce Hallucinations. And as it is the inevitable tendency of our nature to connect every sensation with an external cause—to project it outside of us, so to speak,—we should never think of doubting that every one of these subjective sensations had a corresponding object, did not the suggestions of some *other* sense control this idea. A man feels prickings in his amputated fingers, but he sees that the fingers are not there, and, consequently, he knows that his sensation is deceptive. He smells the horrible stench of a sewer long after he has passed out of the reach of its volatile gases. He tastes the bitter flavour long after the bitter substance has been removed. But the sensations require constant confrontation with the reports of other senses, otherwise they would be credited as sensations produced by actual objects. In the state of cerebral excitement named Hallucination, this confrontation is *disregarded;* in the state of cerebral isolation named Dreaming, this confrontation is *impossible:* the first condition is one in which the cerebral activity completely domineers over the excitations from without; the second condition is one in which the cerebral activity, though feeble, is entirely isolated from external excitations—thus, in both cases, the cerebral reflexes are undisturbed, uncontrolled by reflexes from Sense.

It is the fact that sensations may be subjective—in other words, that they may arise from internal stimuli no less than from external stimuli, and *must* arise whenever the centres are excited: it is this fact which gives us the clue to Dreaming. The avenues of Sense are closed in Sleep, but the sensational centres may be reached from within. That Law of Sensibility, which has been so fully expounded in previous pages, whereby every sensation discharges itself either in a reflex-action or a reflex-feeling (or in both together), and whereby every centre, once stimulated, must inevitably stimulate some other, gives us the explanation why subjective sensations may arise in sleep, or waking, and why they must stimulate cerebral action. Moreover, the external senses have not their avenues completely closed in sleep; and a sound will be heard by the sleeper, though dimly; a light will be seen, or a touch felt. These dim sensations are reflected in his dreams. The coldness of a touch will cause him perhaps to dream that he has grasped a corpse; the rattling of the windows will suggest a storm, or a battle. There is a story told of a lady who dreamt that her servant was coming to murder her; she opened her eyes in terror, and saw the servant at her bedside, knife in hand! The explanation of this is, probably, that she heard in sleep the creaking of the footsteps on the stairs, or the opening of the door; and this sensation, which might have suggested any one of a thousand different trains of thought, happened to suggest (perhaps because the idea was not unfamiliar to her mind) the idea of the servant's intention to murder her. All our experience, both of our own dreams and those of others, ratifies the position, that the train of thought may be, and must be, determined by any sensation felt during sleep; but inasmuch as this sensation cannot be confronted with the actual cause, inasmuch as the other sources of verification are closed, which in waking hours enable us to connect any particular sensation with a particular cause, the perceptions and ideas it will give rise to are usually inaccurate and incoherent. If, when awake, we perceive an odour of something burning, we at once look about to discover the cause. If, when asleep, we perceive this odour, we cannot thus confront the sensation with its external cause; but the sensation suggests a train of thought, and we dream that the house is on fire, or that we are at work in a manufactory. If, when awake, we feel a cold draught of air blowing upon us, we look at the door or window to see whence it comes; but asleep, the sensation of cold will suggest a dream that we are in the street in our night-shirt, or riding outside a coach; the sensation starts a train of ideas—and that is a dream.

If we reflect that the nervous centres must be incessantly called into activity, either through the imperfectly-closed channels of the Five Senses, or through the Systemic Senses, and that these centres, once excited, must necessarily play on each other—and if we reflect farther, that the sensational and ideational activities thus stimulated operate under very different conditions, and in very different conjunctions, during sleep, we shall be at no loss to understand both the incoherence and the coherence of dreams—the perfect congruity of certain trains of thought amid the most absurd incongruities. The coherence of dreams results from the succession of associated ideas; the train of thought follows very much the course it would follow in waking moments, at least when uncontrolled by reference to external things—as in Reverie. The incoherence results from this train being interrupted or diverted from its course by the suggestion of some other train, either arising by the laws of association, or from the stimulus of some new sensation. And because in Dreams, as in Reverie, we do not pause on certain suggestions, do not recur to them, and reflect on them, but let one rapidly succeed another, like shadows chasing each other over a cornfield, we take little or no heed of any incongruities. It is constantly said that in dreams nothing surprises us. I think this is a mistake. Nothing *arrests* us; but every incongruity surprises us, at least as much in dreams as in reveries. I am distinctly conscious of this in my own experience. If when I dream that I am in a certain place, conversing with a certain person, I am also aware that the place suddenly becomes another place, and the person has a very different appearance, a slight surprise is felt as the difference is noted, but my dream is not arrested; I accept the new facts, and go on quite content with them, just as in reverie the mind passes instantaneously from London to India, and the persons vanish to give place to very different persons, without once interrupting the imaginary story. In dreams no perception is confronted with actual objects; no ideas are confronted with present existences. The waves of sensation and of thought succeed each other, and are interrupted, broken, diverted by the fresh streams of sensation constantly pouring in from the changing states of the system. This interruption is to some extent controlled in waking moments by the presence of objects both to the senses and the mind; yet even the presence of objects, and the energetic resolution which a strong motive will give, will not prevent the most steadfast mind from continually wandering, although the mind may be recalled from its wanderings to the subject originally occupying it. If I sit in my study, and my thoughts wander to Bagdad or Bussora, the continual presence of my books, chairs microscope, engravings, &c., infallibly bring me back again before long, and prevent my believing myself to be in the East. If when I am working out some plan, or thinking of some problem, the thoughts wander away, lured by some accidental association, they are soon recalled again by the suggestions of the paper, portfolio, desk, or even by the very attitude in which I am sitting, and I recommence. But, asleep, this recall would never take place. The objects are not there to suggest the requisite thoughts. The sensations I receive are carried along with my dream, each succeeding sensation or idea having a diverging influence; and thus a dream is never long, never very coherent. If the reader wishes to understand this operation of the mind, let him attend to the fluctuating stream of thought which passes through his brain as he lies awake in bed, with his eyes closed, and in perfect stillness; he will soon be aware of the influence which must be exercised by the presence of external objects in determining the direction of his thoughts.

That our dreams are greatly influenced by the Systemic Sensations, will be evident to every one who reflects on the notorious effects of indigestion. The state of the secretions also manifestly determines many dreams. The wretched sufferers in the shipwreck of the Medusa, dying from thirst, had perpetual dreams of shady woods and running streams. The nursing mother is made to dream of her child by the flow of milk to her breasts.

If, therefore, it is certain that Systemic Sensations produce dreams, and certain also that, in spite of the inactivity of the Five Senses, they can, one and all, furnish their accustomed sensations, if excited by external objects, and also if their centres are excited by internal stimuli; and if, farther, it is admitted that sensations determine trains of thought, we shall find little difficulty in assenting to the proposition, which many psychologists deny, that even in the deepest slumber dreams occur. The physiological reasons for supposing that the brain is active during sleep, though not perhaps so active, nor having all its various parts in such perfect co-ordination, as during waking hours, seem to me very strong, although the point scarcely admits of proof. Whereas the only strong argument against it is that we have seldom any consciousness of having dreamed, some persons, indeed, declaring that they never dream. This argument is, however, weakened by the fact that somnambulists, who not only dream, but *act* their dreams, and often in a very surprising manner, never remember these dreams when awake. It thus appears that our forgetfulness of dreams is no proof of dreamless sleep; and Sir William Hamilton declares that, having experimented on his own person, and caused himself to be roused at different hours of the night, he was always able to observe that he was in the middle of a dream. "The recollection of this dream was not always equally vivid. On some occasions I was able to trace it back until the train was gradually lost at a remote distance; on others, I was hardly aware of more than one or two of the latter links of the chain; and sometimes was scarcely certain of more than the fact that I was not awakened from an unconscious state."[*]

In describing Sleep we had occasion to notice the partial though oscillating congestion which characterises it. In proportion as this congestion is extensive will the sleep be profound, and the dreams necessarily imperfect and fragmentary; for the effect of congestion will be to prevent certain parts of the nervous mechanism from being stimulated by the others; and thus, instead of consecutive trains of thought, there will only be irregular and imperfect trains called up by the various sensations.

---

[*] HAMILTON: *Lectures on Metaphysics*, i. 323.

CHAPTER XII.

THE QUALITIES WE INHERIT FROM OUR PARENTS.

The contradictory statements current—Mr Buckle's rejection of the Law of Inheritance examined—The general characteristics which are inherited—Differences in families—Peculiarities inherited—Are the effects of accident transmitted?—Habits and tricks inherited —Influence of the second parent—Vices, Diseases, Idiosyncrasies inherited—Longevity inherited—Predisposition to Insanity—The respective influences of father and mother—Refutation of the notion that the father gives the animal organs, and the mother the vegetal organs—Curious examples—Children of great men—Necessary fusion of the parental influences—Perturbing causes: Atavism; Potency of the individual; Age; Sex—Summary of the evidence—Is marriage justifiable where the hereditary "taint" exists.

"THAT boy is the very image of his mother!" is the exclamation frequently heard; and not less frequently, "That boy is remarkably unlike his parents!" We also hear it said, "He has his father's talent, or his mother's sharpness;" and conversely, "He has none of the family talent." That the sons of remarkable men are generally dunces, and that men of genius have remarkable mothers, are two very questionable statements which have become proverbial.

WIDENER LIBRARY

Harvard College, Cambridge, MA 02138: (617) 495-2413

If the item is recalled, the borrower will be notified of the need for an earlier return. (Non-receipt of overdue notices does not exempt the borrower from overdue fines.)

WIDENER
SEP 1 1 2006
NOV 0 8 2006
BOOK DUE
CANCELLED

Thank you for helping us to preserve our collection!

no shadow of doubt that organisations are inherited. He will not say that it is a mere coincidence which preserves intact the various "breeds" of animals: which makes the bull-dog resemble the bull-dog, and the bull-terrier resemble the bull-dog and the terrier; which make the Jews all over the world resemble Jews, because they keep their race free from admixture, by never marrying into other races; which gives us short-horned cattle, and fan-tailed pigeons; and which makes the pedigree of a horse, or dog, a value estimable in hard cash. Unless parents transmitted to offspring their organisations, their peculiarities and excellencies, there would be no such thing as a breed, or a race. The cur would run the same chance as the best-bred dog of turning out valuable. The greyhound might point, and the cart-horse win the Derby. Daily experience tells us that this is impossible. Science tells us that there is no such thing as chance. Physiology tells us that the offspring always, and necessarily, inherits its organisation from the parents; and if the organisation is inherited, then with it must be inherited its tendencies and aptitudes. Mr Buckle seems to have been misled by that which conceals the fact of transmission from ordinary apprehension, namely, the very great number of instances in which the offspring does *not* closely resemble either parent; or rather, in which the resemblance is not *discernible* by us. If the law of transmission is not a figment, these seeming contradictions are susceptible of explanation; and in the course of the brief survey which will here be given, I hope to be able to convince the reader that an explanation is possible.[*]

We must first note the indubitable fact that the organisation of the offspring always and necessarily resembles that of the parents in its *general* characters. So uniform is our experience of this constancy, that nothing would be more incredible than that negro parents should give birth to a child, with the straight hair, aquiline nose, small heels, &c. of an European; or that two sheep should produce a goat. But while there is this constancy in the transmission of *general* characters, there is considerable variation in the transmission of *individual* peculiarities. One of the negro parents may be tall, robust, joyous, stupid; the other short, feeble, querulous, clever: now as the child cannot be at once short and tall, clever and stupid, feeble and robust, in inheriting its parents' organisations it may either resemble one of the parents more than the other, or may apparently resemble neither, by being a mingling of the two. It is this fact of double parentage, and double inheritance, with an inequality in the amount of influence exercised by each parent, which complicates the question, and produces the seeming contradictions to the law of transmission. Let two Jews produce offspring, and inasmuch as both parents have the Jewish physiognomy, the offspring will be unmistakably Jewish; but let a Jew and a Saxon produce offspring, and the mingling of these two different organisations will be as visible in the offspring as it is in a Mulatto, or in any other cross breed.

---

[*] The materials for this chapter are drawn from the extensive but uncritical work of PROSPER LUCAS: *Traité de l'hérédité naturelle*, 1847-1850; the lectures *On the Physiology of Breeding*, by Mr ORTON, 1855; the *Traité de la Génération*, by GIROU DE BAZAREINGUES, 1828; the work by M. MOREAU: *La Psychologie Morbide*, 1859; and an article, by the present writer, on *Hereditary Influence*, in the July number of the *Westminster Review*, 1856.

People often express surprise at observing the strange differences in aspect and disposition between brothers bred up together in the same nursery, and under similar influences. From Cain and Abel, to the brothers Buonaparte, the diversity in families has been a standing marvel. Nay, such diversities are observed not only between brothers, but between twins; and it was noticed in the striking case of the celebrated Rita and Christina, twins who were so *fused together* that they had only one body and one pair of legs between them, with two heads and four arms, yet they manifested very different dispositions and tempers.* The same was observed of the Presburg twins, the Siamese twins; and of the African twins, recently exhibited in London. The cause of these diversities is the inequality with which the parental organisations were inherited; both parents contributed their elements, but these elements were differently compounded.

It is to this inequality in the influence of a particular parent that we must attribute the fact that, while certain peculiarities, trifling and even whimsical, are sometimes seen to be transmitted, they are not uniformly transmitted. I am disposed to receive with great scepticism many of the cases which have been advanced, without in the least swerving from my position that even trifling peculiarities are inherited. Plutarch speaks of a family in Thebes, every member of which was born with the mark of a spear-head on his body: Plutarch is not a good authority for such a fact; yet we need not summarily reject it. An Italian family had the same sort of mark, and hence bore the name of *Lansada*. Haller cites the case of the Bentivoglio family, in whom a slight external tumour was transmitted from father to son, which always swelled when the atmosphere was moist. Again, the Roman families *Nasones* and *Buccones* indicate analogous peculiarities; to which may be added the well-known "Austrian lip" and "Bourbon nose." All the Barons de Vessins were said to have a peculiar mark between their shoulders; and by means of such a mark, La Tour Landry discovered the posthumous legitimate son of the Baron de Vessins in a London shoemaker's apprentice. If these cases be received with an incredulous smile, we must remember the series of indisputable facts noticed in the breeding of animals. Every breeder knows that the colours of the parents are inherited; that the spots are repeated, such as the patch over the bull-terrier's eye, and the white legs of a horse or cow; and Chambon * lays it down as a principle derived from experience, that by choosing the parents you can produce *any* spots you please. Girou noticed that his Swiss cow, white, spotted with red, gave five calves, four of which repeated exactly the spots of their mother; the fifth, a cow-

---

* GEOFFROY ST HILAIRE: *Philosophie Anatomique*, ii.; and SERRES: *Recherches d'Anatomie transcendante.*

* *Traité de l'Education des Moutons*, i. 116.

calf, resembling the bull. Do we not all know how successful our cattle-breeders have been in directing the fat to those parts of the organism where gourmandise desires it? and have not sheep become moving cylinders of fat and wool, merely because fat and wool were needed?

More questionable are the facts of *accidents* becoming hereditary. A superb stallion, son of *Le Glorieux*, who came from the Pompadour stables, became blind from disease: all his children became blind before they were three years old. Burdach cites the case of a woman who nearly died from hæmorrhage after blood-letting; her daughter was so sensitive that a violent hæmorrhage would follow even a trifling scratch; she, in turn, transmitted this peculiarity to her son. Horses marked during successive generations with red-hot iron in the same place, are said to transmit the visible traces of such marks to their colts. A dog had her hinder parts paralysed for several days by a blow; six of her seven pups were deformed or excessively weak in their hinder parts, and were drowned as useless.* Treviranus † cites Blumenbach's case of a man whose little finger was crushed and twisted, by an accident to his right hand: his sons inherited right-hands with the little finger distorted.

These latter cases, I confess, have an air of suspicion, which almost justifies Mr Buckle's assertion that the evidence for hereditary transmission is founded on mere empirical coincidences. It is not only physiologically unintelligible how the effect of an accident could be thus transmitted; but it is incredible that, were such transmission within the course of nature, men should for centuries have lost arms and legs without becoming the progenitors of armless and legless babies; or how for generations the terrier's ears and tails should be cut short, yet terrier pups never come into the world ready cropped. The savages who tattoo their bodies, and bore holes through their noses, do not bring tattooed babies into the world. The truth of the matter seems to be this: The organisation of the parent is transmitted, and with that organisation all those characters and tendencies which the organisation in activity would naturally manifest. A habit, or trick, which has been acquired, and so long established that it may be said to be *organised* in the individual—whose mechanism has *grown* to its performance—will stand the same chance of being inherited, as the bulk of bone and muscle, or the sensibility of the nervous system. An idiosyncrasy which results from some organic disposition—say, for example, the repugnance to animal food—may as easily be inherited as a good constitution, or a scrofulous tendency. Explain it as we may, there is no fact more certain than that a habit once firmly fixed, once "organised" in the individual, becomes almost as susceptible of transmission as any normal tendency. Pointer pups inherit the aptitude, *i.e.* the organisation, fitting them for easily learning to "point;" and this aptitude is sometimes so strong that they will point before they have been taught. It is the same with dogs that have been taught to "beg." I had a pup, taken from its mother at six weeks old, and before, therefore, it could have learned to beg from her, which spontaneously took to begging for everything it wanted; and one day I found it opposite a rabbit-hutch, begging, apparently, the rabbits to come and play. Girou relates that he knew a man who had the habit of sleeping on his back, with his right leg crossed over his left. One of his daughters showed the same peculiarity from her birth upwards, and constantly assumed the attitude in her cradle. Venette knew a woman who, without being lame, had a sort of limp in her right leg; her daughter had the same defect in her right leg. Every one's experience will furnish examples of trifling peculiarities of manner — too individual to be mistaken — which are manifested by children who have never seen the parents they imitate. Nor is there anything surprising in this. The habit or manner, the attitude or trick, results from some peculiarity in the bodily framework, congenital or acquired, and this peculiarity is transmitted in the framework. It would *always* be transmitted were there not the counter-influence of a second parent, whose organisation is also inherited. This second parent has not the peculiarity, and the peculiarity may therefore be counteracted by her influence. Two pointers will produce pups that easily learn to point, or even do so spontaneously; but if a pointer be crossed with a setter, it is very likely that some of the pups will not point at all, although some may inherit the parental tendency. If a man of great musical aptitude marry a woman with none, it is probable that of two children one will inherit the musical aptitude, and the other be as insensible as the mother; but it is also probable that both will inherit the aptitude, or that neither will. Whenever we observe rigorous constancy in the transmission of qualities—as in the breeds of animals—the secret is that both parents had more or less of these qualities. Whenever we observe inconstancy in the transmission, the secret is, that only one parent had the qualities; and inasmuch as both parents transmit their organisations, the double influence determines the product.

---

* GIROU, p. 127.

† *Biologie*, iii. 452.

Instead, therefore, of feeling any surprise at a quality not being inherited when only one parent had that quality, we must anticipate such a result being very frequent; and our attention should be rather fixed on the numerous cases in which the quality is transmitted in spite of the influence of the other parent. Two consumptive parents will inevitably bring forth consumptive, or scrofulous, children; but one consumptive and one vigorous parent will bring forth children, *none* of whom may be consumptive or scrofulous, or only *some* of them, or *all* of them. These variations throw no doubt on the law of inheritance, but are in strict conformity with it; because no sooner are the disturbing influences removed than the law acts with unvarying uniformity. The evidence of direct transmission is ample, not only in the case of "breeds," but in the case of individual peculiarities. Thus Girou relates the case of a sporting-dog, taken young from its mother and father, who was singularly obstinate, and exhibited the greatest terror at every explosion of the gun, which always excites the ardour of the species. On the owner expressing his surprise to the gentleman from whom he received the dog, he was told that nothing was more likely, for the dog's father had the same peculiarity. How the vicious disposition of horses is transmitted, all breeders know. Again, we know that the vice of drunkenness is very apt to be inherited, and that the passion for gambling is little less so. "A lady with whom I was very intimate," relates Da Gama Machado, "and who possessed great wealth, passed her nights in gaming: she died young, from pulmonary disease. Her eldest son was equally addicted to play, and he also died of consumption at the same age as his mother. His daughter inherited the same passion and the same disease." * Tendencies to particular vices are inherited, and are exhibited in cases where the early death of the parents, or the removal of the children in infancy, prevents the idea of any imitation or effect of education being the cause. That the organisation of a thief is transmitted from father to son through generations, seems tolerably certain. Gall † has cited some striking examples. And murder, like talent, seems occasionally to run in families. Parents with an unconquerable aversion to animal food have transmitted that aversion; and parents, with the horrible propensity for human flesh, have transmitted the propensity to children brought up

---

* *Théorie des Ressemblances*, p. 154, quoted by Lucas.

† *Fonctions du Cerveau*, i. 207.

away from them under all social restraints. Zimmermann cites the case of a whole family upon whom coffee acted as opium acts on others, while opium had no sensible effect whatever on them; and Dr Lucas knows a family upon whom the slightest dose of calomel produces violent nervous tremblings. Every physician knows how both predisposition to, and absolute protection against, certain specific diseases are transmitted. In many families the teeth and hair fall out before the ordinary time, no matter what hygiène be followed. Sir Henry Holland remarks, "The frequency of blindness as an hereditary affection is well known, whether occurring from cataract or other diseases of the parts concerned in vision. The most remarkable of the many examples known to me, is that of a family where four out of five children, otherwise healthy, became totally blind from amaurosis about the age of twelve; the vision having been gradually impaired up to this time. What adds to the singularity of this case is the existence of some family monument long prior in date, where a female ancestor is represented with several children around her, the inscription recording that all the number were blind." * Unless we are to suppose all these cases simple coincidences, we must admit individual heritage; but the doctrine of probabilities will not permit us to suppose them coincident. Let us take the idiosyncrasy of cannibalism, which may be safely said not to appear more

---

* *Medical Notes and Reflections*, p. 23. Compare also a striking passage in Dr INMAN'S *Foundation for a New Theory of Medicine*, p. 5.

than once in ten thousand human beings: if, therefore, we take one in ten thousand as the ratio, the chances against any man manifesting the propensity will be ten thousand to one, but the chances against his son also manifesting it will be—what some more learned calculator must declare.

We can even produce a race having any peculiarities we will, provided we have parents possessing the germs of these qualities. M. Danney made experiments during ten years with rabbits, a hundred couples being selected by him with a view to the creation of peculiarities. By always choosing the parents, he succeeded in obtaining a number of malformations according to his preconceived plan. And such experiments have been repeated on dogs, pigeons, and poultry with like success. It is on this fact of individual heritage that longevity depends. There is no term of life for the "species," only a term for the individual—a fact which sets the speculations of Cornaro, Hufeland, and Flourens at nought. There are limits which neither the "species" nor the individual can be said to pass: no man has been known to live two hundred years; but the number of years which each individual will reach, without accident, is a term depending neither on the "species," nor on his own mode of life, but on the organisation inherited from his parents. Temperance, sobriety, and chastity, however desirable, both in themselves and in their effects, will not insure long life; intemperance, hardship, and irregularity will not prevent a man living for a century and a half. The facts are there to prove both propositions. Longevity is an inheritance. Like talent, it may be cultivated; like talent, it may be perverted; but it exists independently of all cultivation, and no cultivation will create it. Some men have a talent for long life.

M. Charles Lejoncourt published, in 1842, his *Galerie des Centenaires,* in which may be read a curious list of examples proving the hereditary nature of longevity. In one page we have a day-labourer dying at the age of 108; his father lived to 104, his grandfather to 108, and his daughter, then living, had reached 80. In another we have a saddler whose grandfather died at 112, his father at 113, and he himself at 115. This man, aged 113, was asked by Louis XIV. what he had done to so prolong life; his answer was—"Sire, since I was 50, I have acted upon two principles: I have shut my heart and opened my wine-cellar." M. Lejoncourt also mentions a woman then living, aged 150, whose father died at 124, and whose uncle at 113. But the most surprising of the cases cited by Lucas is that of Jean Golembiewski, a Pole, who in 1846 was still living, aged 102, having been eighty years as common soldier, in thirty-five campaigns under Napoleon, and having even survived the terrible Russian campaign, in spite of five wounds and a soldier's recklessness of life. His father died aged 121, and his grandfather 130. Indeed, the practice of every annuity and insurance office suffices to convince us of ordinary experience having discovered that length of life is somehow dependent on hereditary influence. That the predisposition to insanity and nervous diseases is inherited, every one knows.

M. Moreau has cited the following examples from his notes:—At Bicêtre there is now a monomaniac, whose paternal grandmother died mad; his father died mad; his maternal aunt is epileptic; his eldest brother had melancholy madness, which terminated in suicide; his sister is subject to attacks of hysterical epilepsy. Another inmate of the same asylum is subject to dangerous hallucinations; his father, although not precisely insane, is eccentric almost to a degree of insanity; his eldest brother is excessively irritable and violent, and becomes almost maniacal under the smallest excess of alcoholic stimulants; four of his paternal uncles committed suicide. A third case is of an epileptic woman, who has given birth to eight children; three died early from convulsions; one of the remaining five is hysterical, and another epileptic. This woman's sister died *of grief*, aged twenty-five. Her father died mad in an asylum. Her grandfather was insane. Her mother died of consumption, and was slightly epileptic.*

If these cases stood alone, we should suspect something more than coincidence underlying them; but every physician knows that such cases are frequent. No one expects that two hacks will produce a racehorse; two mongrels a pure hound; two scrofulous parents a strong healthy child; two irascible, excitable parents, a gentle, placid child; two semi-idiots a child of genius. Every one expects that the qualities of the parents will be inherited by the child, and when they are not inherited, we assume that there has been some disturbing influence.

---

* MOREAU: *La Psychologie Morbide*, p. 139.

But now comes the difficult part of our inquiry. Which is the predominating influence, the male or the female? If both parents join to form the child, does one parent give one group of organs, and another parent another group, or do both give all?

Speaking of mules, Vicq-d'-Azyr says, with proper caution, " It seems as if the exterior and the extremities were modified by the father, and that the viscera emanate from the mother." The reserve with which the great anatomist expresses himself has not been imitated by his successors. Indeed, it is noticeable that men in general dislike uncertainties and qualified statements; they prefer decisive opinions, distinct formulas. On this subject they have preferred the distinct formula, which has been also adopted by Mr Orton, in his Lectures, namely, " that the male gives the external configuration—or, in other words, the locomotive organs; while the female gives the internal—or, in other words, the vital organs." This is the theory generally adopted; but although it has the countenance of authority, an extensive survey of the facts, and a more accurate study of embryology, seem to me wholly adverse to it.

Linnæus said that the *internal* plant (that is to say, the organs of fructification) is in all hybrids like the female; the *external* organs, on the contrary, resemble those of the male. This statement is diametrically opposed by de Candolle, who announces it as a law that the organs of vegetation are given by the female, those of fructification by the male. When two doctors of such eminence differ on a point like this, we may reasonably suspect that both are right, and both are wrong; and here our suspicion is supported by the mass of facts adduced in the experiments of M. Sagaret, which refute the hypothesis of Linnæus, and the hypothesis of de Candolle. What we have just indicated with regard to plants, has been the course pursued with regard to animals: one class of observations has seemed to prove that the father bestows the "animal system;" another class of observations has seemed to prove that the mother bestows it; and a third class has proved both theories inadequate. Quite recently General Daumas published the result of his long experience with Arab horses,* arguing that, according to the testimony of the Arabs, the stallion was the most valuable for purposes of breeding. Upon this, the *Inspecteur des Haras,* who had traversed Asia for the express purpose of collecting evidence on the subject, published his diametrically opposite conclusion, declaring that it was the mare whose influence preponderated in the foal. General Daumas replied, and cited a letter addressed to him by Abd-el-Kader, who may certainly be said to understand Arab horses better than Europeans. The letter is worth reading for its own sake; we can, however, only quote its testimony on the particular point now under discussion. "The experience of centuries has established," he says, "that the essential parts of the organisation, such as the bones, the tendons, the nerves, and the veins, are always derived from the stallion. The mare may give the colour and some resemblance to her structure, but the principal qualities are due to the stallion." This is very weighty testimony, on which we will only for the present remark, that it merely asserts the *preponderance* of the male influence as respects the locomotive system; it does not assert that absolute independence of any female influence maintained in the formula which we are now combating. Abd-el-Kader's statement is tantamount to that made by Mr Orton,—"I do not mean it to be inferred that either parent gives either set of organs uninfluenced by the other parent; but merely that the leading characteristics and qualities of both sets of qualities are due to the male on the one side, and to the female on the other, the opposite parent modifying them only."

---

* *Les Chevaux de Sahara;* see also an article in the *Revue des Deux Mondes,* May 1855, on "Le Cheval de Guerre."

This is a much more acceptable theory than the other, but it is only an approximation to the truth. Mr Orton's first illustration is the hybrid of the horse and ass: "It is known that the produce of the male ass and the mare is a mule; but I do not think it is equally well known that the produce of the stallion and the female ass is what has been denominated a hinny—yet such is the case. . . . . The mule, the produce of the ass and mare, is essentially a modified ass—

the ears are those of an ass somewhat shortened—the mane is that of an ass—the tail is that of an ass—the skin and colour are those of an ass somewhat modified—the legs are slender, the hoofs high, narrow, and contracted, like those of an ass. The body and barrel are round and full, in which it differs from the ass and resembles the mare."

This description is accurate, but—we put it interrogatively—is it *always* the description of a mule, and *never* that also of a hinny? This latter, the produce of the stallion and the female ass, "is essentially a modified horse—the ears like those of a horse somewhat lengthened—the mane flowing—the tail bushy like that of a horse—the skin is fine like that of a horse—the legs are stronger, and the hoofs broad and expanded like those of a horse. The body and barrel are flat and narrow, in which it differs from the horse, and resembles its mother the ass." From these facts Mr Orton deduces the conclusion, that the offspring of a cross is not simply a mixture of the two parents, nor is it an animal that has accidentally a similitude to one or other of its parents, inasmuch as we can produce at will either the hinny or the mule. The reader will presently see why such a conclusion cannot be accepted; and we may at once anticipate what will hereafter be more fully explained, by saying that the differences Mr Orton signalises are easily interpreted by another theory. In point of fact, both mule and hinny are modified asses: in each the structure and disposition of the ass predominates; and it does so in virtue of that greater "potency of race" which belongs to the ass—a potency which is less effective on the hinny, because the superior vigour of the stallion modifies it, according to ascertained laws.

"I would call your consideration," Mr Orton continues, "to a very curious circumstance pertaining to the voice of the mule and the hinny; to which my attention was called by Mr Lort. The mule *brays*, the hinny *neighs*. The why and wherefore of this is a perfect mystery, until we come to apply the knowledge afforded us by the law I have given. The male gives the locomotive organs, and the muscles are amongst these; the muscles are the organs which modulate the voice of the animal: the mule has the muscular structure of its sire the ass, and brays; the hinny has the muscular structure of its sire the horse, and neighs."

This seems decisive, until we extend our observations, and then we find the law altogether at fault. The produce of a dog and a she-wolf sometimes bark and sometimes only howl, according to Buffon; and the produce of a bitch-fox and a dog, according to Burdach, barked like a dog, though somewhat hoarsely, and howled like a wolf when it was hurt. A similar remark has been made by all who have attended to cross-breeding in birds; the hybrid of the goldfinch and the canary has the song of the goldfinch mingled with occasional notes of the canary, which seem perpetually about to gain the predominance.

These illustrations, apart from their interest, teach us to be cautious in generalising from a few facts, however striking, in questions so complex as all biological questions are. Let us, however, continue to call on Mr Orton for facts. He quotes a letter from Dr George Wilson to Dr Harvey, respecting the produce of the Manx cat and the common cat. The Manx cat has no tail, and is particularly long in the hinder legs. "You will see," says Dr Wilson, "from the facts communicated, that where the Manx cat was the mother, the kittens had tails of a sort; where the Manx cat was the father, three-fourths of the kittens had no tail." Mr Orton also quotes a communication made to him by Mr Garnett, of Clitheroe:—"From these I select those pertaining to the Muscovy duck and some hybrids produced between it and the common duck. You are aware that the Muscovy drake exceeds in a striking degree the duck in size; the drake weighing from 8 to 9¼ lb., while the duck weighs only from 3 to 4 lb. Hybrids produced from the Muscovy drake and common duck followed this peculiarity of the male parent as to the relative size of the male and female hybrids; the male weighing from 5 to 6 lb., the females not half as much. On the other hand, the difference in the size of the sexes when the hybrids were the produce of the common drake and the Muscovy duck, was not apparent."

A valuable observation, certainly. Mr Orton adds the following of his own. He placed a Cochin cock with his common hens, "reasoning that if the vital organs were due to the female, then the cross between these birds (being externally Cochins and internally common hens) should lay white eggs, the secretion of the egg being a vital function. You know that the Cochin lays a chocolate-coloured egg. The half-breed did what theory said they should do—laid white eggs; and not only white eggs, but eggs also which, on the evidence of myself and family, were very inferior in taste, having lost the mellow, buttery taste of the Cochin egg."

But he has recorded another curious fact respecting this same experiment, which might have made him aware of the problematical nature of his theory, had not his sagacity been hoodwinked by the theory:— "These same half-bred birds afforded another and a very unlooked-for illustration of the position we have taken. They were all, when first hatched, like the Cochin cock, profusely feathered on the legs and feet—so much so, that they had to be marked to distinguish them from the pure-bred birds. We see here that, according to the law, the male parent implanted his characteristics; but what was curious, in a few weeks, in some of the half-breeds all, and in many most of the leg feathers were shed. Two out of some twenty birds only retained them in any very conspicuous degree. Now, why was this? The cock had implanted his external characteristics, the hen had given her vital organs. The feathers of the male were there; but the vital organs necessary to their growth were not there;

and consequently, after a time, for want of nutriment, these feathers were shed."

We will not here enter on the question of the growth of feathers (a very complex matter), but, accepting his own premises, ask him, if the external characteristics are thus dependent on the vital organs for their growth and development, and these vital organs are given by the female, how does the child ever exhibit the characteristics of the male, after infancy? Of what use is it for the male to implant his characteristics, when the female influence is thus certain to annihilate them?

Mr Orton further cites the practice of Bakewell with respect to his celebrated Dishley sheep. His rams might be bought or hired, for a good price; but his best ewes were sacred. These he would neither sell nor let. As a counter-statement, let it be noted that, according to Girou, the farmers are more particular about the bull than about the cow when they want a good milking cow, for it is observed that the property of abundant secretion of milk is more certain to be transmitted from a bull than from a cow. We question the fact of the bull having greater influence than the cow, believing that in each case the property is transmitted according to direct heritage; but that the bull should be known to have any importance in this respect, is an evidence that the "vital organs" are not solely given by the female.

The result of Mr Orton's researches proves that the male *does* transmit his qualities to his descendants; as a matter of fact this must be always distinctly remembered; but neither his researches nor those of his predecessors suffice to prove this transmission to be *absolute*, in the sense required by those who maintain that the male gives the *animal* and the female the *vegetative* organs; as well as by those who maintain that the male influence necessarily and invariably predominates in the animal, the female in the vegetative organs. Still it is important to know that by the pollen of flowers we can modify the tints, and produce any varieties of tulip, violet, or dahlia; important to know that we can also modify the plumage of birds and the colour of animals: it is important to know that the male qualities *are* transmissible. But for scientific rigour this is not enough. Before we can establish a law of this kind, we must be sure that the fact is constant and admits of no exceptions, or only of such apparent exceptions as may be classed under unexplained perturbations. Now daily observations, no less than recorded cases, assure us that the law is very far from being constant, that the female as unmistakably transmits her qualities as the male transmits his, and that any theorist who should reverse the current theory, and declare the mother bestowed the animal system, leaving the vegetative to the father, would be able to make a formidable array of facts. Let us glance a while at the evidence.

The male is said to give the colour; but the female does so likewise. A black cat and a white cat will have kittens which may be all black, all white, or black spotted with white, and white spotted with black. Every street will furnish examples. Isidore Geoffroy St Hilaire speaks of a case under his observation, of a black buck and a white doe: the first produce was a black and white fawn; the second a fawn entirely black, except a white spot above the hoof.* Burdach mentions the case of a raven and a grey crow, who had a brood of five—two black like the father, two grey like the mother, and one mixed. The ass has only five lumbar vertebræ, the horse six; but the mule has sometimes five like its mother, and six like its father; the hinny also shows the same inconstancy. The same result is observed with respect to all other qualities. But perhaps the most decisive example we could quote of the twofold influence of parents is in the singular instance recorded by Buffon. The Marquis Spontin-Beaufort had a she-wolf living in his stables with a setter dog, by whom she had two cubs, a male and a female. The male resembled externally his father the dog, except that his ears were pointed and his tail was like that of the wolf; the female, on the contrary, resembled her mother, the wolf, in all external characteristics *except* the tail, which was the same as her father's. Here, in one case, the father gave the external characteristics, in the other the mother; while the tail was in each case, as it were, transposed. But the marvel of this case does not stop here: the cubs manifested a striking difference in disposition, in each case *resembling in char-*

---

* *Dict. Classique d'Histoire Naturelle*, x. 121.

acter the parent it did *not resemble in appearance* and in sex: thus the male cub, which had all the appearance of a dog, was fierce and untamable as the wolf; the female cub, which had all the appearance of a wolf, was familiar, gentle, and caressing even to importunity. Lucas records an analogous case. These hybrids are very instructive, because the wide differences in the aspect and nature of the parents enable us to separate, as it were, the influence of each. The wolf and the dog often breed together; and the following observations, interesting in themselves, will suffice to show the reader how much caution is necessary before drawing absolute conclusions from single illustrations. Valmont Bomare observed in the various hybrids of wolf and dog which came under his notice at Chantilly, a striking preponderance of the wolf over the dog; Marsch, on the contrary, observed in his experience a preponderance of the dog over the wolf; Geoffroy St Hilaire and Pallas found the wolf to predominate; whereas, Marolle found the cubs remarkable for their gentleness and doglike instincts, only recalling the wolf in their voracity and fondness for flesh. Girou found the preponderance to vary; sometimes the father, sometimes the mother reappeared in the offspring. If there were no other evidence, this would suffice to disprove the hypothesis of either parent contributing one group of organs to the absolute exclusion of the other parent.

The same fact of twofold influence is shown in the transmission of deformities, such as extra toes, extra fingers, &c.: sometimes the male, and sometimes the female, is shown to preponderate, by the offspring inheriting the deformity of the male or the female. It is well said by Girou,* that "if the organisation of the male was the only one which passed to the child, the child would resemble the father, as the fruit of a graft resembles the tree from which the graft was taken, and not at all the tree on which it was grafted." And what is here said of the whole organisation, applies with equal force to any one system, such as the nervous or the nutritive.

Moreover, if the hypothesis we are combating be admitted—if the father bestows the nervous system—how are we to explain the notorious inferiority of the children of great men? There is considerable exaggeration afloat on this matter, and able men have been called nullities because they have not manifested the great talents of their fathers; but allowing for all over-statement, the palpable fact of the inferiority of some sons to their fathers is beyond dispute, and has helped to foster the idea of all great men owing their genius to their mothers: an idea which will not bear confrontation with the facts. Many men of genius have had remarkable mothers; and that one such instance could be cited is sufficient to prove the error both of the hypothesis which refers the nervous system to paternal influence, and of the hypothesis which only refers the *preponderance* to the paternal influence. If the male preponderates, how is it that Pericles, who "carried the weapons of Zeus upon his tongue," produced nothing better than a Paralus and a Xanthippus? How came the infamous Lysimachus from the austere Aristides? How was the weighty intellect of Thucydides left to be represented by an idiotic Milesias, and a stupid Stephanus? Where was the great soul of Oliver Cromwell in his son Richard? Who were the inheritors of Henry IV. and Peter the Great? What were Shakespeare's children, and Milton's daughters? What was Addison's only son? an idiot. Unless the mother preponderated in these and similar instances, we are without an explanation; for it being proved as a law of heritage, that the individual does transmit his qualities to his offspring, it is only on the supposition of *both* individuals transmitting their organisations, and the one modifying the other, that such anomalies are conceivable. When the paternal influence is not counteracted, we see it transmitted. Hence the common remark, "talent runs in families." The proverbial phrases, "l'esprit des Mortemarts," and the "wit of the Sheridans," imply this transmission from father to son. Bernardo Tasso was a considerable poet, and his son Torquato inherited his faculties heightened by the influence of the mother. The two Herschels, the two Colemans, the Kemble family, and the Coleridges, will at once occur to the reader; but the most striking example known to us is that of the family which boasted Jean Sebastian Bach as the culminating illustration of a musical genius, which, more or less, was distributed over three hundred Bachs, the children of very various mothers.

---

* *De la Génération*, p. 113.

Here a sceptical reader may be tempted to ask, how a man of genius is ever produced, if the child is always the repetition of his parents? How can two parents of ordinary capacity produce a child of extraordinary power? The answer must be postponed until we come to treat of secondary influences. For the present, we content ourselves with insisting on the conclusion to which the foregoing survey of facts has led, namely, that *both* parents are *always* represented in the offspring; and although the male influence is sometimes seen to preponderate in one direction, and the female influence in another, yet this direction is by no means constant, is often reversed, and admits of no absolute reduction to a known formula. We cannot say absolutely, "the male gives such organs;" we cannot even say, "the male always preponderates in such or such a direction." Both give all organs; sometimes one preponderates, sometimes the other. In one family we see children resembling the father, children resembling the mother, and children resembling both.

This is the conclusion inevitable on a wide survey of the facts. It is equally inevitable *a priori*, if we take our stand upon the evidence of embryology; and as some readers prefer logical deductions to any massive accumulation of facts, we will ask them to consider the question from this point of view. Reproduction, in the vegetable and animal kingdoms, is known to naturalists under three forms. In the first, a single cell spontaneously divides itself into two cells. Here it is quite clear that the child reproduces the totality of the parent. In the second form, the process called "budding" takes place: the child here grows out of the substance of the parent, until its development is completed, and then it separates itself from the parent to live a free life. Here also the parent is reproduced in its totality. In the third form, a higher complexity of organisation has led to a more complex and more special mode of reproduction: the parent gives off from its own substance, by what may be also considered a "budding process," a mass of cells, which as pollen and ovule, as sperm-cells and germ-cells, unite to develop into plants or animals. Here, again, there ought to be no doubt that the parents are reproduced; their offspring truly may be called "their own flesh and blood." Nor would the doubt have ever arisen, had not the great complexity of the organisms admitted the intervention of the Law of Variations, to which all dissemblances are due. But however such interventions may baffle our inquiries, the mind recognises at once the truth of the proposition that sperm-cell and germ-cell are as much to be regarded in the light of reproductions of the parents, as the cells produced by spontaneous division are to be regarded in the light of repetitions of the parent-cell.

It is now time that we should direct our attention to some of the perturbing causes which mask the laws of transmission from our perfect apprehension. While proclaiming as absolute the law of individual transmission, while proclaiming that the parents are always reproduced in the offspring, we are met by the obvious fact of the offspring often exhibiting so marked a departure from their parents, being so different in form and disposition, that the law seems at fault. We may point to the fact of a genius suddenly starting up in an ordinary family, or to a thousand illustrative examples in which the law of individual transmission seems at fault. To explain these would be to have mastered the whole mystery of heritage; all that we can do is to mention some of the known perturbing influences.

Sir Everard Home mentions a striking case, which has become celebrated, of a thorough-bred English mare, who, in the year 1816, had a mule by a quagga —the mule bearing the unmistakable quagga marks. In the years 1817, 1818, and 1823, this mare again foaled, and although she had not seen the quagga since 1816, her three foals were all marked with the curious quagga marks. Nor is this by any means an isolated case. Meckel observed similar results in the crossing of a wild boar with a domestic sow; in the first litter several had the brown bristles of the father; and in each of the sow's subsequent litters by domestic boars, some of the young ones were easily distinguished by their resemblance to the wild boar. Mr Orton verified this fact in the cases of dogs, pigs, and poultry.

Besides this very remarkable perturbing influence, we must also consider the phenomenon of *atavism*, or ancestral influence, in which the child manifests striking resemblance to the grandfather or grandmother, and not to the father or mother. The fact is familiar enough to dispense with our citing examples. How is it to be explained? It is to be explained on the supposition that the qualities were transmitted from the grandfather to the father, in whom they were *masked* by the presence of some antagonistic or controlling influence, and thence transmitted to the son, in whom, the antagonistic influence being withdrawn, they manifested themselves. A man has a remarkable aptitude for music; but the influence of his wife is such that their children, inheriting her imperfect ear, manifest no musical talent whatever. These children, however, have inherited the disposition of their father, in spite of its non-manifestation; and if, when they transmit what in them is latent, the influence of their wives is favourable, the grandchildren may turn out to be musically-gifted. In the same way Consumption or Insanity seems to lie dormant for a generation, and in the next flashes out with the same fury as of old. Atavism is thus a phenomenon always to be borne in mind as one of the many complications of the complex problem.

A third cause of complication is one which we propose to call "the potency of the individual." Both father and mother transmit their organisations, but they do so in unequal degrees: the more potent predominates; just as if you mix brandy with equal amounts of water, soda-water, and ginger-beer, the taste of the brandy will predominate more in the water than in the soda-water, more in the soda-water than in the ginger-beer.

According to Rush (quoted by Lucas), the Danes, intermarrying with women of the East, always produce children resembling the European type; but the converse does not hold good when Danish women intermarry with the men of the East. Klaproth observes the same in the mingling of the Caucasian and Mongolian races. Girou, after five-and-twenty years' experience in the breeding of sheep, found this "potency" destroy his calculations. He fancied that, by means of his Roussillon sheep and the Merino rams, he could sooner arrive at the fineness of wool which distinguishes the Merino, than if he coupled the Aveyron sheep with the Merino rams; but he found that the Roussillon type resisted the Merino so energetically that, after a quarter of a century of successive crossings, it still reappeared, whereas the Aveyron sheep had long ceased to be distinguishable from the Merinos. The same potency of particular species is noticeable in plants. Koelreuter is quoted by Burdach as having fecundated the *Nicotiana paniculata* with the pollen of *N. rustica;* and the hybrids thus produced were fecundated with the pollen of *N. paniculata,* but the plants resembled the *N. rustica.* On reversing this experiment, he still found the female *N. rustica* to have the preponderance; so that, cross the species how he would, the *N. rustica* showed most potency.

But although we thus see that Race has a marked preponderance, we must also remember that it is subject to the individual variations of vigour, health, age, &c. Girou sums up his observations with this general remark: the offspring of an old male and a young female resembles the father less than the mother in proportion as the mother is more vigorous and the father more decrepit; the reverse is true of the offspring of an old female and a young male. In fact, if we consider that the offspring reproduces the organisation of its parents, and, consequently, the organisation of *that particular period*, we see at once that age, health, and general potency of organisation, must be taken into the account of complicating causes. This also will help to explain—but not wholly explain—the great differences observable in the same family: differences of sex, of strength, and appearance. At present, however, science can only take note of it as a "perturbing influence."

Reviewing the evidence which has been adduced, we find it establishing the following important positions. We inherit from our parents the general form and features, the bony, muscular, nervous, and glandular structures: there is no absolute constancy in the transmission of any one part of the organisation; but just as the features may be curiously compounded of the features of both parents, or the eyes and mouth may resemble those of the mother, while the chin, nose, and brow resemble those of the father; so may the various parts of the body be a *compound* of the organs of both parents, or a *distribution* of the organs of both parents. With this inheritance of the general organisation, we necessarily inherit its tendencies. We inherit the temperament, the longevity, the strength, the susceptibility of one or both parents. We inherit the tendency to scrofula, epilepsy, or mania. We inherit the nervous system, no less than the muscular and bony; and with the nervous system, we inherit its general and particular characters—that is to say, the general sensibility of the system, and the conformation of the brain and sensory ganglia, are as much subject to the law of transmission as the size and conformation of the bony and muscular structures are. This being so, it is evident that all those tendencies which depend on the nervous system will likewise be inherited; and even special aptitudes, such as those for music, mathematics, wit, and so on, will be inherited; nay, even acquired tendencies, and tricks of gesture, will be inherited. But this inheritance is in each case subject to the influence exercised by the other parent; and very often this influence is such as to modify, to mask, or even to entirely suppress the manifestation. A man of highly susceptible nervous organisation, a man of genius, marries a woman of powerful organisation, but of rather inferior brain: the influence of the mother is such, that the child turns out perhaps irritable, nervous, but intellectually feeble; or healthy, vigorous, and commonplace; or even stupid, and, it may be, idiotic. Or both parents may be remarkable for intellect, yet because their nervous systems have been developed at the expense of their nutritive systems, their child may be susceptible, but puny and feeble.

Without presuming to fix the limits of inheritance, I must confess that I cannot extend them so far as some writers seem disposed to extend them. F. Cuvier, for instance, says that young foxes, in countries where traps are laid, are less easily caught than young foxes elsewhere: this may be strictly true, but I cannot regard this as evidence that the foxes have inherited greater wariness in regard to traps. It is possible that they may inherit a greater susceptibility to fear of *all* objects, if fear has been more active in their ancestors; because this will depend upon the general susceptibility of the nervous system having been heightened; but to suppose that young foxes inherit any particular fear of traps, is to believe in the existence of innate ideas. It is conceivable that a devout tendency may be inherited from parents in whom devotion was a very eminent quality; because it is conceivable that a peculiarity of organisation may have been inherited, which would impress this *tendency* on the mind of the child; but we cannot conceive anything so specific as the worship of the Virgin to be inherited, even after centuries of devotion.

There are many other topics connected with this subject of inheritance,* but our space will only admit of one more being touched on, and that is the question of whether it is morally justifiable to consent to a marriage when one of the lovers has the "taint" of insanity or consumption,—either directly manifested, or only suspected to exist because it has been manifested in the family. I believe it to be as certain that the tendency to insanity, scrofula, or consumption is transmitted from parent to child as that any other peculiarity is transmitted: if we expect a Roman-nosed father to transmit that form of feature to his son, we must also expect him to transmit the "taint" of insanity, or scrofula. If this were the whole case, the moral rule would be clear, and marriage would be imperiously forbidden. But certain as the law is, it is not single. There is another law equally valid. The child of the Roman-nosed father is just as likely to be born with a nose not at all or very slightly aquiline, inheriting from his mother *as well as* from his father; the child of the tainted father is just as likely to be born with a very slight taint, or none that is appreciable. We see this constantly in families: one parent is carried off by consumption; two or three of the children likewise perish; one or two are merely delicate; the rest seem as vigorous as other people. If this is true in cases where one parent is manifestly and fatally affected, how much more must it be so in cases where the parent is only liable to the *possibility* of being affected, because the disease is in the family? All that we can say is that there is a great risk. Much will depend on the constitution of the untainted parent. A man with tubercles already formed may marry a woman who shall bear him numerous children, all perfectly healthy; whereas the same man may marry another woman whose children would all be doomed. Each individual case would require a deliberate survey of all the conditions before a reasonable judgment could be offered; no absolute rule can be laid down, because none will fit the complexity of the cases.

---

* In HERBERT SPENCER'S *Principles of Psychology* will be found numerous applications of the law of inheritance, especially as regards the Moral Sense and intellectual progress.

CHAPTER XIII.

LIFE AND DEATH.

The mystery of Life—Suspended animation—Is there a Vital Principle?—Examination of the arguments in favour of it—Does Life control chemical affinities?—Does Life precede Organisation?—Life an aggregate unity—What is Life?—Definitions which have been attempted—Cell-life—Life of the parts and life of the whole—The three laws of Life—Vegetal and Animal Life—Criticism of Bichât: Vegetal organs, double and symmetrical; the law of intermittence not confined to animal functions—Life in relation to the external medium—Duration of life—The four Ages—Longevity—Old Age—Death the inevitable residue of Life—Death of the parts—Death of the organism—Apparent Death—Signs of Death.

LIFE and Death are familiar mysteries. Nothing can be more familiar to our minds; of nothing do we know less. The man whom we loved and honoured, who an hour ago was full of energy, affection, hope, and endeavour, is now lying an inert mass before us, insensible, inanimate. What has made this difference? The form remains, and, but for the paleness of the face, we might suppose him sleeping placidly. Yet Life has vanished, and

"All that mighty heart is lying still."

What is it that has vanished? Whence comes this terrible stillness?

The grains of wheat found in Egyptian tombs, on being planted in the soil of France, grew into waving corn. For three thousand years these grains had been without Life, yet we cannot say they were *dead;* they were organic seeds, products of Life, and had themselves once *lived:* they were developed from cells into seeds, and at this point in their development their vital activity was arrested, because the necessary heat, light, and soil were wanting; but no sooner were these supplied, than the arrested activity was once more set free—the seed became a plant.

The microscopic animals known as Rotifers become, to all appearance, dead, when the water of the moss in which they live is evaporated; and in this state of suspended animation they may remain for years, recovering their energy on the addition of a little water; they may be dried and revived fifteen times in succession; but they are not dead in these cases, and they do not decompose. Frogs and toads, especially the latter, when subjected to the action of intense cold, may be perfectly *frozen,* all their fluids becoming congealed, all their tissues rigid. Yet these animals are not dead. Let heat be cautiously applied, and the congelation ceases: in eight minutes, life is once more in full activity.*

---

* The authorities for these statements are GEOFFROY ST HILAIRE, LEEUWENHOEK, SPALLANZANI, ISIDORE ST HILAIRE, and DUMÉRIL. For that relating to the toads, see *Comptes Rendus,* vi. 538, and *Annales des Sciences,* 1852, xvii. 10. Compare also the same work, 1858, x. 335.

In all these cases it is clear that the various phenomena grouped under the term Life are entirely absent; they would *never* manifest themselves, unless the peculiar *conditions* necessary for their manifestation were supplied. The three thousand years would, for the Egyptian grain, stretch to an eternity. The organism in this state of suspended animation is not destroyed; it is like a watch that has run down: until it be again wound up, its delicate mechanism will remain silent and inactive. But once set one part of this mechanism going, and all are set going. Death is not the unwound watch, but the watch with its mainspring broken.

These analogies naturally raise the question, Is there in organic beings anything strictly analogous to the mainspring of a watch? Can we suppose the existence of some vital centre from which all vital actions issue?

Nothing was easier for the early physiologists than to answer this important question. They assumed the existence of two distinct things—dead matter, and a living entity. Life, they said, was an entity, a principle essentially distinct from matter, inhabiting the body, which was composed of dead matter, as living men inhabit dead houses. If this principle quitted the body, the result was Death. If it remained in the body, but wholly inactive, as in swooning, it was said to be "suspended." The Vital Principle animated, directed, formed the body, preserved it from the effects of chemical action, and on quitting it, left it a prey to this destructive action, which was manifested by dissolution.

This is, in brief, the hypothesis of a Vital Principle, which was dominant for many centuries, and is now rejected by all, except a few metaphysicians and meta-physiologists. It has fallen into complete discredit, since men have learned that, besides being in opposition with all the teachings of Science, it is really nothing more than a substitution of words for ideas, and while seeming to explain the phenomena, it only gives a verbal explanation which leaves the problem unsolved. The same remark is true, although to a less extent, of the modern doctrine of a Vital Force, or Vital Forces. So long as vital force merely indicates the dynamical condition of the organism, the phrase is unobjectionable; and if the vital forces are taken as the abstract, of which the vital phenomena are the concrete, we may use the term as freely as we use the terms "mechanical force," "locomotive power," and the like. But when the abstract term misleads us into a belief in there being a concrete existence corresponding with the abstraction—when we suppose that there are vital *forces* independent of, and controlling, the sum of material conditions present—it is as if we imagined a watch-principle turning the hands on the dial-plate, or a moving-principle driving the engine, irrespective of coal burned and water expanded. To conceive the organism in action, and to call its activities by the name of vital forces, is one thing; to conceive that these activities are anything more than dynamic states of the organism is another. The one conception is physiological, the other is *meta*physiological.

Chemists have analysed organic substances, taken them to pieces, and in some cases have been able to *make* organic substances—putting together the sort of pieces they have found in such substances. They have not, indeed, been able to make any of the higher organic substances, such as albumen; but they can very accurately unmake them. In all their researches no one has ever discovered anything which could be called specially organic—that is to say, formed of elements not found in inorganic bodies. No one has ever found that the elements of organic bodies differed, when separated, from those same elements in inorganic bodies; and the inevitable conclusion has been, that since organic bodies do not differ from the inorganic in their *elements*, but only in the *arrangement and combination* of these elements, the phenomena specially manifested by organic bodies must be due to this speciality of arrangement. There can be no such thing as matter essentially dead; there can be no such thing as matter essentially living. That which to-day we class as dead, will to-morrow be classed as living, and that which is living to-day will be dead to-morrow. Living and dead are terms which indicate certain groups of *phenomena;* and these phenomena are dependent on certain groups of *conditions.* The phenomena manifested by sulphuric acid are exceedingly unlike those manifested by the sulphur and the oxygen of which it is composed; in like manner, the phenomena manifested by the excessively complex structures, named organic, are exceedingly unlike the phenomena manifested by the separate substances composing them.

It is true that if there *were* a Vital Principle, or an independent Vital Force, its presence might be the cause of this very difference in the conditions which, we have said, determines the peculiarity of vital phenomena. But we must never gratuitously multiply existences, we must never assume that which is incapable of proof. A Vital Principle is incapable of proof; if it exist, we cannot know it; and unless its existence can be proved, it is to us a mere phrase concealing our ignorance. Phrases serve to build systems, but they hamper the progress of knowledge.

There are only three arguments in favour of a Vital Principle which we need consider here: these are, first, that Life controls chemical affinities; second, that Life *precedes* Organisation, and cannot therefore be a *result* of Organisation; third, that Life is a presiding unity.

Does Life control Chemical Affinities?—The obvious fact, that no sooner is life extinct than the body passes into corruption, its particles yielding to the action of chemical affinities, whereas, as long as vitality remains, the body retains its form, and does not seem to yield to the destructive action of chemical agencies, early led men to conceive that there was some mysterious principle *controlling* chemical action in the vital organism. And a striking confirmation of this idea seemed furnished by the fact that the gastric juice eats away the coats of the dead stomach, but leaves the living stomach unaffected. When, however, we examine closely these two points, we find that an entire misconception lies at the bottom of the hypothesis. Instead of chemical affinities being controlled by vitality, there is no vitalaction possible *without* the incessant and complicated action of chemical affinities—all the molecular changes of composition and decomposition occurring in the living body—all nutrition, secretion, and motion, depend on chemical actions. If the oxygen of the atmosphere destroys the dead tissues, not less energetically does it destroy the living tissues; but the destruction of the dead tissue is not *replaced* by the reproduction of fresh tissue, as in the living organism. I have already had occasion to explain why the living stomach is not destroyed by the gastric juice.* No sooner do we admit that *all* chemical affinities are not controlled by vital laws, no sooner are we aware that chemical actions incessantly take place in the living organism, and these actions are dependent on precisely the *same* affinities as those exhibited out of the organism, than it becomes clear that if *any* chemical actions are prevented from taking place in the organism, it is because the *proper conditions* for such actions are not present. No phenomenon, chemical or vital, can take place unless it be under the condi-

---

*See the chapter on DIGESTION and INDIGESTION, vol. i. p. 216 *et seq.* Doubts have been thrown on the explanation there given. It is urged that the presence of mucus is the cause of the gastric juice being inoperative. If this be so, my argument remains just as strong—since the mucus, and not a vital principle, preserves the stomach.

tions which permit its manifestation. Wet gunpowder will not explode. Water will not solidify above a temperature of 32°. Chlorine will not unite with Hydrogen in the dark; yet in the sunlight their affinity is so strong that they unite with an explosion. A certain range of temperature is necessary for all chemical actions; and those combinations which take place at the ordinary temperatures of the air may be entirely prevented by cold. If, therefore, many chemical phenomena take place in the living organism which are not observable elsewhere, it is because the *conditions* are different. And as for the oft-cited *resistance* to decomposition which the Vital Principle is said to effect, not only is the fact quite otherwise, not only is decomposition incessantly going on in the living organism; but if the fact were as stated, it could be met by the fact that an infinitely better preservative against decomposition is found in Alcohol, Glycerine, or Chromic acid.

Does Life precede Organisation?—There is an unfortunate ambiguity in the word Organisation, which renders it very plausible to say that Organisation is produced by Life, and does not produce it. Men point to the fact of the living germ, which is only a cell, or a mass of cells, and they say, Here is Life, but the organs are not yet formed. They point also to a dead body, and say, Here is Organisation; no lesion has taken place; every organ, every tissue remains intact, yet Life is gone. There was a time when this reasoning seemed to me conclusive; and so long as we understand by an organism a collection of organs, the knowledge of the existence of many plants and animals, and of all embryos in their earliest state, which have no "organs" at all, makes it impossible to reject the conclusion; equally impossible is it, when we conceive the organism as meaning solely the solid fabric, and not also the *whole sum of conditions necessary for its activity.* If I tie an artery, I do not destroy the structure of the limb to which it is distributed, but I destroy the organic relations of the several elements—I destroy the *conditions* necessary to the vitality of that limb. If I prevent the access of oxygen to the blood, I do not destroy the fabric of the body, but I disturb the conditions necessary to the vital activity of the body, as completely as I destroy the conditions necessary to an explosion when I throw water on gunpowder. Recurring to the old analogy of a watch, when the chain has run down, the mechanism is as perfect as ever, but the mechanical relations of the parts are disturbed, and the watch is still: to set it in action once more, we have only to wind it up; that is to say, we have to restore the necessary conditions of mechanical relation.

With regard to Life not resulting from Organisation, this position rests on vague conceptions of both terms. Let us remember that Organisation means the *whole sum of necessary conditions*, no less than the organic fabric, and it will not be difficult to understand that *Life is proportional to Organisation.* The life of a single cell is the *sum-total of the activities of that cell*, and is simple in proportion as the Organisation is simple. The Life of a highly-organised animal is the *sum-total of the activities of all the forces at work,* and is complex in proportion to the complexity of the organism. Take a single cell which is manifesting all its vital activity, spread it out into a layer, and although all the material of which it was composed remains as before, the properties which belonged to it *as a cell*—its Life—can no more be manifested.

And this leads us to the third argument in favour of a Vital Principle, namely, that Life is a presiding unity. "The body is one; all its parts are subordinate; all are bound together in a higher unity. Our consciousness assures us that our Life is a unity." There is an important fact indicated in this argument, but it is misinterpreted. There *is* unity—there is a consensus of the whole organism. But this is not, I conceive, attributable to a Vital Principle, existing independently of the organism; it is due to organic subordination: all the parts are related; all act together by means of the nervous system, as all the parts of an army act together by means of officers and discipline. Where there is no such connection of parts, there can be no such subordination of organs. We may divide a Polype or a worm into several pieces, each piece continuing to live and grow; but we cannot suppose that in such cases we have cut the Vital Principle into several Principles. There is, as we shall see presently, a life of the parts, and a life of the whole organism; each microscopic cell has its independent existence, runs its own career from birth to death; and the *sum-total of such* *lives* forms what we call the Life of the animal: the unity is an aggregate of forces, not one presiding force.

I have dwelt the longer on this topic, because, although the hypothesis of a Vital Principle has fallen into general discredit, the metaphysical attitude of mind which is implied in the hypothesis, is still unhappily too common among physiologists; and as it is the purpose of this work to consider Physiology even more with reference to general culture than as a branch of special science, the discussion became necessary. Having lingered thus to refute the hypothesis of a Vital Principle, I may be asked what is the hypothesis to replace it? Instead of an hypothesis, I shall propose a description of vital phenomena.

It is surely more philosophical to consider Life as an ultimate fact; one of the great revelations of the Unknowable; one of the many mysteries surrounding us. Having made up our minds on this point, having resigned ourselves to the complete relinquishment of all hypothesis, of all endeavours to penetrate into the inscrutable, we cease vexing ourselves with the arcana of Nature, and try to ascertain the *order* of Nature. We no longer set up fictions of our imagination in the place of a reverent observation. There are minds, indeed, which feel distrust at such resignation. They seem to dread lest Life should be robbed of its solemn significance, in the attempt to associate it, even remotely, with inorganic phenomena. But this fear arises from narrow views of Nature. It is because reverence for Nature has not been duly cultivated; because familiarity with inorganic phenomena has blunted our sense of their unspeakable mystery. Men who are thrilled at the tokens of the past life of man, when they see, or read of, buried cities, Palmyra, Nineveh, or Yucatan, tremble with no delicious awe at the tokens of the past life of this earth, when they stand in a quarry, or ramble through a geological museum. Yet surely the crystal is not less mysterious than the plant; the ebb and flow of the tides not less solemn than the beating of the human heart? And if patient observation and induction have enabled us to trace something of the order of Nature in crystallisation and the tides, without aid from the metaphysicians, they may also enable us to understand something of the laws of Life.

The general consent of mankind has consecrated the identity of the words Life and Existence. By the life of an animal is meant the existence of the animal. The particles of which the animal is composed continue to exist after its death—that is, after its existence *as an animal* has ceased. If, then, we accept this general meaning, and say that Life, in its abstract sense, is Existence, we shall have to consider all the various *forms of life* presented to our contemplation, as so many *modes of existence.* These forms often closely resemble each other; they often profoundly differ. According to these resemblances and differences, we group them under two general heads: Organic Existence, and Inorganic Existence. The mystery which underlies all Existence cannot be unveiled by us; but it is not less in the one case than in another; and although custom has affixed the term "living" to one great group of existences, this new term does not introduce a new mystery, nor warrant a new attitude of mind in studying the group. All we are entitled to say is this—there is a *speciality* about vital phenomena, arising from the peculiarity and complexity of the conditions which determine them; and this speciality must warn us against reasoning about them as if they were *not* special, but were in all respects like inorganic phenomena; this speciality, in short, suggests the necessity of studying them in themselves, and not as if they belonged to the general phenomena of physics and chemistry; invaluable as the knowledge of these latter must always be as a means of exploration.

II. DEFINITION OF LIFE.—It would certainly be a great assistance in our efforts to describe vital phenomena, if we could fix clearly in our minds the chief characteristics which distinguish living from non-living bodies; and a definition of Life would be peculiarly acceptable. The attempt has been repeatedly made, but never with entire success.

Aristotle supposed that vital actions were under the dominion of several Vital Principles (*souls, ψυχαι*, he calls them), each distinct, but all subordinate to one supreme Principle. This idea reigned many centuries, and was revived a few years ago by Prout. Kant defines Life "an internal principle of action;" an organism is "that in which every part is at once means and end." This may be said with equal truth of fermentation. Treviranus calls life "the constant uniformity of phenomena under diversity of external influences," which may be equally said of a watch, since if the external influences in any way disturb the organism or the mechanism, the phenomena cease to be uniform. Bichât's celebrated definition, "Life is the sum of the functions by which death is resisted," is only another form of the definition proposed by Stahl, and is every way objectionable; for, on the one hand, it is a paraphrase of the truism that life is the means by which we live; and, on the other hand, it declares that there is a fatal antagonism in external agencies; whereas nothing is more certain than that external agencies are *essential* to vital phenomena: as we saw in the opening paragraphs of this chapter. Dugés calls Life "the special activity of organised beings;" and Béclard says, "Life is the sum of the phenomena proper to organised beings. It consists essentially in this, that organised beings are all during a certain time the centres to which foreign substances penetrate and are appropriated, and from which others issue." De Blainville's definition, adopted by Auguste Comte and Charles Robin, runs thus: "Life is the twofold internal movement of composition and decomposition at once general and continuous;" but this only applies to Nutrition, and does not embrace all the cardinal phenomena: while the definition proposed by Herbert Spencer, namely, "Life is the definite combination of heterogeneous changes, both simultaneous and successive in correspondence with external coexistences and sequences," has the disadvantage of not being perfectly intelligible apart from his explanation of it in detail.[*]

Perhaps the most intelligible and easily remembered definition would be this: "*Life is the dynamical condition of the Organism.*" This embraces every form of life, from that of the simple cell to that of the complex mammal. It expresses every variation in the intensity or complexity of the phenomena, according to the activity or complexity of the organism; and their dependence on all agencies, external and internal. And when we add the three cardinal characteristics which the organism everywhere manifests, Growth, Reproduction, and Decay, we have briefly indicated all that specially distinguishes living from non-living things.

III. CELL-LIFE.—Since the researches of Schwann in 1839, who applied to animals the doctrine of Schleiden with respect to plants, Biology has undergone a renovation almost as great as that which Chemistry underwent after the labours of Lavoisier. The last twenty years, indeed, will be known in history as the cell-epoch, so manifold have been the researches, and so striking the results, with respect to the part played by cells. That in these twenty years the original positions have been much modified will surprise no one; nor have we any security that greater changes are not still awaiting the investigator.

---

[*] ARISTOTLE: *De Animâ*, i. 2; KANT: *Kritik der Urtheilskraft*, Werke, iv. 260; TREVIRANUS: *Biologie;* BICHÂT: *Recherches sur la Vie et la Mort;* STAHL: *Theoria Medica vera*, i. 228; DUGÉS: *Physiol. Comparée*, i. 3; BÉCLARD: *Anat. Générale*, p. 4; AUGUSTE COMTE: *Cours de Philos. Positive*, iii. 295; SPENCER: *Principles of Psychology*, p. 354.

A cell is regarded as the true biological atom. Nothing is living but cells, or what can be directly traced back to cells. However great a departure from the cell-form may be disclosed in an anatomical investigation—as in fibres, vessels, bones, membranes—a *morphological* investigation detects that all these were cells in their origin. As a cell the organism commences; all through its career, a large part of the organism is made of cells, and the rest is of transformed cells, or cell-products.

What is a cell? It is usually a microscopic sac, or vesicle, containing a smaller vesicle, called the *nucleus.* In many mature cells this nucleus contains a minute round body, probably solid, called the *nucleolus;* this is, I believe, never found in young cells. Even the nucleus is not always present: it is absent in cells about to perish,—such as the cells of the outermost layer of the skin; or in cells which have only a transitory existence—such as the blood-cells of mammals. Besides the nucleus we must take into account the *cell-contents.* The delicate enveloping membrane, or cell-wall, which forms the sac, contains a liquid and granules, the special nature of which determines the special properties of the cell. Thus the muscle-cell contains a *contractile* substance; a pigment-cell contains colouring matter; a mucus-cell contains mucus, and so on.

Every cell in the organism is independent: it is born, it grows, it reproduces, and it dies, as if it were a single-celled plant, or animal. The growth and decay of an organ, is like the growth and decay of a nation, or of a tree: the individual cells composing the organ grow and perish, as the individual men, or the individual leaves, grow and perish. There is a certain aggregate unity, but it is made up of distinct units. Just as the life of a nation, or a tree, is the sum total of the lives of all its individual parts, so is the life of an organism the sum total of the lives of its individual cells.

This is one of the great revelations of modern science. We no longer speak of a centre of vital activity, for we know that there are myriads of centres. It is therefore no longer surprising to us to know that a part of an organ may be destroyed without the function of that organ disappearing; for this is no more than a regiment's continuing the battle when its ranks are thinned. If the officers are shot down, the regiment will be dispersed, and each soldier will then have to shift for himself; in like manner, if certain regulating parts in the mechanism of the organ be destroyed, the function of that mechanism will disappear, and the individual cells be left to themselves. Thus a man may have an abscess in his liver, which will not prevent the cells of the other unaffected parts of the liver from secreting bile, and pouring it through the gall-duct; but an abscess in the gall-duct would prevent the *function* of the liver, though it would not interfere with the *property* of the liver-cells to secrete bile. A man with half a lung will continue to breathe by means of the half which is unaffected: he will not breathe so vigorously as he did when his lungs were sound; the amount of oxygen he can respire will be of course diminished in proportion to the diminution of the air-cells; but so long as the air-cells remain, and the respiratory mechanism is in action, the breathing function will continue. The student will do well to familiarise his mind with the distinction between the life of the parts and the life of the whole; and to discriminate between those phenomena which depend upon the individual parts, and those which depend upon the mutual *relations* of the parts—in other words, upon the vital structures, and their mechanism.

We have previously stated that there are three cardinal characteristics distinguishing organic from inorganic beings. 1°, *The Law of Nutrition*, the most fundamental of all vital laws, since in virtue of it the animal continues to exist as an *active* organism, and increases from infancy to maturity. 2°, *The Law of Development, or Differentiation*, which causes the organism to pass through the *definite cycles of change*, constituting what we call Ages, and leading inevitably to the final change, which we call Death. 3°, *The Law of Reproduction*, another aspect of the first law, in virtue of which the organism gives birth to similar organisms, and thus the lamp of life is handed on from generation to generation.

To no inorganic existences can these three cardinal characteristics be assigned. When inorganic bodies are said to grow ("*mineralia crescunt*," Linnæus), their growth is that of mere aggregation, one part adhering to another similar part. The growth arises from no internal necessity, as in organic bodies. The bulk is not increased by a process of assimilation, which converts the unlike into the like, which makes foreign substances homogeneous — minerals do not *feed*, they *cohere*. Nor have they any Development: they pass through no definite cycles of change: they have no stages of growth, no Ages. Reproduction is equally impossible to them.

Thus, by exclusion, we arrive at a description of the three great *special* characters of living beings: Growth, Development, and Reproduction. It is obvious that the phenomena of beings so markedly distinct from all other beings, must have a corresponding speciality, and must be studied in themselves. Without calling in the delusive aid of a Vital Principle, we may insist on the fact that vital phenomena are peculiar, special, unlike the phenomena of inorganic beings, and must be studied with constant recollection of this peculiarity.

IV. VEGETAL AND ANIMAL LIFE.—Since the time of Bichât, physiologists have universally accepted the classification proposed by that brilliant thinker, ranging all vital phenomena under two heads: 1°, Organic, or vegetative life; *2°, Animal, or relative life. The first is common to Plants and Animals; the second is peculiar to Animals. The vegetal phenomena of

---

[*] To avoid the equivocalness and awkwardness of vegetable and vegetative, Mr SPENCER has suggested to me the term *vegetal* as being at once distinctive, and correlative with *animal*: we may thus say vegetal, and vegetality, as well as animal, and animality. I am the

growth, development, and decay, comprise the whole vitality of the Plant; but over and above these the Animal manifests the phenomena of *relative* life, so called because the animal is brought into active relation with the external world. "Cast your eyes," says Bichât, "on individuals of the two kingdoms, vegetable and animal; you will see that the one has only an internal life, its relations with the exterior being only those of nutrition—it is born, grows, and perishes, fixed to the spot where the seed was deposited; the other unites, to this internal life, an external life which establishes manifold relations with surrounding objects—the animal is thus led to approach or avoid objects according to its desires or its fears."

Although open to criticism, this classification is so convenient that it has been adopted; and we may therefore speak of vegetal functions, and animal functions, of vegetal and animal life, without fear of equivoque. But some of Bichât's other systematic views are less acceptable. For example, I think he was singularly unfortunate in his anatomical generalisation, that all the vegetal organs are single and unsymmetrical, while all the animal organs are double and symmetrical: a very slight examination of this point will disclose its inaccuracy. The parotid and sublingual glands, the lungs and breasts, the kidneys and testes, are all vegetal, and all double and symmetrical

---

more disposed to adopt this suggestion because vegetal is a true English word, not unfrequently used by our old writers, especially the Elizabethan dramatists.

—and if the heart and the uterus seem to be single, every anatomist knows that they are as truly double as the cerebrum, or the spinal chord; and although there are certain irregularities, which Bichât has pointed out—in the disposition of the bronchi, for example—there are irregularities as great in the disposition of nerves; and even the brain itself, so confidently relied on by Bichât, is far from perfect in its symmetry—nay, Bichât's own brain was remarkable for the difference in its two halves.

Not less unfortunate is the physiological generalisation Bichât propounded in the celebrated *Law of intermittence,* characterising animal life, and the *Law of continuity,* characterising vegetal life. This generalisation has been accepted as indisputable; let us subject it to a brief scrutiny. All the relative, external functions are intermittent—all the vegetal, internal functions are continuous: that is Bichât's position. In contradiction to this, I affirm that the vegetal functions are intermittent, as well as the animal functions; and that the only processes which can be called strictly continuous are those processes of molecular change—of composition and decomposition—on which existence depends. Let us see which position best covers the facts. It is true that animals require rest after exertion, and sleep, after some hours' waking; this intermittence is caused by an exhaustion of the bodily energy, as we have already endeavoured to indicate.* But that this intermittence is not exclusively animal, may be seen in the equally familiar facts that the vegetal function of digestion is not less periodical; that the vegetal functions of generation and lactation are equally intermittent. A certain time is required to form the gastric juice, and during this interval the function of digestion is impossible. It is the same with all other functions. Nay, even secretion itself is not wholly continuous: the glands are not constantly in action; as we now know from Bernard's discovery respecting the colour of the venous blood flowing from them.* The function of respiration may perhaps be said to be continuous, because its intermittence is so brief and regular; the same also may be said of the circulation; the rapidly intermittent propulsions of the heart, and the continuous processes of molecular change—in the capillaries—on which circulation depends, give it a certain continuity. But if we except these, all the vegetal functions are obviously intermittent: on the other hand, the animal functions have scarcely more claim to intermittence; since even if we suppose that the brain is ever wholly at rest, in sleep—of which there is neither evidence nor probability (see p. 373)—absolute repose for the nervo-muscular apparatus is impossible, so many of the vital functions being dependent on this apparatus. The only approach to a generalisation which seems to me warranted is this: that the vegetal functions are more uniform and rhythmic in their activity, because the conditions and stimuli are more uniform than is the case with the animal functions. The conditions of secretion, and the stimuli to the functions of breathing, &c., are more uniformly present than the conditions of thinking or moving; and the stimulus is less exhausting, therefore less time is required for the reintegration of the molecular condition necessary to activity.

---

* See p. 362.

---

* See vol. i. p. 269.

V. Life in relation to the External Medium.—The error, long dominant, and not even yet entirely effaced, which made men conceive Life in itself as something unrelated to, and wholly independent of, the external medium, was consecrated by the doctrine of a Vital Principle; and yet the facts which plainly spoke against such a conception were familiar facts, although their bearing was misunderstood. Every hour men must have been aware that very slight changes in the external conditions rendered the manifestation of life impossible; a little more carbonic acid, or a little less oxygen, a little more heat or cold, a little less pressure of the atmosphere, a deficiency of water or of food, sufficed to extinguish life, as a lamp is extinguished when the oil disappears. Every one knew that, ascending a mountain, certain classes of animals and plants gradually disappear, a new fauna and flora occupying the higher districts; so that, all the world over, the same species of plant will be found at the same heights above the sea-level, and only these. As we walk down upon the shore at low tide, we find different species inhabiting distinct zones: those which live in deep water being unable to bear the temporary exposure which is daily borne by those which live nearer the tide-mark.

And yet in spite of this knowledge of the constant influence of surrounding conditions on the vital manifestations—in spite of the knowledge that vital phenomena are exalted or depressed, called into existence or entirely destroyed by the influences of light, heat, pressure, moisture, gases, &c., men have gone on asserting that Life is independent of external influences, and that it has the power of controlling physical and chemical laws. In the present state of science such a conception is inadmissible; yet in a widely circulated work we read that "Life resists the effect of mechanical friction," and we find this amazing proposition resting on no firmer basis than an example which will be read with surprise. "The friction," it is said, "which will thin and wear away a dead body, is actually the cause of thickening a living. The skin on the labourer's hand is thickened, to save it from contact with hard substances." Now the fact is, that the friction which wears away a dead body has precisely the same effect on the labourer's hand: so far is Life from resisting this mechanical friction, that in a much shorter time than would suffice to wear away a layer of wood, the living skin would be rubbed bare. When with unaccustomed hands we ply an oar, which is the first to show the effects of friction—the oar or the hand? The peculiarity in the case of a living body is this, that whereas the dead body, when its particles are rubbed off, has no power of *replacing* them, the living body *has* this power, and by its means not only is the loss repaired, but there may be even an increase of the original size. If a labourer's skin is thickened, it is because the growth of its cells has been stimulated, and more are reproduced than are wasted.

We must constantly bear in mind that Life is possible only under the necessary conditions of an *organism*, on the one hand, and an external *medium* on the other. It is the mutual relations of organism and medium which determine the manifestations we name Life; and the error is as great when men attempt to solve vital problems without duly taking all external conditions into account, as when they attempt to solve them without taking the organic conditions into account—that is, when they attempt to explain vital phenomena as if they were simply physical and chemical. The metaphysician, with his "vital forces," is as one-sided as the chemist with his "affinities."

VI. DURATION OF LIFE.—Every organism has its limits of duration which are definite and inevitable; we cannot assign these limits with precision, any more than we can assign the precise stature which an animal will attain; we can only fix boundary-marks within which the duration is possible; for the rest, "the great areas of life cannot be expressed in terms of strict measurement."[*] These areas mark the accomplishment of a series of organic changes; and such changes, although definite in their order of succession, are so dependent on a variety of influences, "that they do not admit of being weighed in the balance, nor can the vital maturity which they develop be measured by the vibrations of the pendulum."

---

[*] JOHN SIMON: *Essay on the Thymus Gland*, p. 30.

These great areas of which Simon speaks are the four ages:

1°. *The Fœtal Epoch*, or that in which the egg passes through its successive changes, from a single cell to a multiple of cells, and thence into a complex structure capable of independent existence when separated from the parent organism, at Birth. This epoch is normally of nine months' duration in the human being; thirty days in the rabbit; sixty days in the guinea-pig; fifty-six days in the cat; sixty-four in the dog; one hundred and eight days in the lion; twenty months in the elephant; and so on, varying in the different groups. Nevertheless, although this epoch is one of which the limits are less variable in each group than any of the other epochs, we must remember that Time is only *our* conventional means of measurement; and that the phenomena measured are in no sense regulated by Time. The epoch is really determined by organic conditions, not by Time, and it may therefore be prolonged or shortened by those conditions; the fœtal epoch in human life may last seven months, or eleven.

2°. *Infancy* extends from Birth to Puberty. In this epoch bodily and mental development pass rapidly and strikingly through their phases; but its limits are very variable, puberty being much earlier in some organisms than in others, and always earlier in the female than in the male.

3°. *Manhood* is a sort of table-land of life, but its limits are very variable. In it the body does not increase, except in solidity and vigour. The balance of waste and repair is tolerably even.

4°. *Old Age.* — The balance begins to lean; the movement of Assimilation slackens, and Death slowly advances. The limits of this epoch are the most variable of all.

These Ages are the Cycles of Change through which every organism must inevitably pass, if it be not earlier destroyed, and they are therefore inseparable from our conception of Life. We have seen how each epoch varies in duration, and we can understand, therefore, how the whole of each life will vary one from the other; so that although it is possible to assign the limits for the race, this can only be done as an *average,*—we can never know what will be the duration of an individual life.

And here it may be well to point out a common error in works which treat of Longevity, and one which is explicitly stated by Flourens, the latest writer on that subject.* It is that of supposing the duration of Life to be measurable by beats of the pendulum. We are told that the normal length of life for a human being is one hundred years; and that if the majority of men die earlier, it is because accident, disease, or imprudent habits, have shortened the term. The argument rests on the fallacy that, because some men reach the age of one hundred, all men would reach it were there not disturbing causes (*des causes troublantes*); but inasmuch as these disturbing causes are part of the conditions of human life, inasmuch as man is dependent on the atmosphere, on climate, and on food, and cannot escape their variations—to say that he would live a century were it not for the disturbing influences, is to say that he would enjoy a longer life, if life itself were otherwise ordained. One man lives "faster" than another; he lives more in the same period. If he live imprudently at the same time, he may hasten the changes which will bring about Death. But the utmost prudence will not shield him entirely against external conditions; nor will the wisest care do more than modify the hereditary disposition, which he brought with him into the world, and which will make him short-lived or long-lived.

---

* FLOURENS: *De la Longévité Humaine.* 1855.

What is it which terminates Life, in the natural course of things? Seeing that the organism, like the single cell, has within it the power of Assimilation—the power of repairing its substance wasted in functional activity; and seeing also that this power continues for a long period, a century and upwards, in some cases, why should it not be indefinitely prolonged? The prolongation of human life has been one of the dreams of the speculative, and its essential error has been seldom perceived. Hufeland, who wrote a book on the art of prolonging life, refers to the fact (questionable fact) of men having lived to the age of 150 and even 180 years; and he declares that he can see no reason why others should not live to 200. But I can see no reason why he should modestly stop there. Two centuries is not a magical period, beyond which

Life *must* be impossible. There is, indeed, the disturbing fact of no man having yet reached the age of 200; but if we are to argue that, because 150 and 180 have been reached, therefore 200 may be reached—we are surely entitled to argue that, if 200 can be reached, then 300, then 400, and so on, until some *necessity* be shown for the termination.

And here we bring the question to the test. What is the inherent necessity for a termination of that process of repair which we know the organism for a long period possesses? Let us first see what are the phenomena of old age; they may help us to an answer. A French physician graphically says: "Man begins in a *gelatinous*, and terminates in an *osseous* condition." The processes of Life induce changes in the structure which gradually impede its functions. "The great characteristic of all the changes," says Dr Van Oven,* which gradually occur from early to extreme old age, is *consolidation*, a diminished plasticity and mobility of parts, increased firmness of structure, and diminished bulk: interstitial fat is then absorbed; the muscles become stringy and fibrous, and at their terminations ligamentous; cartilages become bony; bones lose their cancellous structure, and become merely solid masses, whilst the blood-vessels are diminished or obliterated. The coats of the arteries become harder, and lose their tonicity; many of the smaller trunks are obliterated, whilst, however, the veins have become larger and more dilatable than they were in early life; perspiration is nearly at an end, for the skin has become harsh and dry, wrinkled and discoloured; and even, as in the vegetable world plants as they grow older become more and more woody, and the sap traverses only the larger vessels, so too in the old man the circulation is carried on only by the larger trunks, and hence the whole body becomes thinner, firmer, more harsh, more dry, and loses strength and mobility, and the power of repairing injured or regenerating lost parts. The muscular system has become so weak as to be almost useless. The once powerful man, now unable to stand erect, stoops; the shoulders are raised, and the head falls forward. In walking, the spine is much curved; the aid of a strong stick or an able arm is required to keep the body in equilibrium; the step is tottering and uncertain; the hands are unable to grasp any object firmly—they tremble in a palsied manner if the attempt be made, and the command over the particular fingers is very uncertain."

---

* VAN OVEN: *On the Decline of Life.*

In this picture of old age, some points of which may be questioned, we see plainly enough that very important structural changes must have occurred in the organism; the source of these changes lies in the very nature of the nutritive process itself. If the Repair were always identical with the Waste, never varying in the slightest degree, Life would be only terminated by some accident, never by old age. But the Repair is never thus nicely balanced with the Waste. Moreover, in Food we are constantly introducing different substances, which produce variations in the composition of the Blood, and variations in the nutrition of the parts. These differences accumulate their influence in those changes named the Ages, and they culminate in the final change, named Death. The cellular mass becomes *differentiated* into various tissues; these in turn become differentiated as they grow and act; and thus it may be said that the very processes of Life bring with them a gradual deposition of Death.

That Death is really and truly the inevitable residue of the activity of Life, and that its approach is determined by the rapidity with which developmental changes take place, is proved by the experiments of Réaumur on insects. He shows* that the duration of an insect's existence may be prolonged twofold, nay fourfold, beyond that of its species; or, *vice versâ*, it may be abridged, without doing it any other injury whatever, simply by retarding, or accelerating, those stages of metamorphosis, by which it must pass to complete its career. In Kirby and Spence's delightful work we read: "The aphidivorus flies live in the larva state ten or twelve days, in the pupa state about a fortnight, and as perfect insects possibly as long, the whole term of their existence in summer not exceeding at the very utmost six weeks. But one which I put under a glass on the 2d June 1811, when about half grown, and, after supplying it with aphides once or twice, by accident forgot, I found, to my great astonishment, alive three months after; and it actually lived till the following June without a particle of food. It had therefore existed in the larva state more than eight times as long as it would have lived in all its states, if it had regularly undergone its metamorphoses, which is as extraordinary a prolongation of life as if a man were to live 560 years."* At the moment of writing this,† I have the tadpole of a Triton, which was taken last autumn from a pond, and kept in a glass vessel of water through the whole autumn and winter in a very cold place; it has remained all this while untransformed; and its "infancy," instead of having a duration of a few days, has been prolonged over many weeks.

---

* RÉAUMUR: *Mémoires pour servir à l'hist. des Insectes,* vol. ii.; preface, p. ii.

Now it is clear that the causes which determine the acceleration or retardation of Development—which make each epoch of Life longer or shorter—must be also those which prolong or shorten Life itself. In other words, Death is determined by the metamorphoses of Life. And thus, in a certain sense, those philosophers are justified who teach that every organism has an allotted amount of vitality, which it may expend in a shorter or a longer time, but which it cannot increase. We can no more live our Life and keep it, than we can eat our cake and keep it.

In England one person in every 3100 reaches the age of 100. Some small minority exceeds the hundred years, and cases of 150 and even of 180 are cited, but the evidence for them seems questionable. Respecting the numerous cases of long-lived people, so often spoken of, it is to be observed that wherever a registration of births has been established, such marvellous examples never appear. In Sweden and Norway we have a striking illustration of this. In one country the cases of men living to 120 and 140 are said to be frequent; in the other they never occur. In the one country there are no official documents to test the age of the people; in the other there is such a test.

---

* KIRBY and SPENCE: *Introduction to Entomology*, ed. of 1856, p. 225.

† January 1859.

I do not assert that there never has been an example of human life prolonged even to 180 years. Let such an example be authenticated, and the physiologist will no more see reason for surprise, than at the child remaining eleven months in the womb, or commencing its teething at two years; for the physiologist knows that Time itself is not a factor in the sum—is not a condition of Life; it is only a mode we adopt of measuring changes. But until such an example is authenticated, we may reasonably be sceptical.

VII. DEATH.—We have seen reason to consider Death as the closing scene of a successive series—the terminal act of Life; not therefore as an antagonist of Life—a negative which destroys a positive—a darkness which shrouds up a light; but the *dénouement* of the drama, inevitable, and indissolubly linked with all that has constituted Life. Death is a new-birth: with it certain forms of existence are completed, and certain others are commenced. It is the *destruction of the organic unity, and consequently of the phenomena which were* *dependent on that unity.* This definition seems preferable to that which says, "Death is the cessation of the functions of life;" for that cessation may take place in an organism still capable of life, and therefore not dead. The congealed frog, and the dried rotifer, exhibit none of the functions of life; yet they are not dead; the unwound watch is silent, but it is not destroyed.

As there is Life of the parts, of the cells, and of the whole organism; so likewise is there Death of the parts, of the cells, of the whole organism. In each case the destruction of the organic unity brings a cessation of the organic functions.

1°. *Death of the Parts.*—The life of the cell depends upon a twofold movement of composition and decomposition, of assimilation and disintegration, of repair and waste. In the first of these movements there is *union* of elementary principles, and *growth* of the substance of the cell; in the second movement there is *separation* of elementary principles from the existing substance, and this is *decay,* or Death. Thus indissoluble are Life and Death.

Molecular death, or waste of substance consequent on vital activity, is incessant; but the organism lives on, surviving this death of the organs, as the nation survives though men perish daily.

2°. *Death of the Organism.*—Although differing only in unessential particulars, there are three heads under which all forms of death are classed: 1. Natural death; 2. Accidental death; 3. Death from disease.

It is clear, however, that whether the interruption of the vital manifestations arise from the gradual decay and obstruction which is brought about by old age, by disease, or more suddenly by wounds and poisons; in each case Death results from the interruption of that *relation* between the several parts of the vital mechanism, which brought all its units into active unity, making each play into the other. In old age this interruption comes on gradually. The old man dies in detail; his functions dwindle; his senses grow one by one less active, and many ordinary causes of sensation cease to affect him. In disease the interruption is rapid; still more so in fatal accidents.

Yet, although the organism may be dead, many of the organs may be living: the interruption may have taken place, and the unity be destroyed, but the units are not destroyed. Hours after a man is dead, his muscles live, and will contract; his glands live, and will secrete; his heart lives, and will beat; his stomach lives, and will digest.* Beard, hair, and nails will grow, and cut surfaces grow together. M. Gerdy says that the head rolling on the scaffold is not dead, for the features can be made to contract, as in horrible passion, twelve or fifteen hours after decapitation. Dr Ure experimented on the body of a man who had been hanged, and frightened the spectators by the sight of this corpse starting up, its features convulsed as with passion and terror, when a powerful galvanic battery was applied.

---

* In certain fishes, digestion will continue twenty-four hours after death.—CHAS. ROBIN: *Mémoires de la Société de Biologie*, v. 134.

On the other hand, many of the parts may be dead while the organism continues to live. In disease and wounds this is constantly the case. The negroes on the African coast frequently begin to putrefy at their extremities before death comes on—their limbs rotting, while heart, lungs, brain, and stomach are active.

VIII. APPARENT DEATH.—It is of the first importance that we should be able to discriminate real from apparent death; and that we should have some simple memorable indications which may preserve us from those fearful mistakes of "buried alive," common enough in ancient times, and not wholly unknown in our own.

Writers on this subject are fond of citing the case of François Civille, a Norman gentleman of the days of Charles IX., who described himself as having been "thrice dead, thrice buried, and thrice resuscitated by the grace of God." Winslow, the famous anatomist, is said to have been twice buried. If many of the reputed dead have awakened from their lethargy in time, there is a frightful suspicion that many others did *not* awake in time. It is easy to say that only culpable ignorance, and culpable carelessness, can in our day permit such mistakes; but I find an authentic case so late as 1843, in France, in which a beggar was found drunk, on the road to Nantes, and was mistaken for dead; all preparations for burying him were made, when those who came to carry him away were startled by his opening his eyes, shaking himself, and asking where he was? Nay, so late as 1849, the French Academy accorded a prize of 1500 francs to the essay of M. Bouchut,* the money having been furnished by a philanthropic professor at the University of Rome, P. Manni.

The definition of Death which I have proposed, furnishes the real clue in every case. The common notion that Death is the cessation of the vital functions, is no guide. It does not discriminate between Lethargy and Death. Those who were buried alive gave no sign whatever of Life; and except one little detail, which will presently be noted, there was nothing which could lead by-standers to the suspicion that a corpse was not before them.

Mr Braid, in his *Observations on Trance*, has collected examples of the Indian Fakeers, which seem tolerably authenticated. "In one of these vouched for by Sir Claude M. Wade, the Fakeer was buried in an underground cell, under strict guardianship for *six weeks;* the body had been twice dug up by Runjeet Singh during the period of interment, and had been found in the same position as when first buried." In another case narrated by Lieutenant A. Boileau in his *Narrative of a Journey in Rajuarra*, 1835, "the man had been buried for ten days in a grave lined with masonry, and covered with huge slabs of stone, and strictly guarded; and he assured Lieutenant B. that he was willing to submit to an interment of twelve months' duration, if required. In a third case, the trial was made under the direct superintendence of a British officer, a period of nine days having been stipulated for on the part of the devotee; but this was shortened to three days at the desire of the officer, who feared lest he should incur blame if the result was fatal. The appearance of the body when first disinterred is described in all instances as having been quite corpse-like, and no pulsation could be detected in the heart or arteries. The means of restoration employed were chiefly warmth to the head, and friction to the body and limbs."

---

* BOUCHUT: *Traité des signes de la mort et des moyens de prévenir les enterrements prématurés.* 1849.

Let us now glance at the various signs of Death, and estimate the value of each.

1°. *The cadaverous aspect of the face*, familiar to every one, is by no means a certain characteristic, since it is observed in persons much exhausted by chronic maladies; and, moreover, it is not observed in persons who have died from accident, or from acute malady.

2°. *The coldness and lividness of the skin* are very equivocal. Those who die from chronic maladies or violent bleeding rapidly become cold; but those who die from asphyxia are a long while becoming cold, nor does the body lose its tints for many hours. I have already noticed surprising cases of the body retaining its warmth many hours after death.*

3°. *Rigidity of the limbs* is one of the most reliable signs. It is always observed sooner or later: sooner in those weakened by old age, or long illness, than in the young, or in those who perish from accident, and in those suffocated, especially those suffocated by carbonic acid. It persists generally twenty-four to thirty-six hours; but the causes which retard its first appearance prolong its duration; for instance, after suffocation by carbonic acid it does not come on till about fifteen hours after death, and lasts several days if the air be at once cold and dry. This cadaveric rigidity is to be distinguished from the rigidity of congelation, and from that convulsive rigidity produced by certain nervous states: in the first case a limb or finger is heard to *crick* when bent—which is said to arise from the breakage of little congealed masses; in the second case, the bent limb returns to its original position when the force which bent it is removed, whereas in the dead body a limb once bent retains that position.

---

* See vol. i. chap. vii. p. 444.

4°. *Cessation of the Respiration and Circulation.*—It is worthy of remark that all the ordinary means of ascertaining whether the heart has ceased to beat, and the lungs to act, have little scientific value. You may fail to detect the slightest pulse at the wrist, temple, or chest. You may place a bright mirror before the mouth, and it will not be obscured; you may hold a flame before the mouth, and it will not flicker; you may place threads, or down, on the lips, and there will be no stir; yet the patient may be living. Nevertheless it is tolerably certain that, in the higher animals at least, actual cessation of the circulation must, if prolonged, be regarded as the sign of death. How then to establish the reality of this cessation? M. Bouchut says there is but one test, and that is infallible—namely, *auscultation.* By the aid of the stethoscope the circulation can be *heard,* when it can be detected by no other means. In the Report of the Commission appointed by the Academy, this cessation of the heart's action, as detected by the stethoscope, is considered to be *the* infallible sign of death—and the time which this cessation must be observed may be estimated at five minutes. If during five minutes no sound whatever be heard, the certainty of death may be relied on.

Taken in conjunction with cadaveric rigidity, this last sign may be said to furnish a positive assurance of Death. But in cases where the stethoscope cannot be applied, or where no skilled practitioner is present to apply it. there is one other sign—namely, *Putrefaction.* This cannot be mistaken. It manifests itself at first as a blue-green tint over the skin, *especially about the abdomen,* and soon spreading all over the body. This sign, if preceded or accompanied by cadaveric rigidity, can be relied on as infallible.

CONCLUSION.

AND now, reader, we have reached the end of our toilsome, but, let me hope, not profitless journey. There are many other questions of interest and importance which the physiologist has to study, but they do not fall within the range of common interests, and must therefore be omitted from these pages. My object was thus explained in the original prospectus:—

"No Scientific subject can be so important to Man as that of his own life. No knowledge can be so incessantly appealed to by the incidents of every day, as the knowledge of the *processes by which he lives and acts.* At every moment he is in danger of disobeying laws which, when disobeyed, may bring years of suffering, decline of powers, premature decay. Sanitary reformers preach in vain, because they preach to a public which does not understand the laws of life—laws as rigorous as those of gravitation or motion. Even the sad experience of others yields us no lessons, unless we understand the *principles* involved. If one man is seen to suffer from vitiated air, another is seen to endure it without apparent harm; a third concludes that 'it is all chance,' and trusts to that chance: had he understood the *principle* involved, he would not have been left to chance—his first lesson in swimming would not have been a shipwreck.

"There is a daily increasing desire for scientific knowledge. Science passes from its laboratories into the public thoroughfares. But although thousands are now anxious to understand the chief physiological laws, the majority cannot overcome their invincible repulsion from anatomical details. Many would rather remain ignorant than gain knowledge through such paths. It is believed by the Author of the proposed work that a clear and accurate conception of the chief physiological laws, sufficient for ordinary practical guidance and for scientific culture, may be impressed upon the mind, and illustrated by memorable facts, *without once appealing to anatomical knowledge.* It will be his object to *expound principles* rather than to teach a science; and these principles will be illustrated by the most striking *facts* hitherto ascertained. Assuming the position of a lecturer addressing a miscellaneous audience, he will imagine that beside the Medical Student there sits an intelligent Artisan—beside the Man of Letters sits the Mother of a family; and he will endeavour to be intelligible and interesting to all, while reproducing the *latest* discoveries of European investigators, and the results of *original research.*"

How far this object has been attained, the reader must judge; but I have received such cheering and generous encouragement from the profession and the public, during the course of monthly publication, as fully to repay the great pains and labour bestowed on the work; and I now part from it, and the reader, with reluctance and regret.

INDEX.

ABD-EL-KADER on the breeding of the Arab horse, ii. 391.
Absorbents or lymphatics, *see* Lymphatics.
Absorption not universally a part of digestion, i. 193.
Abstinence, power of, among the lower animals, i. 12—the effects of partial, 20—its effects on animal temperature, 434.
Acardiac births, cases of, i. 323.
Accidents, hereditary transmission of, ii. 381.
Accidental death, what, ii. 445, 446.
——— peculiarities, hereditary transmission of, ii. 380.
Acid, effects, &c., of the, in the gastric juice, i. 209.
Acids, value of, as alimentary principles, i. 148—free, in the gastric juice, 205, 206.
Actiniæ, influence of light on the, ii. 325.
Activity, hunger a stimulus to, i. 2.
Acquapendente, Fabrice d', the valves of the veins discovered by, i. 305, 308.
Adipose tissue, proportion of fat in, i. 76.
After-tastes, examples of, ii. 305.
Age, relation of, to animal heat, i. 425 *et seq.* *See* also Old age.
Air, importance of fresh and danger of impure, i. 350 *et seq.*—vital, the composition and properties of, 358—quantity of, inspired and expired, 367—alteration of it in respiration, 368.
Air cells, the, in the human lungs, i. 364.
Albinos, light colour of the black pigment of the eye in, ii. 334.
Albumen, how classed by Liebig, i. 71—incapable of supporting life by itself, 81, 82—discovery of Mulder regarding vegetable, 113—experiments on injection of, 117—as an alimentary principle, 122 *et seq.*—proportion of, in various substances, 123—in vegetables, 124—power of the organism to form, 183—coagulation of, in flesh, 158—action of the stomach on, 208, 209—action of bile on, 224—proportion of, in blood, 262.
Albuminous principle, necessity of, in food, i. 120.
——— substances, refutation of Liebig's view regarding, i. 75—shewn to be heat-producers, 90—their rapid destruction within the body, 92, 93—their instability, 128—action of the gastric juice on, 206—influence of fat on digestion of, 211—amount dissolved by the gastric juice, 214—results of stomachic action on, 215—action of the pancreatic juice on, 226.
Alcohol as an alimentary substance, i. 141—its action concentrated and diluted, 142—its dangers, 145—its physiological action, *ib.* 177, 178—the alleged effects of, on the blood, 384.
Alimentary canal, digestion the function of the, i. 193.
——— principles, the various, i. 122 *et seq.*
——— substances, what, i. 59—influence of the body itself on, 63.
Alkalis, influence of, on the gastric juice, i. 212.
Almonds, oil in, i. 134.
Ammonia, influence of, on the coagulation of the blood, i. 260.
Amœba, the, and resemblance of the blood corpuscles to it, i. 250.
Amphioxus, peculiarity of cerebral development of, ii. 91—probable part played by the spinal cord in, 266.
Amputation, sensation referred to the part removed after, ii. 296.

Anæsthetics, the value of, ii. 148.

Anatomical connection determines functions, ii. 46.

Anatomist, views of the, on muscle, contrasted with the chemist's, i. 77—and on blood, 79.

Anatomy, general, what, i. 4—scientific process of, as regards the study of life, &c., and the arguments against it, ii. 86.

Ancients, varieties of food among the, i. 53.

Andersson on the effects of thirst, i. 83.

Andral on the effects of disease on the cerebellum, ii. 129.

Anemone nemorosa, how its odour affects different persons, ii. 316.

Animals, consumption of water by different kinds of, i. 47—on the application of Liebig's classification to their feeding, 73—Lawes and Gilbert's experiments on feeding, 94—nutrition in, fallacy regarding it, 97—alleged incapability of their assimilating inorganic substances, 98—these indispensable to them, 99—influence of their respiration on air, 354—withdrawal of oxygen from the air by them, 394—varieties as regards respiration in, 396—temperature of, 405—the distinction of, into warm and cold-blooded, 418—experimenting on live, defended from charge of cruelty, ii. 86 *note*—nervous systems, &c., of the lower, 44 — systemic consciousness in these, 75—alleged dependence of sensation on the brain in, 85—relative weight of the brain in, 98—sensation in headless, 176 *et seq.*—spontaneous movements in brainless, 184 *et seq.*—the ground on which feeling is assumed on behalf of them, 237—this proof equally applicable in case of decapitated, *ib.*—manifestations and presumed origin of spontaneity and choice, 238—experiments on decapitated, shewing spontaneity and choice, 239 *et seq.*—the sleep of, 346—permanency of breed of, 377—colours and spots transmitted in, 380.

Animal body, amount of waste fatal to the, i. 9.

——— diet, not always strength-giving, i. 121.

——— effluvia, density and slow diffusion of, ii. 317.

——— food, cases in which poisonous, i. 59—arguments in favour of, 174.

——— heat, production of, as a source of waste, i. 17—relations of respiration, &c. to, 454, *See* also Heat.

——— life, what, as distinguished from vegetable, ii. 430—or relative life, Bichât's *ib.*—peculiar to animals, *ib.*—its organs not invariably symmetrical, 432—the so-called law of intermittence as characterising, 432.

Animal organs, the, said to be given by the male, ii. 391, 398.

——— sensibility, the so-called, ii. 77.

——— spirits, the old hypothesis of, ii. 7.

Animalcules, animal heat of, i. 412.

Anterior nerves, muscular sensibility derived through the, ii. 37—their sensory function, 38.

Antipathy, cases of, as regards food, i. 60.

Aorta, the, i. 295—its position, 290, 320—effect of tying it, 279—in the fish, 324.

Aplysia, action of the intestinal juice in the, i. 229—its nervous system, ii. 44.

Apparent death, cases and phenomena of, ii. 447.

Appetite, the first gradation of hunger, i. 3—periodicity of its recurrence, 6—the sensation of, 27—increase of, with cold, 184.

Appetites, origin of, in systemic sensations, ii. 117.

Apricot, sugar in, i. 139.

Aqueous humour of the eye, the, ii. 327.

Arab horses, influence of each parent in the breeding of, ii. 391.

Arbor vitæ, the, in the brain, ii. 89.

Arcet, M. d', his process for extraction of gelatine, i. 130.

Aristotle, definition of life by, ii. 424.

Arnold, Prof., opposition of, to Sir Charles Bell's views, ii. 30—experiments on the posterior nerves, 35.

———, criticisms of, on Marshall Hall's observations, ii. 188 — his work against the reflex theory, and views on the spinal cord, 229.

Arterial blood, change of, to venous, i. 242 — distinction between it and venous, 265—views of the ancients regarding it, 298.

——— bulb, the, in the fish, i. 324.

Arteries, belief of the ancients regarding, i. 297—origin of their name, 298—discovery of Galen regarding, *ib.*—their valves, 309—rate of the circulation in them, 328 — their action, 337.

Ascidian, nervous system of the, ii. 44.

Aselli, discovery of the lymphatics by, i. 316.

Asphyxia, *see* Suffocation.

Ass's milk, caseine in, i. 125—fat in, 135.

Assimilation, process of, within the organism, i. 52—necessity for, as regards the food, 117—assumed to go on chiefly during sleep, ii. 359—a continuous process, 360.

Association of ideas, influence of, as regards voluntary and involuntary actions, ii. 204.

Astley, Mr, taming of wild animals by thirst, i. 38.

Atavism, what, and phenomena referable to it, ii. 405.

Atmosphere, how its balance is restored, i. 393 *et seq.*

Attention, relations of, to sensation, ii. 50—difference between it and consciousness, 53.

Auditory nerve, the, ii. 319.

Auricles, the, i. 293, 294—in the fish, 324.

Auscultation, importance of, for determining the reality of death, ii. 451.

Austria, horse-flesh used in, i. 163.

Austrian lip, the, a case of hereditary transmission, ii. 380.

Automatic action, relations of, to sensation, ii. 58.

Bachs, the congenital musical genius of the, ii. 402.

Back, tactile sensibility of the, ii. 297.

Baden, sale of horse-flesh in, i. 162.

Baillarger, M., conclusions of, regarding the white and gray matters of the brain, ii. 98.

Bain, Mr, on volition, and on voluntary and involuntary actions, ii. 208, 209 *et seq.*—on respiration, 279—on the musical note, 322.

Bakewell, value attached to the ewe by, ii. 397.

Baking, action of, on meat, i. 155.

Baly, Dr, case of partial loss of sensation from, and examination of inferences from it, ii. 261—on the excitation of taste by mechanical stimuli, 302.

Barclay, Captain, command over sleep possessed by, ii 354.

Bardeleben, experiments by, with salt, i. 142.

Barley, albumen in, i. 124—gluten in, 128.

——— meal, sugar in, i. 138.

Bartholin, discovery of, regarding the lymphatics, i. 317.

Bathing, effects of, on thirst, i. 34.

Beans, nitrogen in, and their nutritive value, i. 82—gluten in, 128.

Beaumont, Dr, on the cause of the sensation of hunger, i. 29—on the influence of fats on the formation of bile, 136—table of digestibility of various meats from, 157—his observations on the gastric juice, digestion, &c. 201—on the muscular action of the stomach, 208.

Béclard, definition of life by, ii. 425.

Becquerel, his thermo-electrometer, i. 410.

Beddoe, Dr, experiment by, on the flesh of the donkey, i. 165.

Bee, the brain of the, the sensibilities, &c. manifested by it, ii. 91, 93—affinity between its wings and those of the bird, 93—and between their brains, 94.

Beef, muscle of, albumen in, i. 123—and fibrine, *ib.*—why more digestible than veal and chicken, 152—boiled, roasted, &c., time in which digested, 157, 158.

Beer as an article of diet, i. 175—how classed by Liebeg, 71.

Beet-root, sugar in, i. 139.

Beetle, the nervous system of, ganglia of head and body, &c., ii. 178.

Bell, Sir Charles, the discovery of, regarding the nerves, ii. 26 *et seq.*—views of, on sensation, 143, 144—his so-called muscular sense, 280 *et seq.*—case recorded by him illustrating it, 285.

Bérard, M., cases of alleged fasting from, i. 13—on the life-sustaining power of water, 20—on cases of abstinence from liquids, 48—on transfusion of blood, 276—on the alleged discovery of Cæsalpinus, 304—on that of Harvey, *ib.*—on respiration, 358—on the so-called olfactory nerves, ii. 310.

Berger and Delaroche, experiments by, on animal heat, i. 420.

Bergmann and Leuckhart, their definition of the brain, ii. 159.

Berlin, sale of horse-flesh in, i. 163.

Bernard, Claude, experiment by, on a dog, i. 46—experiments by, on injection of sugar, &c., 117—on the conversion of gelatine, 134—discovery of the sugar-forming power of the liver by, 140 *note*—on the action of the saliva, 195—on chymification, 203—parallel by, between the action of the gastric juice and boiling, 215—on the action of bile in the stomach, 224—on the pancreatic juice, 226—the digestive tract from, 230, 320—on the lacteals, 232—researches of, on coagulation of blood, 258—on venous blood, 269—on the quantity of the blood, 272—experiments by, on vitiated air, 374—on respiration, 381—and on carbonic oxide, 385—experiments of, on combustion and respiration, 390—experiments of, on the nerves, ii. 16—hypothesis of, respecting the nerves, 38—on sensibility, 72—on the action of the Woorara poison on the nerves, 79—experiment on a frog by, 258—case from, of sense of smell in absence of olfactory nerves, 311—on the relation between these, 313.

Berthé, classification of oils by their digestibility by, i. 135.

Betz, the muscular action of the stomach denied by, i. 203 *note*.

Beyer, case of animation in brainless infant recorded by, ii. 227.

Bichât on the tissues, i. 3—views of, on the capillaries, 314—and on respiration, 352—definition of life by, ii. 425—his classification of life, 430, 431—on the vegetal and animal organs, 431—his laws of intermittence and continuity, 432.

Bidder and Schmidt on the amount of food required, i. 10—on the functions of water in the body, 37—researches of, on the action of saliva on starch, 198, 199—on the acid of the gastric juice, 209—on the quantity secreted, 213—on the effects of fats on the bile, 220—on the action of the intestinal juice, 229.

Bile, influence of fats on formation of, i. 136—its action on the food, 220—action of fats and water on it, 221—part played by it, 232 *et seq.*—excess and deficiency of, as a cause of indigestion, 232.

Bilharz on the blood parasite, i. 252.

Biliousness, alleged production of, by fats, i. 221.

Biology, importance of form in, i. 112.

Bird, the, constancy of its temperature, i. 406—its wings and those of the bee, ii. 93—analogy between the brains of the two, 94.

Birds, power of resisting starvation among different classes of, i. 11—the flesh of, as an article of diet, 158—the blood discs in, 244, 246—the valves in their veins, 307—respiratory organs in, 362—respiration and animal heat among, 447—the cerebral development of, ii. 91—effects of the removal of the cerebellum on, 124—relations of the latter to the vertebral column in, 127.

Birth, cases of, without a heart, i. 323.

Bischoff, estimate of the quantity of the blood by, i. 273.

Black Hole of Calcutta, Mr Holwell's narrative of, i. 38 *et seq.*

Black pigment of the eye, the, ii. 326, 332—peculiarity of it in invertebrates, 333—its importance in relation to sight, 334.

Blainville, M. de, on the supposed discovery of Cæsalpinus, i. 303—definition of life by, ii. 425.

Blind, sensibility of the, to sound, ii. 325.

Blindness, congenital, its frequency, ii. 356.

Blondlot, experiments on dogs by, i. 202—on the muscular action of the stomach, 203—on chymification, 208.

Blood, effects of starvation on the, i. 8—as a food, how classed by Liebig, 71—different views of the chemist and the anatomist on, 79, 80—proportion of salt in it, 104—regarded as a standard of food, 115—reasons against its being so, 116—experiments on feeding with, 118—fibrine in, 124—the clot, *ib.*—iron in, 147—effects of vegetarianism on, 175—the function of digestion to make it, 193—the river of life, 239—the so-called purifying it, 240—quantity of nutritive matter and waste conveyed by it, 241—its weight, *ib.*—its change from arterial to venous, 242—variety of substances carried by it, *ib.*—the discs, 243—the colourless corpuscles, 249 *et seq.*—parasite in it, 251—is it alive, 254—its liquid plasma, 255—cases in which not coagulable, 258—cause of the coagulation, 259—its chemical composition, 261 *et seq.*—arterial and venous, 265—its quantity, 271—the letting of, 274—transfusion, *ib.*—what are the vivifying elements in, 278—it nourishes and stimulates, 281—oxygenation of, the result of respiration, 356, 359—state in which carbonic acid and oxygen exist in, 371—effects of carbonic oxide on, 385, 386—excitation of taste by reaction through it, ii. 306.

—— circulation of the, its wonders, i. 270—did Harvey discover it, 291—diagram of it, 293—its course from the heart, &c., 294—history of its discovery, 296 *et seq.*—errors of the ancients, 297—the general, discovered, 302—discovery of the valves of the veins, 305—gaps in Harvey's doctrine, 310—the capillaries discovered, 312—and the lymphatics, 315—its course recapitulated, 319—cause of it, 322—it without a heart, 323—its rapidity, 327—action of the arteries, 337—of the capillaries, 338 *et seq.*

—— cells, the vivifying element in the blood, i. 280—their power of absorbing oxygen, 281—their relation to nutrition, 284 *et seq.*

—— discs, representation of the, i. 8, 243 *et seq.*—influence of their form, &c., 246—their structure, 247—analysis of them, 263.

Bloodhound, the, why it hunts with its nose to the ground, ii. 317.

Blood-letting, extraordinary cases of, i. 274.

Blundell, revival of transfusion of blood by, i. 276.

Böcker, Dr, experiments of, on the action of tea, i. 176.

Body, the, the comparison of it to a steam-engine examined, i. 7—large proportion of water in, 36—its influence on the food, 63—everywhere animated by the soul, ii. 224.

Body-sensations of Prochaska, the, ii. 42.

Boerhaave, seat assigned the sensorium by, ii. 41.

Boileau, reception of Harvey's doctrine by, i. 310.

Boiling, action of, on meat, i. 155—resemblance of the action of the gastric juice to, 215.

Bolivia, clay eating in, i. 54.

Bomare, on hybrids between wolf and dog, ii. 399.

Bones, effects of starvation on the, i. 22—phosphate of lime indispensable to, 100—the extraction of gelatine from, 130—nutritive properties of, 132, 133,

Bone soup, innutritious qualities of, i. 130, 131, 132.

Bonito, temperature of the, i. 409.

Borelli, researches of, on coagulation of the blood, i. 257.

Bosjesmans, gluttony of the, i. 183.

Bouchut, M., on auscultation in determining the reality of death, ii. 451.

Bouillaud, experiments of, on the cerebrum, ii. 106—views of, as to the cerebellum, 125, 136.

Bourbon nose, the, a case of hereditary transmission, ii. 380.

Boutekoe, seat assigned the sensorium by, ii. 41.

Bronchio-cephalic veins, the, destitute of valves, i. 307.

Braid, Mr, cases of simulated death from, ii. 448.

Brain, the, influence of starvation on, i. 22, 23—proportion of fat in it, 76—its relations to sensation, &c., ii. 4—views held regarding it, 5—views of Prochaska on it, 42—the gray and white matters of, 43—identity of organic relation between ganglia and it, 45—long-continued misconception regarding it, 83—it currently regarded as the sensorium commune, 84—proof that it is not so, *ib. et seq.*—inconsistencies of physiologists regarding it, 85—its structure, 87—view of its component parts, &c., 88, 89—diagram of vertical section, 90—its development in the different vertebrates, *ib.*—connection of intelligence with it, 91—its essential identity in the bird and the bee, 94—shewn by comparative anatomy to be not the sole centre of sensation, 96—its weight in man, &c., 97—the white and the gray matter, 98—functions of Cerebrum, 99—the researches and conclusions of Flourens, &c., &c., on the effects of removing the Cerebrum, 104 *et seq.*—relative size of it in different animals, and relations of this to their intelligence, 120—functions of the Cerebellum, 121 *et seq.*—functions of the Medulla Oblongata, 137—summary of its functions, 149—proofs of voluntary movement after its removal, 184—reflex action in it, 191 *et seq.*—errors arising from holding it to be the sole seat of sensation, 234—proportion borne by spinal cord to, in different animals, 265—visual images not transmitted as such, to, 329—excitement of it prevents sleep, 356—temporary congestion of it the cause of sleep, 358, 363—restorative power of sleep with regard to it, 361 its activity during sleep, 373—probable action of it in dreams, 366 *et seq.*

Brains as an article of diet, i. 159.

Bread countries, the, as defined by Schouw, i. 172.

Breathing, as a source of waste in the body, i. 18—water thrown off in, 34—an example of reflex action, ii. 57—relations of, to the will, 198. *See* also Respiration.

Breathing-mechanism, the, i. 355. *See* Respiration.

Breed, permanency of, ii. 377.

Bronchial tubes, the, i. 295—their office, 364—representation of, 365.

Bronchitis, what, i. 364.

Brothers, frequent differences of character among, ii. 379.

Brown, Sequard, on transfusion of blood, i. 276—researches of, on the blood cells, 281—on the functions of oxygen and carbonic acid in it, *ib. et seq*—on animal heat, 447—experiments by, on the posterior nerves, ii. 35—cases recorded by, of destruction of the cerebellum, 132—experiments of, on Flourens's vital point, 146—on the muscular sense, 285.

Bruch on venous and arterial blood, i. 268.

Brücke, views of, on the coagulation of the blood, i. 261.

Brunner's gland, i. 228.

Buckle, Mr, his views as to inherited qualities examined, ii. 376 *et seq.*

Budd, Dr, on the value of vegetable acids, i. 148.

Buffon on hybrids between wolf and dog, ii. 399.

Burdach on the life-sustaining power of water, i. 20—experiments by, on feeding rabbits, 179—on the transmission of colour, ii. 399.

Busk, Mr, on the blood discs, i. 249 *note*.
Butter, proportion of, in milk, i. 119.
Byron, Admiral, experiences of, after starvation, i. 6.

Cabbage, gluten in, i. 128.
Cæsalpinus, discovery of, regarding the circulation, i. 302.
Calamus scriptorius, the, ii. 146.
Calmeil, views of, on the spinal cord, ii. 228.
Calcar, the illustrator of Vesalius, i. 299 *note*.
Camper, seat assigned the sensorium by, ii. 41.
Cannibalism, congenital tendency to, ii. 385, 386.
Capillaries, the, i. 241—on the liver, 242—transmission of fluids through their walls, 107—not known to Harvey, 292—discovery of them, 312—rate of the circulation in, 327, 328—their action in the circulation, 338—supposed cause of it, 339 *et seq.*
Capillary circulation, continuance of, after death, i. 326.
Carbon, proportion of, in the blood, i. 262—oxidation of, the alleged cause of animal heat, 440.
Carbonic acid, proportion of, in venous and arterial blood, i. 266—its origin, 267—its function in the blood, 281 *et seq.*—suffocation by, 347—proportion of, in air, 354—its removal by respiration, 368—apparatus for shewing this, 369—state in which it exists in the blood, 371—its presence arrests respiration, 378 *et seq.*—proportion which is fatal, 381, 382—not directly poisonous, 383—how removed from the atmosphere, 393 *et seq.*—its effects on odours, ii. 315.
Carbonic oxide, poisonous character of, i. 384—experiments on, 385.
Carburetted hydrogen, why inodorous, ii. 314.
Carnivora, the, their power of resisting starvation, i. 11—peculiarities of their feeding, *ib.*—feed chiefly on herbivora, 115 *note*.
Carotid artery, the, i. 230, 320.
Carpenter, Dr, on the use of alcohol, i. 143—on circulation apart from the heart, 323—on the emptying of the arteries after death, 326—on the cerebellum in the mammalia, ii. 128—on reflex action in the brain, 191 *et seq.*—on taste in the lower animals as a security against poisoning, 304.
Carus, opposition of, to the reflex theory, ii. 229.
Caseine, proportion of, in milk, i. 119—as an alimentary principle, 125.
Cat, the, its diet, i. 62.
Caviare, albumen in, i. 123—oil in, 134.
Cell, the, acts as such only while retaining that form, i. 112—life of, ii. 419, 421, 426 *et seq.*—description of it, 427—the ultimate form of organic life, 428.
Cells, proportion of, in blood, i. 262.
Centipede, nervous system of, ii. 45—analogy of its nervous system to that of man, 88.
Cereals, the, albumen and fibrine in, i. 124.
Cerebellum, the, its position, ii. 88—disposition of gray and white matter in it, 89, 98—its relative weight in man, 97—its functions, 121—its composition, *ib.*—its position, &c., 122—Gall's hypothesis regarding it, *ib.*—experiments, &c., of Flourens, 123—the hypothesis that it is the seat of muscular co-ordination, 124 *et seq.*—its relation to muscular movements, 126—experiments, &c., of Schiff, 126—influence of disease, 128 *et seq.*—case in which wanting, 129—conclusions from this case, 131 *et seq.*—not the seat of muscular co-ordination, 133—a seat of sensation and volition, *ib.*
Cerebral excitement prevents sleep, ii. 356.
Cerebro-spinal axis, the, ii. 14, 87—in the insect, 88.
Cerebrum, the, its position, ii. 88—its convolutions, 89—its relative weight in man, 97—arrangements of gray and white matter in, *ib.*, 98—its functions, 99—effects of pressure on it, 100—case illustrating effects of pressure on it, 101—the researches of Flourens regarding it, 104 *et seq.*—effects of its removal on memory, 105, 107—experiments &c., of Bouillaud on it, 106—of Longet and of Dalton, 108—conclusions of the latter as to dependence of intellectual sensations on it, 109—general conclusions from these experiments, 112—what are its functions, 113—no nerves originate in it, *ib.*—views of Prochaska, &c., on it, *ib.*—its relations to sensibility, *ib. et seq.*—subdivision of its sensibilities, 114—difficulty with regard to this, 115—hypothesis proposed, *ib.*—dependence of the spinal cord on it, 119—the seat of intelligence and emotion, 266.
Chabert, the fire king, i. 416.
Chambers, Dr, on the action of tea, &c., i. 176, 177—on the importance of mastication, 200.
Chambon on hereditary transmission of colours in animals, ii. 380.

Charcoal, suffocation by fumes of, i. 347.
Cheek, tactile sensibility of the, ii. 297.
Cheese, caseine in, i. 125—effects of, on flavour of wines, ii. 305.
Chemical action, intimate connection of, with vitality, ii. 418.
——— affinities, not controlled by life, ii. 417—effect of light on, i. 111.
——— laws, inadequate to explain physiological phenomena, i. 66—cause of this, 68.
——— process, the, in digestion, i. 193.
——— theory of food, errors, &c. of the, i. 57 *et seq.*—its insufficiency, 112, 113.
Chemist, views of, on muscle, contrasted with the anatomist's, i. 76—and on blood, 79.
Chemistry, true relations of, to the science of food, i. 63.
Cherries, sometimes pernicious, i. 60—sugar in, 139.
Cheselden, experiments of, on the effects of piercing the tympanum, ii. 320.
Chest, tactile sensibility of the, ii. 297.
Chewing the food, the, not universally a part of digestion, i. 193.
Cheyne, Dr, case of control over the heart recorded by, ii. 223.
Chicken, muscle of, albumen in, i. 123—and fibrine, *ib.*—why less digestible than beef, 152.
Child, the sleeping, illustrations of choice from, ii. 241—picture of, asleep, 347.
Children, necessity of caution as to diet of, i. 61.
Chloride of sodium, presence and importance of, in the body, i. 103.
Chlorine, effect of light on its affinity with hydrogen, i. 111.
Chocolate, as an article of diet, i. 175.
Choice, manifestation of, and its presumed origin in animals, ii. 238—implies sensation and volition, 239—indications of it in decapitated animals, *ib. et seq.*, 243 *et seq.*
Choroid coat of the eye, the, ii. 326.
Chossat, the experiments of, on inanition, i. 9, 12, 21—experiments by, on withholding phosphate of lime, 100—and on the influence of fasting on temperature, 434, 435.
Chyle, nature of the, i. 229—what, 315.
Chyme, what, i. 206—representation of, 207.
Circulation of the blood, the, its discovery, i. 291—continuous, ii. 433—cessation of it as a sign of death, 450. *See* also Blood.
Civille, François, the case of, ii. 447.
Clarke, Dr E., on the uses of the tympanum, ii. 320.
——— Lockhart, views of, on the cerebellum, ii. 133—on the medulla oblongata, 139 *note*—his microscopic preparations of the spinal cord, 270 *et seq.*
Clay, allaying of hunger by eating, i. 30.
Clay-eaters, tribes of, i. 58 *et seq.*
Climate, influence of, on demand for food, i. 182.
Clot of the blood, the, i. 124, 256.
Clypeus, peculiarity regarding the, i. 277 *note.*
Coagulation of the blood, the, i. 256.
Cochin fowls, on hybrids of, ii. 395.
Cochrane, Captain, on the gluttony of the Yakuti, i, 185.
Cocoa nut, oil in, i. 134.
Coecum, the, i. 219—its position, &c., 230, 231, 320.
Coffee, thirst increased by, i. 38—cases in which pernicious, 60—as an article of diet, 175, 176—effects of, on respiration, 371.
Cold, influence of, on the circulation, i. 342—stupefying effects of, 439—when dangerous, *ib.*—the sensation of, ii. 291—anecdote shewing its power to produce sleep, 352 *et seq.*—moderate, prevents it, 356.
Cold-blooded animals, influence of temperature on feeding of, i. 6—compared with warm-blooded as regards starvation, 10—circulation after removal of heart in, 325—can breathe in air irrespirable by warm-blooded, 379—effects of temperature on respiration of, 400—distinction between, and warm-blooded, 418 *et seq.*—respiration and animal heat among, 446.
Cold climates, alleged greater consumption of heat-producing food in, i. 87—great consumption of fat in, 89—increased demand for food in, 182.
Colin and Bérard on the action of the pancreas, i. 227.
Colliers, case of, shut up without food, i. 27.
Colombo, the pulmonary circulation discovered by, i. 302.
Colon, the, i. 219—its position, &c., 230, 231, 320.
Colour, hereditary transmission of, in animals, ii. 380—from which parent derived, 398 *et seq.*
Colour-blindness, the phenomena of, ii. 341.
Colourless corpuscles, the, in the

blood, i. 249—resemble the amœba, 250.
Combette, case recorded by, of absent cerebellum, and conclusions from it, ii. 129 *et seq.*
Combustion, theory of respiration being identical with, i. 390.
Comparative anatomy, value of, in the study of life, &c., ii. 86, 87—teaching of, regarding the ganglia and the brain, 94, 96.
Concentrated and diluted agents, difference between action of, i. 142 *et seq.*
Condillac, the hypothetical statue of, ii. 344.
Conditions, influence of different, with regard to food, i. 109.
Congestion, partial, the cause of sleep, ii. 363.
Conscious sensibility, the so-called, ii. 72.
Consciousness, connection of the nervous system with, ii. 6—various definitions of, 48—distinction between, and attention in relation to sensations, 42—relation of, to sensation, 61—concurrence of all our sensations to form, 65—that of existence, 66—general, 69—particular, *ib.*—the various forms of—the systemic, 74—the sense, 77—the thought, *ib.*—ambiguity in use of the word, and confusion from this, 194, 195.
Constitution, parental influence on, ii. 384 *et seq.*
Constructive association, Mr Bain on, ii. 217.
Consumption, hereditary transmission of, ii. 384, 385.
Continuity, Bichât's law of, ii. 432.
Contractility, muscular, analogy between, and nervous sensibility, ii. 73, 195.
Control, definition of, with regard to action, ii. 201—its connection with reflex feeling, 202 *et seq.*
Convalescence, peculiarities of appetite during, i. 6.
Convolutions, supposed influence of, in the brain, ii. 98.
Cooking, action of various kinds of, on meat, i. 154 *et seq.*
Cooper, Sir Astley, case of long-continued insensibility recorded by, ii. 101—case of partial paralysis recorded by, 135.
Cornaro, quantity of food consumed by, i. 181.
Cornea of the eye, the, ii. 326.
Corpora quadrigemina, the, by some included in the brain, ii. 90—their functions, 137.
——— striata, the, in the brain, ii. 90—their functions, 137.
Corpus callosum, the, in the brain, ii. 89—held to be the seat of the sensorium, 41.
Cow, the, its flesh, i. 153—transmission of colour in, ii. 380.
Cow's milk, caseine in, i. 125—fat in, 135.
Crime, hunger a stimulus to, i. 2.
Cruciferæ, the, rejected by the horse, i. 62.
Crura cerebri, the, ii. 138.
Crustacea, respiratory organs in the, i. 362—the blind, the eye-pigment wanting in, ii. 334.
Crystalline humour of the eye, the, ii. 327.
Curd of milk, the, i. 125.
Cuticle or scarf skin, the, i. 35.
Cutis or true skin, the, i. 35.
Cuvier on the alleged discovery of Servetus, i. 301—on the conclusions of Flourens regarding the cerebrum, ii. 112.
——— F., on transmitted qualities, ii. 409.

DALTON, J. analysis of vitiated air by, i. 354—the colour-blindness of, ii. 341.
——— Dr, experiments, &c., of, on the effects of the removal of the Cerebrum, ii. 108—his conclusions regarding the latter, 109 *et seq.*—on the effects of injury to it in man, 111—observations on a fowl, *ib.*
Daltonism, what, ii 341.
Damiens, the regicide, anecdote of, ii. 352.
Danney, M., experiments on hereditary transmission in rabbits, ii. 387.
Dareste, M., on the white and gray matters of the brain, ii. 98.
Darkness, effects of, on respiration, i. 371.
Daumas, General, on the influence of each parent in the Arab horse, ii. 391.
Davy, Dr John, on non-coagulable blood, i. 259.
Déal, a Frenchman, singular suicide of, i. 347.
Death, slow or sudden, different effect of, on the heart, i. 335—no analogy between Sleep and, ii. 348—appearance of, contrasted with life, 412—the necessary issue of life, 442—is determined by the changes of life, 443—what it is, 444—that of the parts, 445—of the organism, *ib.*—apparent, 447—the signs of it, 449 *et seq.*
De Candolle, views of, on the influence of each parent on plants, ii. 391.
Democritus, his theory of vision, ii. 327.
Denis, transfusion of blood by, i. 275.
Derma, the, i. 35.

Descartes, reception of Harvey's doctrine by, i. 310—seat assigned to the sensorium by, ii. 41.

Development or Differentiation, the law of, as one of the laws of life, ii. 429.

Dextrine, ready conversion of starch into, i. 197, 227.

Diastole of the heart, what, i. 329.

Dietaries, table of various, i. 187, 188.

Dietrich on pulsation after death, i. 336.

Differentiation, the law of, ii. 429.

Digestibility essential to estimate of food, i. 115—not the sole test of nutritiveness, 152.

Digestion, influence of vegetable acids on, i. 148—importance of, to health and comfort, 190—complexity of the process, 191—its function, 193—process in the mouth, 194—in the stomach, 201—in the intestines, 219—the tract of, 230—causes of bad, 282—arrested in the stomach by bile, 228—influence of the nervous system on, 233—effects of, on respiration, 371—is intermittent, ii. 433.

Digestive fluids, the, proportion of water in, i. 37.

——— tract, view of the, i. 230.

Discs, the, in the blood, i. 243.

Disease, death from, 445, 446.

Distoma hæmatobium, the, i. 252.

Dog, the, diet of, i. 62—hybrids between, and the wolf and fox, ii. 394, 399, 400 *et seq.*

Dogs, experiments on feeding, on gluten, &c., i. 82—transmission of peculiarities in, ii. 382, 384.

Donders, researches of, on the action of saliva on starch, i. 198.

Donkey, the flesh of, as an article of diet, i. 165.

Donné, M., experiments of, on gelatine, i. 131.

Dormouse, loss of weight by, during its winter sleep, i. 18.

Dowler, Dr B., on phenomena after death from yellow fever, i. 327—on animal heat after death, 444.

Draper, Professor, on the colourless corpuscles of the blood, i. 252—on the circulation in the liver, 323—his hypothesis of the action of the capillaries, 339 *et seq.*—his theory of vision, 333 *et seq.*

Dreams, the current views of, and their defects, ii. 365—definition of 366—action of the brain in, 367—influence of subjective sensations in, 368—theory of, 369 *et seq.*—their coherence and incoherence, 370—we are surprised but not arrested by the latter, 371—influence of the systemic sensations in, 372—alleged unconsciousness of, 373.

Drowning, death by, i. 348, 388—its cause, 349.

Drum of the ear, the, *see* Tympanum.

Drunkenness, how it causes double vision, ii. 340.

Dublin hospital, effects of ventilation illustrated in, i. 373.

Dubois, Reymond, on nerve force, ii. 18.

Duck, time in which digested, i. 159.

Dugés, definition of life by, ii. 425.

Düiker, the, its power of subsisting without water, i. 48.

Duodenum, the, i. 219, 220—its position, &c., 230, 231, 320.

Dutrochet, the thermo-electrometer of, i. 411.

Dyspepsia, *see* Indigestion.

EAR, the, as the organ of hearing, ii. 317—description and sections of it, 318 *et seq.*

Ears, power of moving the, sometimes possessed, ii. 222.

Eating, effects of, on respiration, i. 399.

Ecker on the hydra, i. 251 *note*—on double vision in the drunken man, ii. 340.

Eckhard on the nervous system of the heart, i. 333.

Edwards, W. F., his researches on respiration, i. 352.

——— observations of, on animal heat, i. 426, 428, 429, 430, 451—experiments of, on frogs, &c., 453.

——— and Balzac, the memoir of, on gelatine, i. 131.

Egg, the, inexplicable by chemistry, i. 65—white of, its nutritive value, 122—sugar in, 140.

Eggs, cases in which pernicious, i. 60—yolk of, oil in, 134—as an article of diet, 169.

Ehrenberg, analysis of edible clay by, i. 54.

Eland, the, its power of subsisting without water, i. 47—its flesh, 166.

Electric action, relations of, to the sensibility concerned in muscular adjustments, ii. 287.

Electrical stimuli, sensation of taste excited by, ii. 302, 303.

Electricity, hypothetic identity of nerve force with, ii. 8.

Emotion, relations of, to sensation, ii. 117, 118—how it affects the action of the heart, 148.

Emotional excitement, effects of, on respiration, i. 399.

Encephalon, the, or Brain, *see* Brain.

Endosmometer, the, i. 106.

Endosmose, the law of, and its promotion within the body by salt, i. 105, 106—action of, in the circulation, 314.

England, increased attention to ventilation in, i. 373.
Ephemerom, the blood discs in the larva of, i. 245 *note*.
Epidermis, the, i. 35—effect of its destruction on tactile sensibility, ii. 298.
Epigenesis, Wolff's doctrine of, i. 286.
Epithelium, the, what, i. 218 *note*.
Esquimaux, the, unprolific, i. 168—food consumed by, 185.
Essential oils, the, as condiments, i. 137.
Etherisation, sensation during, ii. 64.
Euphorbia, the, the food of the black, and deadly to the white rhinoceros, i. 62.
Ewe's milk, caseine in, i. 125—fat in, 135.
Excito-motor actions, the so-called, ii. 165, 193.
Excito-motory fibres, the supposed, now rejected, ii. 173.
Exercise, value of, to the dyspeptic, i. 233—influence of, in counteracting cold, 439.
Existence, the sensations which constitute the consciousness of, ii. 66.
Exosmosis, what, i. 106.
Experiment, true value of, as regards the study of life, &c., ii. 87.
External medium and conditions, influence of, on life, ii. 434.
Extra-tropical vegetables, the, i. 171.
Eye, the, as the organ of sight, ii. 325—description of it, 326—structure, &c., of the retina, 330—the black pigment, 332—the blind spot in the, *ib.*—duration of impressions on it, 338—single vision, 339.
Eyes, effects of starvation on the, i. 21.

Face, comparative sensitiveness of the, ii. 290—cadaverous aspect of, as a sign of death, 449.
Families, diversities of character, &c., in, ii. 379.
Family, the, not the type of the state, i. 66.
Fasting, amount of waste caused by, fatal, i. 9—prolonged, examination of cases of, 10, 13 *et seq.*—physiological arguments against these, 16 *et seq.*—supposed accumulation of the gastric juice during, 29—effects of, on the stomach, 32—state of the stomach during, 202—effect of, on the quantity of the blood, 272—influence of, on animal temperature, 434.
Fat, loss of, during starvation, i. 21—proportions of, in different tissues, 76—the consumption of, in cold countries, 89—influence of, on the digestion of albuminous substances, 211—results of stomachic action on, 215.
Fats, how classed by Liebig, i. 71—and oils, as alimentary principles, 134—different digestibilities of, 135—action of the gastric juice on, 206—intestinal action on, 219—alleged influence of, on the bile, 220—do they cause biliousness? 221—action of the bile on them, 225—and of the pancreatic juice, 226—proportion of, in blood, 262, 265.
Father, on the relative influence of, upon the offspring, ii. 390 *et seq.*
Fatigue, how it induces sleep, ii. 363—how excessive prevents it, 364.
Fauces, the, held by Müller to be the organ of taste, ii. 301.
Feeling, the link of, and its connection with voluntary action, ii. 210 *et seq.*—ground on which assumed with regard to animals, ii. 237. *See also* Sensation.
Female, the, the pulse in, i. 330—temperature of, 431.
Fenestra ovalis, the, in the ear, ii. 321.
Ferments, tendency of the albuminous substances to act as, i. 128.
Fibrine, how classed by Liebig, i. 71—incapable of nourishing by itself, 81, 82—proportion of, in various substances, 123—as an alimentary principle, 124—in vegetables, *ib.*—anomalies regarding, 127—conversion of starch into sugar by, 198—of the blood, 255, 256—different from muscle fibrine, 257—proportion of, in blood, 262.
Figs, sugar in, i. 139.
Filberts, oil in, i. 134.
Finger, comparative tactile sensibility of the, ii. 297.
Fish, dried, use of, among the Hindoos, i. 85—as an article of diet, digestibility, &c., of, 166—Leeuwenhoek on, 167.
Fishes, power of abstinence of, i. 12—the blood discs in, 244, 245, 246—no valves in the veins of, 307—circulation in, 324—diagram of it, 325—respiratory organs in, 362—temperature of, 408—the cerebral development of, ii. 91—relative weight of brain and spinal cord in, 97.
Fish-eating tribes, said to be prolific, i. 168.
Fixed ideas, relations of, to sensation, ii. 58.
Flesh as a food, how classed by Liebig, i. 71—muscular qualities of, 80—use of, as an article of food, 152 *et seq.*—changes in, by lixiviation, 153—action of cooking on, 154 *et seq.*—intestinal action on, 219.
Flourens, M., his history of the dis-

covery of the circulation of the blood, i. 296—the experiments and conclusions of, regarding the cerebrum, ii. 104 *et seq.*—and regarding the cerebellum, 123 *et seq.*, 126—conclusions from Combette's case against his view, 131—the vital point of, 145—on longevity, 438.

Fluids, transfusion of, by endosmose, i. 106.

Fœtal epoch of life, the, ii. 437.

Fœtus, the voluntary motions of the, ii. 206.

Follicles, what, i. 29 *note*.

Fontana, voluntary contraction of the iris by, ii. 221.

Food, the want of, the cause of hunger, i. 5—frequency of demand for, in different genera, 6—insufficiency of, its effects, 20—effects of injection of, into veins, &c., 30—water the carrier of, in the body, 37—varieties of, as to form, and identity as to aliment, 51—assimilation of all within the organism, 52—how to investigate, 56—the chemical method and its defects, 57 *et seq.*—what that method has done, 57—different kinds of, required by different organisms, 61—true relations of chemistry to the science of, 63—Liebig's classification of, examined, 70 *et seq.*—inorganic, 95—its importance, 96—effect of different conditions with regard to, 109—neglect of this by the chemist, 110—effect of the interference of life, 111—insufficiency of the chemical theory of, 112, 113—digestibility essential to, 115—blood as a standard of, *ib.*—milk as a standard, 118—four classes of principles necessary in, 120—the various alimentary principles, 122 *et seq.*—articles of, 151 *et seq.*—nutritive value of, regulated by relation to organism, 179—on the quantity required, 180 *et seq.*—action of the mouth on, 194—of the stomach, 202—results of stomachic action on, 215—intestinal digestion of, 219 *et seq.*—ill-selected, as a cause of indigestion, 233—relations of, to animal temperature, 431 *et seq.*

Foot, comparative sensitiveness of the, ii. 290.

Forehead, tactile sensibility of the, ii. 297.

Form, importance of, in physiology, i. 112.

Foville, views of, as to the function of the cerebellum, ii. 134.

Fowls, time in which digested, i. 159—effects of removal of the cerebrum on, ii. 105, 107, 111.

France, baking of meat in, i. 156—consumption of horse-flesh in, 164.

Franklin on salt-water bathing for allaying thirst, i. 34—his experiments on relation of colour to temperature, and theory of vision illustrated from these, ii. 383.

Frederick the Great, small amount of sleep taken by, ii. 357.

French workmen, effects of an animal diet on, i. 175.

Friction, assertion that life resists, ii. 435.

Frog, vitality of the, i. 279—results of removal of its head, in connection with the reflex theory, ii. 175 *et seq.*—voluntary movement in, after removal of the brain, 184—after being skinned also, 185—experiments on, with reference to the muscular sense, 282 *et seq.*—and shewing the relations of electric action to it, 287.

Frogs, respiratory organs in, i. 362—the winter life of, 452—relative weight of brain and spinal cord in, ii. 97—effects of removal of cerebellum on, 126—relations of the latter to the vertebral column in, 127—decapitated, experiments as to sensation in, 239, 245 *et seq.*—suspended animation in, 413.

Frying, effects of, on fish, i. 166.

Function, distinction between, and property, ii. 20 *et seq.*—how determined, in nerves, 82—illustration of its relations to property, 93.

Functional and mechanical activity, Bernard on, i. 269.

GALEN, views of, on respiration, and discovery regarding the arteries, i. 298.

Gall, views of, regarding the cerebellum, ii. 122—on the experiments of Sue, 236.

Gall-bladder and duct, position of, i. 230, 320.

Game, the flesh of, i. 158.

Gaming, hereditary transmission of tendency to, ii 385.

Ganglia, disposition of the nerve fibres in, i. 333 *note*—as part of the nervous system, ii. 9—sensibility their property, 18, 24—identity of structure of, 43—position of gray and white matter in, *ib.*—their identity of organic relation with the brain, 45—their essential identity with it, 94, 95—proof of sensibility being the property of all, 179.

Ganglion, description, &c., of a, ii. 10.

Ganglionic system of the heart, the, i. 332 *et seq.*

——— tissue, sensibility the property of, ii. 40—analogy between, and muscular, 73.

Garden beetle, nervous system of the, ii. 95, 96.

Garnett, Mr, on hybrids from the Muscovy duck, ii. 395.
Gas, effects of, on the air, i. 387.
Gases, how they affect the sense of taste, ii. 302—odorous, Graham on, 314.
Gasserian ganglion, the, ii. 307.
Gastric follicles or glands, the, i. 205.
——— juice, the, supposed attacking of the stomach by, i. 29—supposed accumulation of, during fasting, *ib.* —Dr Beaumont's work on, 201——first obtained, 204—its composition, action, &c., 205—nature of it, &c., 210—quantity secreted, 218—estimates of its solvent power, 214—its action on the stomach, 215—its action arrested by bile, 223—insufficiency of, as a cause of indigestion, 232—attacks the dead but not the living stomach, and the supposed proof from this as to life controlling chemical affinities, ii. 417.
Gaucho, animal diet of the, i. 121.
Gavarret on the animal heat of man, i. 416.
Gelatine incapable of supporting life by itself, i. 81, 82—as an alimentary principle, 129—action of the stomach on, 207.
——— commission, the, and its report, i. 131—extract from its report, 80.
Gelatinous tissues, supposed formation of the, from gelatine, i. 133.
Gemsbuck, the, its power of subsisting without water, i. 48.
Genius, where hereditary, ii. 401.
Gerdy on the use of the terms sensation, &c., ii. 61—on the classification, &c., of the senses, 274.
Gerlach, his mode of making microscopic preparations, ii. 272.
Germany, baking of meat in, i. 155—use of horse-flesh in, 162—indifference to ventilation in, 373.
Giacomini, denial of the blood discs by, i. 346 *note.*
Gills, vessels, &c., of the, in the fish, i. 324—breathing by, 356, 362.
Giron on hereditary transmission of colours, ii. 380—and of other qualities, 384—on hybrids between wolf and dog, 400—on the potency of race as shewn in sheep, 407.
Glands, Bernard on the functional and mechanical activity of, i. 269.
Gluten, capable of supporting life, i. 82—as an alimentary principle, 127—conversion of starch into sugar by, 198.
Gluttony, examples of, i. 183.
Glycose, conversion of dextrine into, i. 197—starch converted into, 219, 227.
Goat, the, plants rejected by, i. 69—its flesh, 153.
Goat's milk, caseine in, i. 125—fat in, 135.
Goldsmith, account of the agonies of starvation from, i. 23.
Goodwyn, views of, on asphyxia, i. 352.
Goose, time in which digested, i. 158.
Gooseberries, sometimes pernicious, i. 60—sugar in, 139.
Graham, Mr, on odorous substances, and the nature of the action, ii. 314.
Grainger, Mr, the views of, on the reflex theory, ii. 172—his assertion that there are reflex fibres, 173—his experiments, &c., on division of spinal cord, 189, 253—arguments of, against its sensational power, 228—denies the sensibility of the medulla oblongata, as well as the spinal cord, 234.
Grandfather or grandmother, transmission of qualities from, ii. 406.
Granié, death of, by voluntary starvation, i. 19.
Great men, the children of, ii. 401.
Greengages, sugar in, i. 139.
Greenland, no cereals grown in, i. 172.
Gray matter of the nervous system, &c., the, ii. 13—distribution and supposed function of, in the brain, 90.
Grünewaldt, Dr, his observations on the gastric juice, i. 202—on the quality of the gastric juice, 214.
Guiana, clay-eating in, i 54.
Guildhall feast, a, contrasted with the meal of an Irish peasant, i. 51.
Gullet, the, its action on the food, i. 202—its position, &c., 230, 231, 320.
Gums, how classed by Liebig, i. 71.
Gums, the, shewn not to be the organ of taste, ii. 301.

Habits, relations of, to sensation, ii. 68—hereditary transmission of, 383.
Hair, excretory function of, i. 286, 287.
Hall, Marshall, his theory of reflex action, and examination of it, ii. 165 *et seq.*—his error, 172—observations of, on a headless turtle, 180—refutation of his conclusions from these, 181—general character of his investigations, 183—his observations on division of the spinal cord, and criticism on them, 186 *et seq.*—on the phenomena following division of spinal cord, 256—case of diseased spine recorded by, and examination of his inferences from it, 261.
Haller, cases of alleged fasting from, i. 13—cases of blood-letting from, 274—error of, regarding the motions of

the heart, 329 *note*—on the cause of these, 331.
Hallucinations, how produced, ii. 367, 368.
Hamilton, Sir W., on sensation, ii. 52—on the number of the senses, 274—on the muscular sense, 280—on the tactile sensibility of various parts of the body, 297—on dreaming and dreams, 373.
Hand, the, comparative sensitiveness of, ii. 290—as the organ of touch, 291, 292—tactile sensibilities of back and front, 297—difference between right and left as regards temperature, 298.
Hardening infants, theory of, and its errors, i. 429 *et seq.*
Harless, Professor, on the effects of thirst on the nerves, i. 49 *note*—case of pulsation after death from, 336.
Harley, Dr, on the quantity of the gastric juice, i. 213 *note*—on the protection of the stomach from the gastric juice, 218 *note*.
Harvey, views of, on the vitality of the blood, i. 254—what was his discovery, 291, 292—the general circulation discovered by, 302—the publication of his work, 306—his originality, 307—reception of his doctrine, 309—gaps in it, 310—views of, on the lymphatics, 318—his claims, 321.
Head, life of the, after decapitation, ii. 446.
Health, importance of digestion to, i. 190—importance of a knowledge of physiology to, ii. 452.
Hearing, the medulla the centre of the nerves of, ii. 139—the sense of, 317—the apparatus of, 318 *et seq.*—integrity of the tympanum not essential to it, 319—sketch of the process of it, 321—intensity, pitch, and quality of sound, 322—the highest and lowest audible sounds, *ib.*—the musical and unmusical ear, *ib.*—the effect of music, 323—effect of tones, *ib.*—subjective sounds, 324—appreciation of direction, *ib.*—its acuteness in the blind, 325.
Heart, the, tissues of which make up, i. 4—its position, 230. 294, 320—error of the ancients regarding, 297, 298—its valves, 309—is it the cause of the circulation? 322—circulation without one, 323—and after removal of it, 325—its motions, 329—what causes these, 331—its movements after death, *ib. et seq.*—its nervous system, &c., 332 *et seq.*—cessation of its pulsations no sign of death, 335—its motion an example of reflex action, ii. 57—arrest of its action by irritation of various nerves, 147—influence of sudden emotions on, 148—cases of power over the beating of it, 222—active during sleep, 348.
Heat, effects of, on fat, i. 137—and on the gastric juice, 209—tendency of, to induce sleep, ii. 354—
—— animal, phenomena of, i. 405—instruments for measuring, 409—relations of perspiration to, 418—its constancy in man, 422—its difference at different ages, 425 *et seq.*—relations of sex to, 431—and of food, *ib. et seq.*—influence of the seasons, 435—the chemical theory of, examined, 440 *et seq.*—effect of muscular contraction on, 442—cases of it after death, *ib.*—influence of organic conditions on, 450 *et seq.*
Heat-making substances, Liebig's class of, i. 71.
Helvetius, argument of, against vegetarianism, i. 173.
Henle, views of, on the capillaries, i. 314—on the excitation of taste by mechanical stimuli, ii. 302.
Hepatic veins, the, destitute of valves, i. 307.
Herbivora, the, their power of resisting starvation, i. 11—peculiarities of their feeding, *ib.*—differences in the food of, 62—the chief food of the carnivora, 115 *note*.
Herbst on respiration, i. 367—on the effects of dress on it, 368.
Hereditary qualities, *see* Parents.
Hewson, the researches of, on the blood, i. 245, 247.
Hindoos, the, their diet a non-nitrogenous one, i. 84.
Hinny, the, alleged influence of each parent in, ii. 392, 393, 399.
Histology, what, i. 4.
Holland, Sir H., on the relations of attention to sensation, ii. 51—his view of sleep, 349, 350—on congenital blindness, 386.
Holwell, Mr, his narrative of the Black Hole of Calcutta, i. 38 *et seq.*
Horne, estimate of weight of food by, i. 186.
Horse, the, plants rejected by, i. 62.
Horses, taming of, by thirst, i. 38.
Horse-flesh, on, as an article of diet, i. 160 *et seq.*
Hottentots, gluttony of the, i. 183.
Hufeland, account of suicide by starvation by, i. 25—error of, regarding longevity, ii. 439.
Human blood, chemical analysis of, i. 262.
—— milk, caseine in, i. 125—fat in, 135.
Humboldt, account of the clay-eating Otomacs by, i. 54, 55.
Hunger, economic and social influences of, i. 1, 2—as a stimulus to

activity, 2—and to crime, *ib.*—what is it, 3 - the cause of it, 4—want of food the primary cause, 5—why it recurs periodically, 6—its effects, 8 *et seq.*—amount of waste fatal, 9—the sensation of, 27—what causes the sensation, 28 *et seq.*—the seat of it, 31—the systemic sensation, 32—kills by a putrid fever, 49.
Hunter, John, experiments by, on hybernating animals and their food, i. 6—views of, on the vitality of the blood, 254—small amount of sleep taken by, ii. 357.
Hutcheson on the classification, &c., of the senses, ii. 274.
Huzard, testimony of, in favour of horse-flesh, i. 161.
Hyaloid membrane of the eye, the, ii. 327.
Hybernating animals, loss of substance by, during their winter sleep, i. 18—dependence of, on external temperature, 413—influence of season on, 437.
Hybernation, food not required during, i. 6—respiration during, 397—waste of the tissues during, ii. 361—the sleep of, 364.
Hybrids, peculiarities of various, ii. 395.
Hydrogen, effect of light on its affinity with chlorine, i. 111—proportion of, in the blood, 262—why inodorous, ii. 315.
Hydrophobia, Laycock on the phenomena of, in connection with reflex action, ii. 192 *et seq.*

ICE, influence of, in allaying thirst, i. 46, 110.
Ices, baking of, in China, i. 420.
Iceland, no cereals grown in, i. 172.
Ideas, influence of guiding, with regard to voluntary actions, ii. 215 *et seq.*
Ideation, relations of, in the nervous system, ii. 118.
Ideo-motor actions, the, ii. 194.
Idiosyncrasies, hereditary transmission of, ii. 332.
Ileum, the, i. 219.
Iliac veins, the, destitute of valves, i. 307.
Images, visual, not transmitted as such to the brain, ii. 328 *et seq.*—are they formed on the retina? 329.
Impressions, current views regarding, and refutation of these, ii. 69 *et seq.*
Incus or anvil, the, in the ear, ii. 321.
Indian Fakeers, simulated death among the, ii. 448.
Indigestion, misery attendant on, i. 190—causes of, 232—influence of, in dreams, ii. 372.
Individual potency, influence of, in hereditary transmission, ii. 406.
Infancy, the animal heat during, i. 425—necessity of warmth to, 429—great amount of sleep during, and its alleged cause, ii. 358—the life-epoch of, 437.
Infant, the, peculiarities regarding feeling in, ii. 210, 211—cannot localise its sensations, 294.
Inherited qualities, *see* Parents.
Inorganic existence, how distinguished from organic, ii. 429.
——— food, what, i. 95—its importance, 96.
——— phenomena, mystery and grandeur of, ii. 423.
——— principles, necessity of, in food, i. 120.
——— substances, alimentary powers of, i. 72—are animals capable of assimilating? 98—indispensable to animal life, 99.
Inosite, composition, &c., of, i. 139.
Inquiry, the right spirit of, ii. 83.
Insanity, congenital tendency to, ii. 339.
Insects, feeding of, on sugar, i. 140—animal heat of, 413—the nervous system of, ii. 45 *et seq.*—sensation, &c., in headless, 177—experiments on, 179 *et seq.*
Insect life, undue prolongations of, ii. 442.
Insensible perspiration, amount of, i. 35.
Instinct, uncertainty attending the use of the term, and error of contrasting it with intelligence, ii. 91.
Instincts, alleged dependence of, on the cerebrum, ii. 105—proof to the contrary, 108—origin of, in systemic sensations, 117.
Intellect, relations of, to sensation, ii. 117—relations of the cerebellum to, 136.
Intellectual activity, effects of, on respiration, i. 398, 399.
Intelligence, the attempt to separate it and instinct, ii. 91—connection of, with cerebral development, *ib.*—supposed dependence of, on the gray matter of the brain, 98—importance of the cerebrum to it, 100 *et seq.*—Dalton's views of the relations of the cerebrum to it, 109—relations of size of brain to, 120.
Intensity of sound, what, ii. 322.
Intermittence, Bichât's law of, in its supposed application to the animal life, ii. 432.
Intestinal canal, length and subdivisions of the, i. 219.
——— glands, the, i. 228.
——— juice, action of the, in digestion, i. 228.

Intestines, the, injection of food into, i. 30—and of water, 34—digestion in, 219 *et seq.*—their movements an example of reflex action, ii. 57.

Invertebrata, no valves in the veins of, i. 307—the attempt to deny intelligence to, ii. 91 *et seq.*—peculiarity regarding the eye in, 333.

Involuntary action, difficulties of distinguishing between, and voluntary, ii. 197—capability of controlling, 221—cases illustrating this, *ib. et seq.* *See* also Voluntary.

——— nervous phenomena, the so-called, ii. 165.

Iris of the eye, the, ii. 326—cases of power of voluntary contraction of, 221.

Iris persica, how its odour affects different persons, ii. 315.

Irish, the, their diet a non-nitrogenous one, i. 84.

——— peasant, food of the, contrasted with that of the London alderman, i. 51.

Iron, as an alimentary principle, i. 147—in the blood, 242.

Itching, the sensation of, due to the nerve-filaments of the skin, ii. 289.

JACOB'S membrane in the eye, ii. 330.

Jacubovitch, researches of, on the action of saliva on starch, i. 198—views of, on the nerve-cells of the spinal cord, ii. 157.

Jamaica, clay-eating in, i. 54.

Java, clay-eating in, i. 54.

Jejunum, the, i. 219.

Jellies, nutritive powers of, i. 129.

Jew, the, permanency of the peculiarities of, ii. 377, 378.

Jockeys, use of fish by, i. 168.

Johnston on odours, ii. 314 *note*.

Jones, Dr Bence, on the importance of salt to the organism, i. 107.

——— Wharton, on the blood discs, i. 249 *note*.

Jouffroy on sounds and their influences, ii. 60.

Jugular vein, injection of sugar into the, i. 117.

KAMTSCHATKA, clay-eating in, i. 54.

Kant, definition of life by, ii. 424.

Kid, flesh of the, i. 153.

Kidneys, the, water removed by, i. 36—as an article of diet, 159—peculiarity of the venous blood in, 269—in the fish, 324.

King, transfusion of blood by, i. 275.

Kirby and Spence, experiment on prolongation of insect life from, ii. 412.

Kitto, Dr, on the muscular adjustments necessary to speech, ii. 286.

Klaproth on the effects of race on hereditary transmission, ii. 407.

Kölliker on the blood discs, i. 247—experiments by, on the action of the Woorara poison on the nerves, ii. 79—on the spinal cord, 154 *note*, 156—his mode of making microscopic preparations, 270, 271.

Königsbaden, use of horse-flesh at, i. 162.

LABIATÆ, the, rejected by the ox, i. 62.

Labillardière, account of clay-eating by, i. 54.

Labyrinth of the ear, the, ii. 319, 321.

Lacteals, the, i. 315—their position, &c., 230, 231, 320.

Lallemand, views of, on the connection of the cerebrum and spinal cord, ii. 119—and on the spinal cord, 227.

Lamb, difference between, and mutton, i. 153—time in which digested, 157.

Lamprey, proportion of the brain to the spinal cord in the, ii. 266.

Lancisi, seat assigned the sensorium by, ii. 41.

Landry, M., views of, on voluntary and involuntary actions, ii. 224 *note*—views of, regarding the spinal cord, 231 *note*, 240 *note*.

Large intestines, what, i. 219.

Larrey, testimony of, in favour of horse-flesh, i. 161.

Laughing, sometimes voluntary, sometimes involuntary, ii. 199.

Lavigne, M., case of prolonged fasting recorded by, i. 14.

Lavousier, his theory of respiration, i. 352.

Lawes and Gilbert, Messrs, on Liebig's classification of food, i. 73—experiments by, on feeding sheep on turnips, 83—on effects of temperature as to feeding of animals, 88—their experiments on feeding of animals, and results, 94—on the necessity of inorganic substances, 100.

Laycock, Dr, views of, on the brain as a centre of reflex action, ii. 191—on the phenomena of hydrophobia in connection with it, 192 *et seq.*

Leblanc, analysis of vitiated air by, i. 355.

Leech, effect of bite of, on the blood, i. 259.

Leeuwenhoek on fish as an article of diet, i. 167—on the selection of diet, 169—on the blood discs, 244, 247—discovery of the capillaries by, 312.

"Leg asleep," sensation of, how caused, ii. 300.

Legallois, views of, on respiration, i. 352—views of, on the spinal cord, ii. 226.

Lehmann on the production of fibrine, i. 127—on sugar in the egg, 140—on

fish as an article of diet, 168—experiments of, on the action of coffee, 176—on the effect of water on the gastric juice, 210—on the solvent power of the gastric juice, 214—analysis of blood by, 263—estimate of quantity of the blood by, 272—experiment by, regarding the nervous character of the retina, ii. 331.
Lejoncourt, M., on congenital longevity, ii. 338.
Lens of the eye, the, ii. 327.
Lentils, nitrogen in, and their nutritive value, i. 82.
Leuchs, discovery of the chemical action of the saliva by, i. 196.
Levy on the non-nutritious properties of albumen, &c., i. 81.
Lieberkühn's glands, i. 228.
Liebig, examination of his classification of food, i. 70 *et seq.*—on the uses of water and fat in the organism, 77—error of his view, 78—on the nutritive value of beans, wheat, &c., 82—on that of rice, 84—on the dietary of cold and warm climates, 87—on the combustible elements of the albuminous substance, &c., 90 *et seq.*—on the indispensability of inorganic substance, 101—denial of the existence of salt in muscle by, 108—on caseine, 125—nutritive power denied to sugar by, 140—on the action of alcohol, 145—on the necessity of iron to the body, 148 *note*—on the cooking of meat, 153 *et seq.*, 155—on the quantity of food required, 182—on the relations of respiration and animal heat, 441.
Life, inadequacy of chemistry to explain the problems of, i. 65—effects of the interference of, with regard to food, 111—processes of, in their relation to animal heat, 407—consciousness always the property of, ii. 75—mind the psychical aspect of, 344—no antagonism between sleep and, 348—duration of, influence of hereditary tendency on, 387—contrast between appearance of, and death, 412—suspended animation, 413—the hypothesis of a vital principle examined, 414 *et seq.*—the so-called vital force or forces, 415—chemical analysis adverse to the doctrine of the latter, 416—its phenomena dependent on conditions, *ib. et seq.*—it does not control chemical affinities, 417—does it precede organisation? 419—as dependent on vital unity, 421—to be regarded as an ultimate fact, 422—identical with existence, 423—definition of it, 424 *et seq.*—that of cells, 426 *et seq.*—the entire life the aggregate of the cell-life, 428—its three laws, 429—these distinguish the living organism from the inorganic body, *ib. et seq.*—vegetal and animal, 430—influence of the external medium on it, 434—error in supposing it independent of this, 435—its duration, 436—its four epochs, 437—why does it cease, 438—the excess of waste the cause of its decline, 441—death a necessary issue, 442—determined by its changes, 443—death as its terminal act, 444.
Light, effect of, on chemical affinity, i. 111—analogy between, and sensation, ii. 66—sensibility to, distinct from seeing, 325.
Limbs, rigidity of the, as a sign of death, ii. 449.
Linnæus, views of, on the influence of each parent in plants, ii. 390.
Linseed, oil in, i. 134.
Lips, tactile sensibility of the, ii. 297.
Liquor sanguinis, the, i. 255—analysis of it, 264.
Liver, the, effects of starvation on, i. 21—formation of sugar by, 139—as an article of diet, 159—secretion of bile by, 220—its position, &c., 230, 231, 320—the capillaries on it, 242—circulation in it, 323.
Livingstone, Dr, on the Eland, i. 47—on the animal heat of the European and the negro, 416—case of animal heat after death from, 444.
Lixiviation, effects of, on flesh, i. 153.
Lizards, effects of removal of cerebellum on, ii. 126.
Localising sensations, development of the power of, ii. 293 *et seq.*, 298.
London alderman, food of the, contrasted with that of the Irish peasant, i. 51.
Londonderry steamer, tragedy on board the, i. 350.
Longet on death from hunger and thirst, i. 49—experiments of, on the effects of removal of the cerebrum, ii. 108—views of, on the cerebellum, 122—experiments on it, 124—on the effects of disease in the cerebellum, 129—views of, on the spinal cord, 227—on the so-called olfactory nerves, 310.
Longevity, hereditary tendency to, ii. 387 *et seq.*—current fallacy regarding, 438—the alleged cases of, 443.
Lower, transfusion of blood by, i. 275.
Lucas on congenital longevity, ii. 388.
Ludwig on perceived sensations, ii. 50 *note*—on the effects of disease in the cerebellum, 129.
Lungs, the, proportion of water in, i. 36—position of, 230, 294, 295, 320——breathing by, 356, 362—in man, 364 *et seq.*—their capillaries, 366.
Lymph, what, i. 315.

Lymphatics, position of, i. 230, 320—not known to Harvey, 292—their function, &c., 295—discovery of, 315.

MACARONI, carbonaceous character of, i. 87.
Maceration, process of, in digestion, i. 193—importance of, as regards the food, 200.
M'Leod, Janet, the case of, i. 15 *et seq.*
M'Neill, Sir J., case of spectral illusion recorded by, ii. 337.
Macnish on sleeping in church, &c., ii. 355.
Magnus, the researches of, on arterial and venous blood, i. 266.
Maize, albumen in, i. 124—gluten in, 128—oil in, 134—sugar in, 139.
Majendie on the non-nutritive qualities of fibrine, &c., i. 80—on the white of egg, 122—experiments by, on feeding rabbits, 179—the blood discs denied by, 245—views of, on the transfusion of blood, 276—on the so-called olfactory nerves, ii. 309.
Male, the, the pulse in, i. 330—temperature of, 431
Malleus or hammer, the, in the ear, ii. 321.
Malpighi, the blood discs discovered by, i. 244—researches of, on coagulation of the blood, 256—reception of Harvey's doctrine by, 310—discovers the capillaries, 312.
Mammals, respiratory organs in, i. 362—the cerebral development of, ii. 91—the cerebellum in, 128.
Man, time in which death ensues from fasting in, i. 12—variety of dietary of, 151—the blood discs in, 247, and *note*—respiration in, 364 *et seq.*—the animal temperature of, 415—its constancy, 422—his power of adjustment to season, 436, 437—the systemic consciousness in, ii. 76—average weight of the brain in, 97—effects of injury to the cerebrum in, 110—proportion of the brain to the spinal cord in, 265—action of the so-called muscular sense in, 280.
Manhood, the life-epoch of, ii. 437.
Maniacs, cause of the sleeplessness of, ii. 356.
Manx cat, progeny of the, ii. 395.
Marcet, M., on the action of the bile on fats, i. 226.
Mare, influence of, in horse-breeding, ii. 391 *et seq.*—curious case of a, in connection with hereditary transmission, 405.
Margo on pulsation of the heart after death, i. 336.
Marolle on hybrids between wolf and dog, ii. 400.
Marriage, the question of, in connection with hereditary taint, ii. 410.
Marsch on hybrids between wolf and dog, ii. 400.
Martins, M., observations of, on animal heat, i. 426—on the influence of sex in temperature, 431—and of food, 432, 434.
Martins, account of clay-eating by, i. 54.
Mateucci on the heat originated by muscular contraction, i. 442
Mayer on the blood discs, i. 247—seat assigned the sensorium by, ii. 41.
Mayo, Dr, on the effects of division of the spinal cord, ii. 252—on sight and muscular adjustment, 286.
Meals, proper interval between, i. 237.
Meat, fat in, i. 134—various kinds of, as articles of diet, 152 *et seq.*—action of cooking on, 154 *et seq.*—digestibility of various kinds, 157.
——— soup, comparison between, and bone soup, i. 132.
Mechanical process, the, in digestion, i. 193.
——— stimuli, sensation of taste excited by, ii. 302.
Mechanism, analogies between the animal organism and a, ii. 160 *et seq.*
Medulla Oblongata, position of the, ii. 88, 89—olivary bodies of, 90—weight of, in man, 97—functions of the, 137—its relations to the spinal cord, 138—its structure, &c., *ib.*—views on it, 140—a centre of sensation, 141—the centre of the nerves of respiration, 145—why its destruction causes death, *ib.*—the supposed vital point in, *ib.*—effect of irritation of it on the heart, 147—reason why its removal affects voluntary movement, 155—its essential identity of structure with the spinal cord, 233.
Medusa, shipwreck of the, the dreams of the sufferers, ii. 372.
Meissner, the tactile corpuscles of, ii. 292 *et seq.*
Memory, influence of pressure on the cerebrum on, ii. 103, 105, 107.
Men, respiration in, compared with women, i. 370.
Mental phenomena, grouping of, in relation to the nervous system, ii. 116 *et seq.*
Mesenteric ganglia, the, i. 230, 320.
Mialhe, researches of, on the action of the saliva on starch, i. 197.
Microscope, value of, in physiological investigations, ii. 173.
Microscopic preparations, best modes of, ii. 270.
Milk, on, as a standard of food, i. 118—analysis of it, 119—caseine in, 125—fat in various kinds of, 135.

Milne-Edwards on the vitality of the blood, i. 255.

Mind, the, the brain not exclusively its organ, ii. 4—what it includes, 5—the psychical aspect of life, 344.

Mineral substances, proportion of, in the body, and their importance, i. 99.

Mitchell, James, keenness of the sense of smell in, ii. 316.

Moisture, presence of, necessary to taste, ii. 305

Molecular death, what, and its constancy, ii. 445.

Moleschott on Mulder's discovery regarding albumen, i. 114—on the formation of albumen within the organism, 133—on the action of alcohol, 146—on the colourless corpuscles of the blood, 253.

Molière, reception of Harvey's doctrine by, i. 310.

Mollusca, absence of coagulation in blood of, i. 256—respiratory organs in, 362—nervous system of, compared with that of the insect, ii. 95—limited sensibility of, 277.

Monotony, tendency of, to induce sleep, ii. 355.

Moreau, M., on hereditary insanity, ii. 389.

Morgagni, cases of noncoagulable blood reported by, i. 258.

Mother, on the relative influence of, upon the offspring, ii. 390 *et seq.*—the assertion that genius is derived from, 401.

Motion as a source of waste in the animal body, i. 19.

Motor function, proof of, in the posterior nerves, ii. 38.

——— nerves, are there such as distinguished from sensory, ii. 25—what, 26—proved to be also sensory, 35.

Mouse, proportion of the brain to the spinal cord in, ii. 266.

Mouth, the, what takes place to the food in, i. 194—its membrane held by Valentin to be the organ of taste, ii. 301.

Mulder, the discovery of, regarding albumen, i. 113—on the nutritive power of gelatine, 129—on the assimilation of gelatine, 133.

Mule, the, as influenced by the father and the mother, ii. 390, 392, 399.

Müller on sensation and attention, ii. 50—on the experiments, &c., of Flourens, 112—views of, on the medulla oblongata, 140—share of, in the reflex theory, 166—on reflex action in connection with sensation, 175—on voluntary motions, 206—on volition, 208—views of, as to the organ of taste, 301—on after-taste of sweet flag, 305—on excitation of taste through the blood, 306—on the effect of certain sounds, 324—on the restorative power of sleep, 361.

Mummy wheat, the germination of, ii. 413.

Muscle, proportion of fat in, i. 76—different views of the chemist and the anatomist on, 77—all its constituents essential to, *ib.*—salt present in, 104—the nerves which move, &c., ii. 34.

Muscles, the, effects of starvation on, i. 22—action of different poisons on, ii. 80—sensations derived through, 345—activity of, during sleep, 349.

Muscle-nerves, what, ii. 39—proof of sensory as well as motor action of, 85—action of the Woorara poison on, 79.

Muscovy duck, peculiarity in hybrids of the, ii. 395.

Muscular adjustment, relations of sight to, ii. 286.

——— contraction, heat originated by, i. 442.

——— co-ordination, is the cerebellum the seat of, ii. 124 *et seq.*

——— exertion, influence of, on perspiration, i. 419.

——— fibre, contractility the property of, ii. 24.

——— movements, relations of the cerebellum to, ii. 126.

——— sense, the, nerves connected with, ii. 35—sensations derived through, 67—when these become perceived, *ib.*—the so-called, 279, 280 *et seq.*—various views regarding it, 282—experiments illustrating it, *ib. et seq.*

——— system, analogy drawn between, and the nervous, ii. 195.

Musculine or syntonin, what, i. 124, 257.

Music, nature of the effect of, ii. 322 *et seq.*

Musical ear, the, how distinguished from the unmusical, ii. 322.

——— note, definition of a, ii. 322.

Musk, how it affects the sense of smell, ii. 314—offensive to some, 315.

Mutton, case in which poisonous, i. 60—difference between, and lamb, 153—time in which digested, 157, 158.

NAPOLEON, command over sleep possessed by, ii. 354.

Narcotics, physiological action of, i. 177—effects of, on respiration, 371.

Narwhal, temperature of the, i. 409.

Nasse on the blood discs, i. 249 *note*—views of, on the spinal cord, ii. 229.

Natural death, what, ii. 445, 446.

Nature, balance of, as regards carbonic acid and oxygen, i. 393.
Navy, dietary in the, i. 188.
Negro, the animal heat of the, i. 416—constancy of breed in the, ii. 378.
Negroes, clay-eating among the, i. 54.
Nerves, the, activity of change in, i. 279—as part of the nervous system, ii. 9—the comparison of them to telegraphic wires, 14—are they conductors, and of what, *ib. et seq.*—proof they are possessed of force apart from their centres, 16—distinction between the function and property of, 21—the distinction between sensory and motor, *ib.*, 25 *et seq.*—distribution of their roots on the spinal cord, 33—causes of their differences of action, *ib. et seq.*—pain the result of direct stimulus to, 34—effect of dividing the posterior, 35—the anterior in relation to muscular sensibility, 37—the special sensibilities of, *ib. et seq.*—the motor function of the posterior, 38—posterior and anterior, both at once sensory and motor, 39—more strictly distinguished into muscle and skin nerves, *ib.*—and nerve-centres, general statement of propositions regarding, 81, 82—their neurility excited by electric disturbance, 288.
Nerve-cells, what, ii. 9, 10—of the spinal cord, 156.
——— centres, property and functions of, how determined, ii. 82—microscopic preparations of, 270—sensations the activity of the, 275.
——— fibres, description, &c., of, ii. 9, 10, 11—neurility as the property of, 9, 24—sensibility stimulated in the nerve-centres by, 24.
——— filaments of the skin, their sensitiveness, ii. 289—varying degrees of this, and these not dependent on their number, 290.
——— force, nature of the, ii. 7, 18.
——— tissue, composition of, ii. 9—water in, i. 36—similarity of, in all animals, ii. 44.
Nervous centres, the influence of, on each other, ii. 146, 147.
——— diseases, congenital tendency to, ii. 389.
——— fluid, the, the old hypothesis of, ii. 8.
——— phenomena, Marshall Hall's classification of, ii. 165.
——— sensibility, analogy between, and muscular contractility, ii. 73.
——— substance, resistance of, to starvation, i. 21.
——— system, the, effects of thirst on, i. 49 *note*—influence of, on digestion, 233—study of it a physiological problem, ii. 3—it all implicated in sensation, &c., 4—its structure, 9—in different classes of animals, 44 *et seq.*—the theory of reflex action examined, 165 *et seq.*—analogy between, and the muscular, 195—general connection among its centres, 203.
Neurility, definition of, ii. 8, 18—the property of nerve-fibre, 24—the common property of all nerves, 81—proof of excitement of, by electric disturbance, 288.
New Caledonia, clay-eating in, i. 54.
New Zealanders, the, fish-eaters, i. 169.
Night, effects of, on respiration, i. 398.
Nitrogen, proportion of, not the measure of nutritive value, i. 82—feeble affinity of, for oxygen, 91—comparison of flesh and fish as regards, 169—proportion of, in the blood, 262—and in air, 353.
Nitrogenous foods, Liebig's, i. 71.
——— matter, small proportion of, in milk, i. 119.
——— substances, true relations of, to the body, i. 74—Liebig on them, 79—their proportion in the body, 80.
Noble, Dr, views of, on the cerebellum, ii. 186 *note*—on the optic thalami, &c., 187 *note*.
Nœud Vital of Flourens, the, ii. 145.
Non-nitrogenous bodies, disproof of their being merely heat making, i. 86—Liebig's, 71—true relations of, to the body, 74.
Nose, the, its tactile sensibility, ii. 297—as the medium of smell, ii. 306.
Nucleus, the, in the blood-discs, i. 247 *et seq.*—and nucleolus of the cell, ii. 427.
Nutrition in plants and animals, fallacy regarding, i. 97—oxygen the agent of, in the blood, 281 *et seq.*—relations of blood to, 284 *et seq.*—examination of the hypothesis that it goes on chiefly during sleep, ii. 358 *et seq.*—the law of, as one of the laws of life, 429.
Nutritive material, quantity of, carried by the blood, i. 241.
——— value, influence of the organism on, i. 179.

OATS, albumen in, i. 124—gluten in, 123.
Oatmeal, sugar in, i. 138.
Observation, distinction between, and scientific explanation, i. 192.
Ochsenhausen, sale of horse-flesh at, i. 162.
Ocular spectra, cases and theory of, ii. 335 *et seq.*
Odours, different estimates formed of, ii. 315. *See* Smell.
Œsophagus, the, its action on the food, i. 202—its position, &c., 230, 231, 320.

Oil, consumption of, in Sicily, &c., i. 87.

Oils and fats, necessity of, in food, i. 120—different digestibilities of, 135.

Oily fishes, the, i. 166.

——— seeds, albumen in, i. 124.

Old age, amount of sleep required during, ii. 358—the life-epoch of, ii. 438—peculiarities attending, 440 *et seq.*

Olfactorius, the, ii. 306—not a nerve, 307.

Olfactory ganglion, the, ii. 90.

——— nerves, the so-called, ii. 306—doubts as to their being so, 308 *et seq.*—case in which the sense of smell was possessed in their absence, 311 *et seq.*

Olivary body of medulla oblongata, the, ii. 90, 138, 139.

Olives, effects of, on flavour of wines, ii. 305.

Olive seeds, oil in, i. 134.

Oncidium, respiratory organs in the, i. 362.

Optic nerve, the, ii. 138—its special sensibility, 37—the effect of pressure or irritation on it, 325.

——— thalami, the, by some included in the brain, ii. 90—functions of, 137.

Orator, effect of the tones of the, ii. 323.

Organs, made up of tissues, i. 4—life of, after death, ii. 446.

Organic or vegatative life, Bichât's classification of, ii. 430.

——— principles, necessary in the soil to plants, i. 97.

——— sensations, what, ii. 279 *et. seq.*

——— sensibility, the so-called, ii. 72.

——— substances, chemical analysis of, adverse to the doctrine of a vital force, ii. 416.

——— unity, death the destruction of the, ii. 444.

Organisation, proof that it is inherited, ii. 377 *et seq.*—transmission of peculiarities of, 382—examination of the question as to whether life precedes it, 419—definition of it, and of the relations between it and life, 420.

Organism, the, constancy of waste and repair in, i. 4—necessity of the food being adapted to it, 61 *et seq.*—its influence on the food received, 63—inorganic substances indispensable to it, 95 *et seq.*—uses, &c., of water in it, 102—influence of relation to, on nutritive value of food, 179—the intervention of, as regards respiration, 379—its power of adjusting itself to the seasons, 436—regarded as a vital mechanism, ii. 160 *et seq.*—development of sensibility with its increasing complexity, 277—the repair of it a continuous process, 359, 360—its death, as distinguished from that of the parts, 445.

Orton, Mr, views of, on the relative influence of each parent, ii. 390, 392 *et seq.*

Osmazome, what, i. 152—want of, in fish, 168.

Otomacs, clay-eating among the, i. 54.

Owsjannikow, views of, on the spinal cord, ii. 232, note.

Ox, the, plants rejected by, i. 62—its flesh, 153.

Ox-blood, chemical composition of, i. 262.

Ox-head bones, fat in, i. 135.

Ox-liver, albumen in, i. 123—fat in, 134.

Oxidation within the body, Liebig's views of, i. 72—theory of respiration being a process of, 389—internal, the alleged cause of animal heat, 440.

Oxygen, proportion of, in the blood, i. 262—its proportion in venous and arterial blood, 266—its origin, 267—absorption of, by the blood-cells, 231—its function in the blood, *ib. et seq.*—proportion of in air, 353—the absorption of, by the blood the office of respiration, 356, 359—necessary to development, 360—effects of, on the separated tail of the tadpole, 361—absorption of, in respiration, 368—its state in the blood, 371—relative proportion of, necessary to respiration, 381, 382—how returned to the atmosphere, 393 *et seq.*—relations of, to odorous gases, ii. 314.

Oyster, the, as an article of diet, i. 167.

PAGET, Mr, on the nutritive relations of the blood, i. 286.

Pain, the result of direct stimulus to the nerves, ii. 34—the sensation of, 142, 291—influence of, on the action of the heart, 148.

Palate, the, shewn not to be the organ of taste, ii. 301.

Pallas on hybrids between wolf and dog, ii. 400.

Pancreas, function of the, i. 226—its position, 230, 320.

Pancreatic juice, action of the, i. 228.

Papillæ, the, i. 35.

Papin, process of, for extracting gelatine from bones, i. 130.

Parents, caution to, regarding diet of children, i. 61—qualities inherited from, ii. 375—contradictory views on the subject, *ib. et seq.*—those of Mr Buckle examined, 376—permanency of breed, 377 *et seq.*—constancy in transmission of general, and variations in that of individual, peculiarities, 378—diversities of character in families, 379—inheritance of peculiarities, 380—transmission of accidental defects, &c., 381—theory regarding this, 382—transmission of acquired habits, 383—possible counteracting influence of one parent, *ib. et seq.*—transmission of vices, 385—of longevity, 387—of tendency to insanity, 389—on the respective influence of each, 390 *et seq.*—transmission of deformities, 400—the children of great men, 401 *et seq.*—both parents represented in the children, 403—*a priori* deductions, *ib. et seq.*—perturbing causes, 404—atavism, 405—individual potency, 406—influence of race, 407—results of difference of age, vigour, &c., in the parents, 408—general summary on the subject, *ib. et seq.*—marriage in connection with hereditary taint, 410.

Paris, Dr, on pastry, i. 170.

Parisot, M., case of prolonged fasting reported by, i. 14.

Parotid gland, position, &c., of, i. 230, 231, 320—and secretion, 196.

Parry, Captain, on the gluttony of the Esquimaux, i. 185.

Parts, death of the, ii. 445—before actual death, 447.

Pastry, as an article of diet, i. 170.

Patin, Guy, opposition of, to Harvey, i. 310.

Paton, Dr, views of, on the spinal cord, ii. 231 *note.*

Paxton, Dr, voluntary contraction of the iris by, ii. 221.

Payen, analysis of rice by, i. 84—experiments by, on feeding with blood, 118.

Peach, sugar in, i. 139.

Pears, sugar in, i. 139.

Peas, nitrogen in, and their nutritive value, i. 82—gluten in, 128—sugar in, 139.

Pecquet, discovery of, regarding the lymphatics, i. 317.

Pecquet's reservoir, position, &c., of, i. 230, 231, 320.

Peculiarities, hereditary transmission of, ii. 385.

Pepsin, present in the gastric juice, i. 205—effects of heat on, 210.

Peptones, what, i. 206—conversion of the albuminous substances into, 129.

Peptone-albumen, properties of, i. 209.

Perception, distinction between, and sensation, ii. 53—illustration of its relations to the latter, 54—can sensation exist without, 61—importance of the distinction between it and sensation, 69 *et seq.*—origin of, in sense-sensations, 117—the blending of thought with, 343—gradual loss of power of, in sleep, 351.

Pereira, proportion of gluten in grain, &c., from, i. 128—on the influence of fats on formation of bile, 136—on the alleged effects of fish as regards prolificness, 168—tables of dietaries from, 187.

Persoz, experiments of, on feeding geese, i. 93.

Perspiration, moisture thrown off in, i. 34—apparatus for, 418—its influence, &c., 419 *et seq.*

Perspiring, case of voluntary power of, ii. 222.

Peru, clay-eating in, i. 54.

Peruvian Indians, keenness of sense of smell in, ii. 317.

Pesserais, the, a fish-eating race, i. 169.

Peters, Dr, cases of non-coagulable blood reported by, i. 258.

Peyer's glands, i. 228.

Pflüger, E., views of, on the spinal cord, ii. 230—on choice in sleeping children, 241—experiments on decapitated frog as illustrating choice, 245.

Philip, Dr W., views, &c., of, on the spinal cord, ii. 227.

Phosphate of lime, proportion, &c., of, in the body, i. 99—proportion of, in milk, 119.

Phosphates, Liebig on the, i. 82.

Phosphorus, effects of combination with nitrogen on, i. 91—as an alimentary principle, 148.

Phrenology, anatomical difficulties attending, ii. 115.

Physical laws inadequate to explain chemical phenomena, i. 66.

Physiological laws, distinction between, and chemical, i. 69.

Physiologists, error of, regarding "unconscious sensibility," ii. 65.

Physiology, true relations of chemistry to, i. 65—relations of, to psychology, ii. 2.—importance of a knowledge of, 452.

Pichegru, alleged small amount of sleep taken by, ii. 357.

Pig, time in which digested, i. 157.

Pigeon, muscle of, albumen in, i. 123—proportion of the brain to the spinal cord in, ii. 266.

Pigeons, effects of removal of the cerebrum on, ii. 105, 108.

"Pins and Needles," sensation of, how caused, ii. 300.

Pitch of sound, what, ii. 322.

Plants, nutrition in, fallacy regarding it, i. 97—cause of motion of sap in, 339—influence of respiration of, on air, 354—influence of, on the atmosphere, 394—development of heat in, 412, 413—the so-called sleep of, ii. 347—influence of each parent in, 390—the potency of race shewn in, 407—

vegetal life in, common to them and animals, 430.
Plantain, the, productiveness of, i. 171.
Plasma of the blood, the, i. 255.
Plastic or tissue making foods, Liebig's classification of, i. 71.
Plateau on the duration of impressions on the eye, ii. 338.
Plongeau, M., case of prolonged fasting reported by, i. 14.
Pneumogastric nerves, influence of, on the heart, ii. 147.
Pointer, the, why it hunts with its nose to the ground, ii. 317.
Poisons, different actions of, as regards the nerves, ii, 79.
Poisoning, alleged relations of taste in the lower animals to, ii. 304.
Poisonous plants, eaten by animals, i. 62.
Polar regions, alleged greater consumption of heat-producing food in, i. 87.
Polype, influence of light on the, ii. 325.
Pommer, the experiments of, on starvation, i. 11.
Pons Varolii, the, in the brain, ii. 89, 138—ganglion of, 90—relative weight of, in man, 97.
Porcupine, the, its power of abstinence from water, i. 48—quills of, how raised, ii. 39.
Pork, time in which digested, i. 157, 158.
Portal, case of abuse of vinegar from, i. 149.
Portal vein, the, i. 230, 320—injection of sugar, &c., into it, 117—destitute of valves, 307—in the fish, 324.
Posterior nerves, motor function of the, ii. 34—proof of this, 38
Potatoes, gluten in, i. 128.
Potter, Mr, illustration of sensation under chloroform by, ii. 64.
Pressure, the sensation of, ii. 291.
Priestley, the discoveries of, regarding air, i. 352—the experiments of, on combustion and respiration, 390.
Prochaska, views of, on the sensorium, ii. 42—his views as to reflex action, 166, 167, 191—and on the spinal cord, 226.
Property, distinction between, and function, ii. 20 *et seq.*—illustration of the relations of function to, 93.
Protein bodies, the so-called, i. 126.
Proteus, power of abstinence of the, i. 12—its blood discs, 246.
Prout, Dr, case of mutton being poisonous, given by, i. 60—on milk as a standard of food, 118.
Prussic acid, effects of, on the blood, i. 386.
Psychology, relations of, to physiology, ii 2—definition of it, *ib.*
Puff-paste, indigestible, i. 170.
Pulmonary artery, the, i. 230, 320—and veins, i. 293, 294, 295.
—— lobules, the, in man, i. 364.
Pulse, use of, among the Hindoos, i. 85—insufficiency of, eaten alone, *ib.*
—— the, average of, at different ages, i. 330—cause of it, 331.
Pupil of the eye, the, ii. 326.
Putrefaction, action of the gastric juice in arresting, i. 210—as a sign of death, ii. 451.

QUADRUMANA, the blood discs in, i. 247.
Quadrupeds, the blood discs in, i. 244, 246.
Qualitative and quantitative, distinction between, in connection with physiological and chemical laws, i. 69.
Qualities, the hereditary transmission of, ii. 375 *et seq.* *See* Parents.
Quality of sound, what, ii. 312.

RABBITS, experiments on feeding of, i. 179.
Race, influence of, on transmission of qualities, ii. 407.
Réaumur, experiments by, on digestion, i. 204—experiments of, on prolongation of infant life, ii. 442.
Rectum, the, i. 219.
Redford, Mr, case of animal heat after death from, i. 444.
Redi, experiments by, on death of birds by starvation, i. 19.
Reflex action and feeling, illustrations of, ii. 56 *et seq*—action, what, 159—the theory of it, and examination of this, 165 *et seq.*—its error in assuming these actions to be independent of sensations 167—the views of Prochaska, *ib.* *et seq.*—those of Mr Grainger, 172—destitute of anatomical basis, 174—Muller on it, 175—is sensation present in, *ib.*—arguments against theory from sensation in headless animals, 176 *et seq.*—observation of Hall on headless turtle, 180—refutation of his conclusions 181—observations in support of it on division of spinal cord, and criticisms on these, 186 *et seq.*—in the brain, 191 *et seq.*—phenomena of hydrophobia in connection with it, 192 *et seq.*—reference of the movements, &c., of decapitated animals to, 234.
—— fibres, Mr Grainger's supposed, ii. 173—Schroeder van der Kolk's, *ib.*
—— and reflected impressions, the current views regarding, ii. 46.
—— nervous phenomena, the so-called, ii. 165.

Reflex theory, the modern, ii. 42—how the spinal cord is regarded by, 225, 228—spontaneity in decapitated animals denied by its supporters, 239. *See* also Reflex action.

Regnault, experiments by, on respiration, i. 381—on the chemical theory of animal heat, 442.

Remak on the ganglionic system of the heart, i. 333—on its pulsation after death, 336.

Renal veins, the, destitute of valves, i. 307.

Renault, M., experiment by, on horse-flesh, i. 168.

Repair, constancy of, in the organism, i. 4—its inequality with waste the cause of the decline of life, ii. 441.

Reptiles, the blood discs in, i. 246—no valves in the veins of, 307—respiratory organs in, 362—the cerebral development of, ii. 91—relative weight of brain and spinal cord in, 97.

Reproduction, the different forms of, ii. 403—the law of, as one of the laws of life, 429.

Respiration, the air breathed, i. 353—the breathing mechanism, 355—definition of it, *ib. et seq.*—variety of organs for, 361—illustrated from the triton, 363 *et seq.*—in man, 364 *et seq.*—effects of tight-lacing on, 367—alteration of the air in it, 368—necessity of ventilation to, 372—experiments shewing this, 374 *et seq.*—it a vital problem, 377—theories of, examined, 389 *et seq.*—varieties in animals as regards, 397—effects of sleep, 398—of temperature, 400—alleged dependence of animal heat on, 410 *et seq.*—relation between them, 446—the medulla oblongata the centre for the nerves of, ii. 145—function of, continuous, 433—cessation of, as a sign of death, 450.

Respiratory foods, Liebig's classification of, i. 72.

——— nervous phenomena, the so-called, ii. 165.

——— sense, Bain on a, ii. 279.

Rete mucosum, the, i. 35.

Retina, the, proportion of water in, i. 36—as the medium of sight, ii. 325—its position, &c., 326—the formation of images on it, and relations of this to sight, 328 *et seq.*—examination of its structure, and proof that it is not on it images are formed, 330—is it nervous, 331—transparent during life, 332—it insensible to light, 332.

Rhinoceros, the black and white, different food required by, i. 62.

Rice, analysis of, i. 84—its peculiarities as an aliment, 86—albumen in, 124—gluten in, 128—sugar in, 139—productiveness of, 171.

Richardson, Dr, on non-coagulable blood, i. 258—researches on the blood, 260.

Richerand, the blood discs denied by, i. 245.

Rickets, cause of, i. 100.

Rigidity, as a sign of death, ii. 449.

Riolan, the opponent of Harvey, i. 317.

Roasting, action of, on meat, i. 154.

Robin, C., on the blood discs, i. 249.

——— and Verdeil, views of, as to oxidation of carbon in the organism, i. 441.

Rolando, hypothesis of, regarding the nerves, ii. 14 *note*.

Ross, Sir John, on the relations of diet and cold, i. 184.

Rotifera, suspended animation in the, ii. 418.

Roughness, the sensation of, ii. 291.

Rousseau, E., on pulsation of the heart after death, i. 336.

——— J. J., his argument for vegetarianism, i. 173.

Rudbeck, discovery of, regarding the lymphatics, i. 317.

Ruminants, the blood discs in, i. 247.

Runge, views of, on the circulation, i. 311.

Rush, on the effects of race on hereditary transmission, ii. 407.

Russian bath, the, i. 439.

Ruysch, researches of, on coagulation of the blood, i. 257.

Rye, albumen in, i. 124—gluten in, 128—meal, sugar in, 139.

Rye, estimate of weight of food by, i. 186.

SAGARET, M., his experiments on transmitted qualities in plants, ii. 391.

Sago, as an article of diet, i. 171.

St Hilaire on horse-flesh as an article of diet, i. 160—error of, regarding Descartes and Harvey, 310 *note*—on the influence of the parents as to colour, ii. 399—on hybrids between wolf and dog, 400.

St Martin, Alexis, the case of, i. 201.

Salamander, the, respiratory organs in, i. 362—observations on division of spinal cord of, ii. 189.

Salamandra atra, the, i. 248.

Saliva, the, action of, on the food, i. 194—influence of, on the digestion of albuminous substances, 212.

Salivary glands, the, i. 196—may be cut off, 199.

Salmon, difficulty of digesting, i. 166.

Salt, presence and importance of, in the body, i. 103—uses of, in promoting endosmose, 105, 107—its action concentrated and diluted, 142.

Salts, proportion of, in the body, and their importance, i. 99—amount of, in blood, 262.
Salt bath, effects of, on thirst, i. 34.
Samoyedes, gluttony of the, i. 183.
Sanctorius, estimate of weight of food by, i. 186.
Sap, Draker on the motion of, in plants, i. 339.
Sauvages, cases of abstinence from liquids reported by, i. 48.
Savart, M., on the lowest sound audible, ii. 322.
Scarf-skin, process of the waste and repair of the, ii. 360.
Schaffhausen, sale of horse-flesh in, i. 162.
Scherer on inosite, i. 189.
Schiff, Professor, on nervous sensibility, ii. 34 *note*—experiments of, regarding the grey and white matter of the spinal cord, 39 *note*—experiments and views of, regarding the function of the cerebellum, 127—experiments of, on Flourens' vital point, 146—views of, regarding the spinal cord, 231—on the so-called muscular sense, 280—on the so-called tactile corpuscles, 293—on sensation after amputation, 296.
Schirmer, views of, as to organ of taste, ii. 301.
Schmidt, experiments of, with regard to bats, &c., i. 90, 92—on the solvent power of the gastric juice, 314.
Schouw, definition of the bread countries by, i. 172.
Schrœder van der Kolk, his observations on the gastric juice, i. 202, 214—views of, on the necessity of the brain to sensibility, ii. 85 *note*—as to the cerebellum and spinal cord, 127 *note*—on the medulla oblongata, 141—and on the spinal cord, 154 and *note*—the alleged reflex fibres of, 178 *et seq.*—on Pflüger's experiments on decapitated animals, 248—and on those of Szokalski, 257—his mode of making microscopic preparations, 271—views of, on the muscular sense, 282.
Schultz, C. H., his views on the saliva, i. 195.
Schwann on the chemical action of the saliva, i. 196—application of the cell theory to animals by, ii. 426.
Sciences, the, different classes of problems submitted to each, i. 65.
Scientific explanation, distinction between, and common observation, i. 192
Sclerotic membrane of the eye, the, ii. 326.
Scoresby, Rev. Dr, on spectral illusions, ii. 336.
Scorpion, power of abstinence of the, i. 12.
Scurvy, cause of, at sea, i. 148.
Sea anemones, the, digestion in, i. 194 and *note*.
Sea-hare, action of the intestinal juice in the, i. 229—nervous system of the, compared with that of the insect, ii. 95.
Sea-weeds, temperature of, i. 408.
Seal, the, its power of abstinence, i. 12—quantity of blood in, 273—excretory function of the hair in the fœtus, 287.
Seamen, command over sleep possessed by, ii. 354.
Seasons, the, influence of, on animal temperature, i. 435 *et seq.*
Secretion, function of, not continuous, ii. 433.
Seeberg and Ericksen on the so-called olfactory nerves, ii. 309.
Seeing, relations of, to various muscular adjustments, ii. 236. *See* Sight.
Sensation, mystery of, ii. 1—inconsistency of current views regarding, 47—relations of, to consciousness, 49—proposed restriction of the term, 54—illustration of reflex action following on, 58—the active state of sensibility, 59—can it exist without perception, 61—illustrations of it during sleep, 62 *et seq.*—distinction between, and impression and perception, 69—circumstances which determine the kind of, 82—its alleged dependence on the brain, 85—the cerebellum as a seat of, 133—the views of Sir Charles Bell on, 143, 144—proofs of, in headless animals, 177, 239 *et seq.*—blending of thought with perception in connection with, 348.
Sensations, the nerves which convey, ii. 34—the isolation of, by attention, 51—illustration of this, 52—difference between consciousness of and attention to them, 53—illustration of perceived and unperceived, 54—never self-terminating, 55—concurrence of, to form the general consciousness, 65—those which give rise to that of existence, 66—those derived through the muscular sense, 67—the systemic, 68—alleged dependence of, on the cerebrum, 105—sense and systemic, 117—importance of guiding, in the animal mechanism, 162—action of, in exciting other sensations, 208—influence of ideas of, in guiding actions, 216—definition of, 275—how different kinds are determined, 276—specialised by different organs, *ib. et seq.*—classified into systemic and sense, 278—

organic, 279 — surface, 288 — how those of touch are localised, 293—development of this power, 295, 296—subjective, relations of, to dreaming, 367.

Sense, the special organs of, difference between their sensibility and that of the skin, ii. 291.

Senses, the, number of, ii. 273 *et seq.*—objections to the number five, 274—difficulties regarding the number, 275—the different organs specialise the sensations, 276—the muscular, 280—value of, in warning against danger, 303—touch, 292 *et seq.*—taste, 300 *et seq.*—smell, 306 *et seq.*—hearing, 317 *et seq.*—sight, 325 *et seq.*—dormancy of, during sleep, 348—partial inactivity of, during sleep, and relations of this to dreaming, 368 *et seq.*

Sense-consciousness, definition of the, ii. 77.

Sense-sensations, relations of, to intellect, ii. 117—what, 278, 291 *et seq.*—undue importance given to the, 344.

Sensibility, definition of, ii. 8—the property of the nerve centres, 13, 24—what it is, 19—the property of all ganglionic tissue, 24, 40—the current views of, as limited to the brain, 46—inconsistency of those regarding its relations to sensation, 47—the so-called unconscious, 48—general statement of the law of, 55—sensation the active state of, 59—the current classification of, into conscious and unconscious, 72—nervous, analogous to muscular contractility, 73—the common property of all nerve-centres, 82—circumstances which regulate the form of, *ib.*—the current views of the necessity of the brain to it, 85—relations of the cerebrum to, 113—popular subdivision of the forms manifested by the latter, 114—hypothesis proposed regarding this, 115 *et seq.*—proof that it is common to all ganglia, 179—analogy between, and muscular contractility, 195—the development of, in connection with complexity of the organism, 277—activity of, during sleep, 349.

Sensori-motor actions, the, ii. 193.

Sensorium, position of the, ii. 40.

—— commune of the physiologists, the, ii. 40—current views regarding, 84—of Prochaska, the, 167 *et seq.*

Sensory and motor, the distinction of nerves into, ii. 21, 25 *et seq.*—definition of, 26—examination of the question, 31 *et seq.*—researches on, and their results, 34.

Sensory function, the anterior nerves proved to possess, ii. 38.

Sepoy, the, compared with the Gaucho, i. 121.

Serum of the blood, the, i. 256.

Servetus, discovery of, regarding the circulation, i. 300.

Sex, relations of, to animal heat, i. 481.

Sheep, the, plants rejected by, i. 62—experiments on feeding on turnips, 83—and on cross-breeding, ii. 407.

Sheeps' feet bones, fat in, i. 135.

Siam, clay-eating in, i. 54.

Siamese twins, differences of character between the, ii. 379.

Siberia, clay-eating in, i. 54.

Sighing, what, i. 399.

Sight, proofs of retention of, after removal of cerebrum, ii. 108—the sense of, 325—the eye as the organ of, *ib.*—description of it, 326—various theories of, 327—error of the popular conception, 328—the image on the retina not transmitted as such to the brain, *ib.* *et seq.*—where is the image formed? 329—shewn to be not on the retina, 332 *et seq.*—Professor Draper's theory of it, 333 *et seq.*—its subjective character, 334—spectral illusions, 335—duration of impressions on the eye, 338—single vision with two eyes, 339 *et seq.*—cases of double, 340—why objects are not seen inverted, 341.

Simon, John, on the blood, i. 116—eras of life as defined by, ii. 436, 437.

Singing, adjustment of the muscles in, how directed, ii. 286.

Skin, the scarf and the true, i. 35—respiration by the, 356—the movement of the muscular fibres of, ii. 39—views of Schiff, &c., on, in regard to the muscular sense, 282—universal sensativity of, 289—different degrees of this, 290—its different forms, 291—difference between it and the special organs of sense, *ib.*—coldness, &c., of the, as a sign of death, 449.

—— nerves, what, ii. 39—unaffected by the Woorara poison, 79.

Sleep, effects of extreme thirst on, i. 49—effects of, on respiration, 398—illustrations of sensation during, ii. 62 *et seq.*—action of the spinal cord during, 267—what is it? 346—difficulty of defining it, 347, 349—its phenomena, *ib.*—no analogy between it and death, and no antagonism between it and life, 348—held by some to be the normal condition, *ib.*—Sir H. Holland's view of it, 349—a constant oscillation between sleeping and waking, *ib.*—gradual transition from waking to, 350 *et seq.*—the impulse to it irresistible, 352—cases illustrating this, *ib.* *et seq.*—effects of cold in inducing, *ib.* *et seq.*,—ex-

amples of command over, 354—heat as a predisposing cause, *ib.*—tendency to, in church, 355—prevented by cold, cerebral excitement, &c., 356—its duration, *ib.*—amount of, at different ages, 358—does nutrition go on chiefly during it, *ib.*—its restorative influence, 361—produced by temporary congestion, 363—how prevented by over fatigue, 364—dreams, 365.
Sleeplessness, why attendant on starvation, i. 22.
Slug, loss of weight by the, during want of food, i. 19—the blood-vessels of, 314.
Small intestines, what, i. 219.
Smell, its relations to taste, ii. 304—the sense of, 306—the organs of, 307—the so-called olfactory nerves not essential to, 310 *et seq.*—only affected by substances in gaseous state, 314—Graham on it, *ib.*—varieties of sensibility, taste, &c., in regard to it, 315—its uses, 316—its keenness in certain circumstances, *ib.*
Smith, Dr F., observations on Alexis St Martin, by, i. 202.
Smoking, influence of, on digestion, i. 236.
Smoothness, the sensation of, ii. 271.
Snake, effects of division of spinal cord on a, ii. 186 *et seq.*
Snakes, power of abstinence of, i. 12.
Snow, thirst aggravated by, i. 110.
Soil, organic principles necessary in, to plants, i. 97.
Solander, Dr, anecdote of, ii. 352 *et seq.*
Solaneæ, the, rejected by the goat, &c., i. 62.
Soldier, dietary of the, i. 188.
Soldiers, command over sleep possessed by, ii. 354.
Solly, Mr, report by, of a case of the cerebellum wanting, ii. 129.
Somnambulists, peculiarity regarding the dreams of, ii. 373.
Soul, the, animates the whole body, ii. 224, 226.
Soul-sensations of Prochaska, the, ii. 42.
Sound, the sensations of, how heard, ii. 317—intensity, pitch, and quality of, 322—the highest and lowest audible, *ib.* *See* Hearing.
Sounds, effects of habitual, as regards sensation and consciousness, ii. 60—subjective, how caused, 324—appreciation of the direction of, *ib.*
Soup, prepared by slow boiling, i. 155.
Soups, nutritive powers of, i. 129.
South America, clay-eating in, i. 54.
Spallanzani, the experiments of, on digestion, i. 204—experiments by, on the circulation, 342—researches of, on respiration, 352, 392—observations of, on the tadpole, 361—on temperature and respiration, 400.
Spawning, inferiority of fish after, i. 166.
Spectral illusions, theory of, ii. 335.
Spencer, Herbert, definition of life by, ii. 425.
Spices, thirst increased by, i. 33.
Spider, power of abstinence of the, i. 12.
Spiders, respiratory organs in, i. 362.
Spiess, views of, on the muscular sense, ii. 282.
Spinal cord, the, a seat of sensibility, ii. 23—composition, &c., of the, 26 *et seq.*—distribution of the nerve-roots on, 33—alleged difference of function between the two halves of, 39 *note*—positions of the gray and white matter in it, 43—relative weight of, in different animals, 97—supposed dependence of, on the cerebrum, 119—effects of division of in different ways, 125—relations of the medulla oblongata to, 133—its structure, 151—old hypotheses regarding it, 152—anterior and posterior aspects of nerves and cord, 153—transverse section of it, 155, 156—its nerve cells, 156—views regarding these, 157—it shewn to be a series of ganglionic centres, 158—Marshall Hall's observations on division of, 186 *et seq.*—criticism on his conclusions, 187 *et seq.*—Grainger's experiments, &c., 189—as a sensational and volitional centre, 225—views of earlier writers on it, 226 *et seq.*—error of the reflex theory regarding it, 229—deductive evidence in favour of its sensational character, 231—its similarity of structure to the brain, 232—and to the medulla oblongata, 233—similarity of property to be inferred from this, *ib. et seq.*—inductive evidence that it is a seat of sensation, &c., 237—spontaneity and choice in decapitated animals, 239 *et seq.*—experiments shewing results of its division, 249—division originates two nervous centres, and errors arising from disregarding this, 251 *et seq.*—evidence against its sensibility, 259—examination of the evidence from patients with diseased spines, 260 *et seq.*—its proportion to the brain in different animals, 265—what part it plays, 266—probable character of its sensibility, 267—microscopic preparations of it, 270 *et seq.*
——— veins, the, destitute of valves, i. 307.
Spine, disease of the, examination of evidence from attendant phenomena against the sensibility of the spinal cord, ii. 260.

Spleen, the, its position, i. 230, 320.
Spontaneity, proofs of, after removal of the brain, ii. 184 *et seq.*—how manifested by animals, 237—how originated in them, 238—as a means of recognising sensation and volition, 239—indications of it in decapitated animals, *ib. et seq.*
Stagbeetle, power of abstinence of the, i. 12.
Stahl, the doctrine of, regarding the soul, ii. 224—definition of life by, 425.
Stallion, influence of, in horse-breeding, ii. 391 *et seq.*
Stapes or stirrup, the, in the ear, ii. 321.
Starch, how classed by Liebig, i. 71—as an alimentary principle, 138—intestinal action on, 219—converted into sugar by the saliva, 196 *et seq.*—action of the pancreatic juice on, 226, 227.
Starvation, the last gradation of hunger, i. 3—its effects on the blood, 8—periods when death ensues from, 12—the aspects of, 21—its effects on the nervous matter, *ib.*—sleeplessness attendant on it, 22—its agonies, 23—case of voluntary, 25.
State, the, the family not the type of, i. 66.
Steam-engine, the comparison of the body to a, i. 7.
Steinbuck, the, its power of subsisting without water, i. 48.
Stethoscope, value of, for determining the reality of death, ii. 451.
Stevelly, Professor, on spectral illusions, ii. 335 *et seq.*
Stich on the taste of gases, ii. 302.
Stilling, his mode of microscopic preparation, ii. 270.
Stimulation, carbonic acid the agent of, in the blood, i. 281 *et seq.*
Stomach, the, effects of prolonged abstinence on, i. 24—emptiness of, not the cause of hunger, 28, 29—the food in it still outside the organism, 31—the seat of the sensation of hunger, *ib.*—its state in the fasting animal, 32—is sugar injurious to? 140, 141—process of digestion in, 201 *et seq.*—its mechanical action, 202—its chemical, 204—amount of digestion in, 215—action of the gastric juice on it, 216—digestion in, arrested by bile, 223—its position, &c., 230, 231, 320.
Stone, the, arrested by vegetable diet, i. 175.
Strawberries, sometimes pernicious, i. 60.
Structure, identity of, implies identity of properties, ii. 21—relation of, to property in nerves, 82.
Subjective sounds, what, ii. 324.
Sublingual gland and secretion, the, i. 196—position, &c., of, 230, 231, 320.
Submaxillary gland and secretion, the, i. 196—position, &c., of, 230, 231, 320.
Sudoriparous or sweat glands, *see* Sweat.
Sue, J. J., views of, on the spinal cord, ii. 226.
Suffocation, effects of, on coagulation of the blood, i. 259—case of, by charcoal fumes, 347—by drowning, 348—identity of death in these, 349—causes of, and phenomena attending it, 383—deaths from, 387.
Sugar, case in which pernicious, i. 60—how classed by Liebig, 71—action of yeast on, 113—experiments on injection of, 117—proportion of, in milk, 119—as an alimentary principle, 138 *et seq.*—starch converted by the saliva into, 196 *et seq.*—presence of, in blood, 263.
Sugars, necessity of, in food, i. 120—action of the gastric juice on, 206—results of stomachic action on, 215.
Suicide by starvation, case of, i. 25—by charcoal fumes, 347—by drowning, and identity of these two, 348.
Sulphur as an alimentary principle, i. 148.
Sunlight, effects of, on respiration, i. 371, 398.
Surface, respiration by the, i. 356.
Surface-sensations, what, ii. 288—one of the classes of systemic sensations, 279.
Suspended animation, illustrations of, ii. 413.
Swammerdamm, the blood discs discovered by, i. 244—experiment of, on tying the aorta, 279—reception of Harvey's doctrine by, 310.
Sweat glands, action of the, and representation of one, i. 35, 418. *See* also Perspiration.
Sweets, effects of, on flavour of wines, ii. 305.
Sweetbread, albumen in, i. 123—fibrine in, *ib.*—as an article of diet, and its composition, 159. *See* also Pancreas.
Sweet flag, after-taste from the, ii. 305.
Swimming, shewn to be not a reflex action, ii. 257.
Syntonin, what, i 124, 257.
Systemic consciousness, what, ii. 74 *et seq.*—importance of it, 344
——— sensations, the, ii. 68, 278 *et seq.*—that of thirst, i. 46—relations of, to emotion, ii. 117—their influence in dreams, 372.
Systole of the heart, what, i. 329.
Szokalski, experiments on brainless animals by, and his deductions from them, ii. 257.

Tactile corpuscles, the so-called, ii. 292—not tactile organs, 293.

Tadpole, the, development of the blood in, i. 278 *note*—circulation, &c., in its tail, 361.

Tannin, after-taste of, ii. 305.

Taste, the medulla the centre of the nerves of, ii. 139—the sense of, 300—the tongue as its organ, *ib.*—other views held as to its organ, 301—solution necessary to it, *ib.*—excited by mechanical and electrical stimuli, 302—its use in warning against deleterious substances, 303—relations of smell to, 304—after-tastes, 305—variety necessary in relation to, *ib.*—excited through the blood, 306.

Tea as an article of diet, i. 175, 176—effects of, on respiration, 371.

Teeth, the, is sugar injurious to, i. 140, 141—action of, on the food, 194—importance of it, 200.

Temperament, origin of differences of, i. 69.

Temperature, influence of, on the demand for food, i. 6—alleged influence of, as to character of dietary, 87—effects of, in Messrs Lawes and Gilbert's experiments, 88 *et seq.*—influence of, on the circulation, 342—on respiration, 400—on perspiration, 419—and on the breathing of the frog, &c., 453—the sensation of, ii. 142—this received through the skin, 289—effect of destruction of epidermis on sensibility to, 298—difference in right and left hand as regards, *ib.*—relations of, to vision, 333 *et seq.*—dependence of chemical action on, 419.

Tepid drinks, influence of, in allaying thirst, i. 46.

Tetanus, effects of, on animal heat, i. 445.

Theine, different effects of, in different conditions, i. 110.

Thermo-electrometer, the, i. 409.

Thigh, tactile sensibility of the, ii. 297.

Thinking, *see* Thought.

Thirst, the sensation of, i. 33—its primary cause, 34—rapidity of death from, 37—suffering from it, 38—the Black Hole of Calcutta as an illustration of, *ib. et seq.*—tepid drinks or ice best for allaying, 46—the effects of, 49—kills by an inflammatory fever, *ib.*—aggravated by snow and allayed by ice, 110.

Thoracic duct, the, position, &c., of, i. 230, 231, 320.

Thought, mystery of, ii. 1—the blending of, with perception, 343.

Thoughts, how they are controlled, ii. 217.

Thought-consciousness, definition of the, ii. 77.

Ticking, the sensation of, due to the nerve filaments of the skin, ii. 289.

Tight-lacing, effects of, on respiration, i. 367.

Tissue, what it is, i. 3—various kinds of, 4.

Tissues, the, non-nitrogenous substances essential to, i. 75—how nourished by the blood, 285—their waste and repair continuous, ii. 360, 361.

Tissue-making foods, Liebig's, i. 71.

Toads, respiratory organs in, i. 362—the winter life of, 452—suspended animation in, ii. 413.

Tobacco, influence of, on digestion, i. 236.

Todd and Bowman on the nervous system of the heart, i. 333—on the motor and sensory nerves, ii. 32—on sensation and perception, 53—on the brain, 84—on the so-called olfactory nerves, 308.

Tones, the effect of, ii. 323.

Tongue, the, comparative sensitiveness of, ii. 290—more sensitive as to touch than the hand, 292—tactile sensibility of its tip, 297—as the organ of taste, 300, 301.

Tortoise, effects of removal of the brain on a, ii. 184.

Total abstinence, errors of the advocates of, i. 142 *et seq.*

Touch, the sensation of, ii. 142, 291—the sense of, 292 *et seq.*—localising of its sensations, 293 *et seq.*—development of this faculty, 295—sensibility of various parts of body, 297—effect of destruction of epidermis on it, 298—the process of localisation, 299.

Towns, proposals to purify the air of, by trees, i. 394, 395.

Trachea, the, i. 295—its office in respiration, 364.

Transfusion of blood, the, i. 274 *et seq.*—when applicable, 277.

Trees, proposals to plant in cities to purify the air, i. 394.

Treviranus, definition of life by, ii. 424.

Tripe as an article of diet, i, 159.

Triton, the, its blood discs, i. 246—circulation after removal of the heart in, 326—phenomenon on opening one, *ib. note*—respiratory organs in, 362—the lung of, 363—decapitated, experiments as to sensation in, ii. 239, 243 *et seq.*—experiments on division of spinal cord of, 249 *et seq.*—proportion of the spinal cord to the brain in, 265, 266.

Trituration, process of, in digestion, i. 193.

Tropical vegetables, the, i. 171.
Turkey, time in which digested, i. 159.
Turnips, experiments on feeding sheep on, i. 83—gluten in, 128.
Turtle, Marshall Hall's observations on a headless, ii. 180 *et seq.*
Twins, differences of character between, ii. 379.
Tympanum of the ear, the, ii. 319—its integrity not essential to hearing, *ib.*—part it plays, 320.

UNCONSCIOUS sensibility, the so-called, ii. 47—error of physiologists regarding, 65, 72.
Unfelt sensations, so called, ii. 47.
Uterine veins, the, destitute of valves, i. 307.

VALENTIN, estimate by, of quantity of food required, i. 180—on the relations of sensitiveness to the abundance of nerve filaments, ii. 290—views of, as to the organ of taste, 301—on the waste of the tissues during hybernation, 361.
Van Oven, on the causes of life's decline, ii. 440.
Vapour, exhalation of, in respiration, i. 368.
Veal, muscle of, albumen in, i. 123—and fibrine, *ib.*—why less digestible than beef, 152—time in which digested, 158.
Vegetables, albumen and fibrine in, i. 124—as articles of diet, 171—intestinal digestion of, 219.
Vegetable albumen, how classified by Liebig, i. 71—Mulder's discovery regarding, 113.
—— life, influence of, on the atmosphere, i. 394.
—— matters, results of stomachic action on, i. 215.
Vegetal life, what, ii. 430—common to plants and animals, *ib.*—its organs not invariably single, 431—the so-called law of continuity as characterising, 432.
Vegetative organs, the, said to be given by the female, ii. 391, 398.
Vegetarianism, arguments against, i 173.
Veins, the, injection of food into, i. 80—and of water, 84—error of the ancients regarding, 297, 298—discovery of their valves, 305—these not always present, 307.
Venæ cavæ, the, i. 280, 294, 320—destitute of valves, 307—in the fish, 324.
Venison steak, time in which digested, i. 157.
Venous, when the blood becomes, i. 242.
Venous blood, distinction between, and arterial, i. 265—sometimes scarlet, 269—views of the ancients regarding, 298.
Ventilation, necessity of, i. 372 *et seq.*—increased attention to, in England, 373—experiments shewing its importance, 374.
Ventricles, the, i. 293, 294—in the fish, 324.
Verdeil on the effect of vegetarianism on the blood, i. 175.
Vertebral column, relations of the cerebellum to the, ii. 127.
Vertebrate animals, progression and variety of cerebral development in, ii. 90 *et seq.*
Vesalius, discovery of, regarding the heart, i. 299—Titian not the illustrator of his work, *ib. note*—case of pulsation after death experienced by, 336.
Vices, hereditary transmission of, ii. 385.
Vicq-d'-Azyr on the mule, ii. 390.
Vienna, consumption of horse-flesh in, i. 168.
Vieussens, seat assigned the sensorium by, ii. 41.
Villedieu, the Abbé de, case of, i. 59.
Vinegar, use of, i. 148—its employment to keep down fatness, 149.
Virchow on the fibrine of the lymph, i. 315—on nervous activity in relation to nutrition, ii. 362.
Viscera, the sensations derived through the, ii. 345.
Vision, relations of animal heat to, i. 427—why single with two eyes, ii. 339—cases of double, and explanation of them, 340. *See* also Sight.
Vital activity, effects of, on respiration, i. 398.
—— force or forces, the so-called, ii. 415 *et seq.*—fail to control chemical affinities, 417.
—— mechanism, the, ii. 160 *et seq.*
—— principle, the so-called, i. 216—in relation to animal heat, 407—the old hypothesis of a, 414.
—— point, the supposed, ii. 145.
—— processes, true relations of chemistry to, i. 64.
—— unity, what, and its influence, ii. 421.
Vitality, importance of, in the mechanical adjustments of the organism, ii. 161—intimate connection of chemical action with, 418.
Vitiated air, experiments shewing effects of, i. 374 *et seq.*
Vitreous humour of the eye, the, ii. 327.
Vivisection, on the alleged cruelty of, ii. 36 *note.*
Volatile oils, the, as condiments, i. 137.
Volition, alleged dependence of, on

the cerebrum, ii. 105—proof to the contrary, 108—the cerebellum as a seat of, 133—relations of, to the actions, 197 *et seq.*—Müller's views on, 208—and Mr Bain's, 209 *et seq.*—the exhibitions of, in decapitated animals, 239 *et seq.*

Volkmann, experiments of, on nervous sensibility, ii. 34 *note*—experiments by, on brainless animals, 258.

Voluntary actions, distinction between, and involuntary, ii. 197—the popular view, *ib.*—defects of this, 199 *et seq.*—nature of control, 200 *et seq.*—influence of association of ideas, 204 *et seq.*—no essential difference between them and involuntary, 205—Müller on them, 206 *et seq.*—Mr Bain's views, 209 *et seq*—the link of feeling, 210 *et seq.*—influence of guiding ideas, 215 *et seq.*—as guided first by sensations, then by ideas of these, 216—Mr Bain's definition of, 219—the control of involuntary, 221—final conclusion regarding, 223.

——— movement, proofs of, after removal of the brain, ii. 184—why removal of the medulla oblongata arrests it, 185.

——— nervous phenomena, the so-called, ii. 165.

——— power, gradual failure of, in sleep, ii. 350.

Von Bibra on the phosphate of lime in the bones, i. 100 *note*—on the action of alcohol, &c., 177.

Vulpian, M., observations of, on the tadpole's tail, i. 361.

WAKING, no antagonism between sleep and, ii. 348.

Walking, shewn to be not a reflex action, ii. 257.

Warm climates, smaller demand for food in, i. 182.

Warm-blooded animals, influence of temperature on feeding of, i. 6—comparison between, and cold-blooded, as to starvation, 10—effect of tying the aorta in, 279—their respiration arrested by carbonic acid, 378 *et seq.*—effects of temperature on respiration of, 401—distinction between, and cold-blooded, 413 *et seq.*—cooling apparatus of, 417—respiration and animal heat among, 446.

Warmth, the sensation of, ii. 291.

Waste, constancy of, in the organism, i. 4—amount of, fatal to life, 9—production of animal heat as a source of, 17—breathing as a source of, 18—water in the body the vehicle of, 87—blood the vehicle for removal of, 116—diminution of it, by tea, coffee, &c., 176—quantity of, conveyed by the blood, 241—predominance of, over repair, the cause of the decline of life, ii. 441.

Water, prolongation of life during starvation by, i. 19—thirst allayed by injection of, 34—loss of, by breathing, perspiration, &c., 35, 36—large proportion of, in the body, 36—its physiological importance, 37—rapidity of death from abstinence from, *ib.*—requirements of different animals for, 47—cases of abstinence from, 48—essential to muscle, 77—Liebig on its uses, &c., in the organism, *ib.*—proportion of the body it forms, 99—indispensable to the organism, and its uses there, 102—effects of different conditions on, 109—proportion of, in milk, 119—effect of, on the gastric juice, 210—the secretion of bile increased by, 222—proportion of, in blood, 262.

——— birds, the flesh of, as an article of diet, i. 158.

——— newt, the lungs and process of respiration in, i. 363—winter life of, 453. *See* also Triton.

Watson, Dr, on the stupefying effects of cold, i. 439.

——— Dr Forbes, on the diet of the Hindoos, i. 84.

Weber, estimate of the quantity of the blood by, i. 272—experiments of, on the relations of the pneumogastric nerves and the heart, ii. 147—power of arresting the beating of the heart possessed by, 222—measurement by, of the tactile sensibility of different parts, 296—his experiments on the effect of loss of the epidermis on tactile sensibility, 298.

Wheat, nitrogen in, and its nutritive value, i. 82—albumen in, 124—gluten in, 127, 128—productiveness of, compared with rice, &c., 171—Egyptian or Mummy, an illustration of suspended animation, ii. 413.

——— bread and flour, sugar in, i. 138.

White of egg, the, as an alimentary principle, i. 122 *et seq.*

White fish, digestibility of, i. 166.

——— matter of nervous system, the, ii. 13—distribution and supposed function of, in the brain, 98.

——— mustard, oil in, i. 134.

Whytt, R., views of, on the sensorium, ii. 41—on the so-called unconscious sensations, 61—on unperceived and perceived sensations, 66, 67—views of, on the spinal cord, 226.

Wild animals, tamed by thirst, i. 38.

Will, the relations of, to the actions, ii. 197 *et seq.*—Mr Bain's views on the, 209 *et seq.*

Willan, Dr, case of voluntary starvation given by, i. 19.
Willbrand, views of, on the circulation, i. 311.
Willis, Dr R., experiment of, on the nerves, ii. 16—seat assigned the sensorium by, 41—on Combette's case, 132 *note*.
Wilson, Erasmus, on the sweat glands, i. 418.
—— Professor George, on colour blindness, ii. 341, 342—on the Manx cat, 395.
Wind, influence of, on temperature, i. 438.
Windpipe, the, i. 294, 295.
Wine, effects of, on respiration, i. 371.
Wines, thirst increased by, i. 33—how classed by Liebig, 71—as an article of diet, 175—effect of various substances on flavour of, ii. 305.
Winking, an illustration of reflex action, ii. 57—is it a voluntary or involuntary action, 199.
Winslow, the anatomist, a case of apparent death, ii. 447.
Wistinghausen on the action of the bile on fats, i. 226.
Wolf, the, hybrids between, and the dog, ii. 394, 399, 400 *et seq*.
Wolff, on the nutritive relations of the blood, i. 236.
Women, respiration in, compared with men, i. 370.
Woorara poison, action of the, on the nerves, ii. 79.
Würtemberg, sale of horse flesh at, i. 162.

Yakuti, gluttony of the, i. 185.
Yeast, importance of form to the action of, i. 113.

Zittau, horse-flesh sold in, i. 163.
Zoophytes, temperature of, i. 403.

THE END.

PRINTED BY WILLIAM BLACKWOOD AND SONS, EDINBURGH.

25

WIDENER LIBRARY
Harvard College, Cambridge, MA 02138: (617) 495-2413

If the item is recalled, the borrower will be notified of the need for an earlier return. (Non-receipt of overdue notices does not exempt the borrower from overdue fines.)

WIDENER SEP 1 1 2006 BOOK DUE WIDENER NOV 0 8 2006 CANCELLED	

Thank you for helping us to preserve our collection!

3 2044 020 486 072